

# Democracy under High Inequality: Capacity, Spending, and Participation

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## Abstract

Contrary to the view that inequality reduces turnout, political participation among low income voters is higher in democracies with high levels of inequality and intermediate levels of state capacity. We address this puzzle by analyzing the link between political mobilization and spending decisions at different levels of inequality and state capacity. Under high inequality and low levels of capacity, parties find it optimal to mobilize low income voters via targeted goods. But as inequality decreases and capacity increases, targeted mobilization becomes less effective a strategy for voters' mobilization. To evaluate the implications of this argument we exploit a quasi-natural experiment, the anti-corruption audits by the Brazilian federal government on its municipalities. We show that an exogenous increase in monitoring effort by the state breaks the existing equilibrium around high electoral participation of the poor and leads to a reduction in turnout rates, a reduction in the provision of targeted goods at the local level, and a decline in the likelihood of re-election by incumbents.

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# 1 Democracy under High Inequality

Inequality is known to bias citizens' political influence, undermining the idea of citizens as political equals (Dahl, 1991; Przeworski, 2010). A widely shared understanding in comparative political behavior is that inequality reduces, in relative terms, the political participation by low income voters<sup>1</sup>. And yet, in younger, less developed, and very unequal democracies poor voters often seem as willing (if not more) to engage in politics as their counterparts in rich democracies (Krishna, 2008; Stokes et al., 2013; Kasara and Suryanarayan, 2014). As a matter of fact, the relationship between inequality and electoral turnout in the developing world reverses the patterns observed in wealthier democracies: higher levels of inequality are associated with high electoral participation, rather than low, in places like Mexico, Brazil, or Peru even after one accounts for obvious institutional factors such as compulsory voting laws.

How does democracy work under very high economic inequality to feature at once high levels of economic inequality and high levels of formal political equality? To address this puzzle we reason from the premise that turnout levels reflect primarily parties' efforts to mobilize voters, especially those situated in the lower half of the income distribution. The explanation of turnout requires not only an account of voters' incentives to engage in elections but also of parties' choices about (1) whom to target and (2) how to target.

We argue that mobilizing low income voters through targeted spending is an optimal strategy in contexts of high inequality and low capacity, that is in contexts where weak administrative capacity allow incumbents to manipulate budgets for political benefit. In such an environment, parties see an electoral advantage in seeking the support of a large pool of low income citizens. Accordingly, they prioritize policies that effectively work as targeted efforts towards low income voters based on a conditional exchange for political

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<sup>1</sup>The effect occurs through a variety of channels. Inequality limits the resources poorer individuals need to engage in politics (Verba et al., 1995; Solt, 2008; Gallego, 2010; Mahler, 2008); alters the structure of informational networks (Bond et al., 2012; Abrams, Iversen and Soskice, 2011); privileges wealthier voters via campaign contributions (Campante, 2011; Przeworski, 2015) or political representation (Bartels, 2009; Gilens, 2012; Gingerich, 2013); undermines pro-redistributive coalitions (Franzese and Hays, 2008); and alters the incentives of political parties to target low income voters (Anduiza Perea, 1999; Anderson and Beramendi, 2012; Gallego, 2014).

support. It is in these settings where we expect to observe a higher involvement by low income citizens and, as a result, higher levels of turnout. But as inequality falls and capacity increases, the incentives to rely on targeted efforts towards the poor, including clientelism, decline.<sup>2</sup>

We make three contributions. First, theoretically, we take the choice of strategic targeting of spending back to first principles and focus on its determinants without assuming a given ideological space *ex ante*. Previous contributions to the literature on clientelism and turnout buying (Nichter, 2008; Hidalgo and Nichter, 2016; Larreguy, Marshall and Querubin, forthcoming) assume a policy space where the ideological distance between parties and voters is part of the latter's utility function. Assuming a policy space implies the existence of programmatic politics, that is a style of political competition where targeting low income voters explicitly is far less frequent, which is part of what we aim to explain.<sup>3</sup>

Our second contribution lies in how we approach the analysis of strategic spending decisions. While a large literature has focused on the connection between development and modes of political competition (Kirchheimer, 1965; Kitschelt and Wilkinson, 2007; Stokes et al., 2013), the link between inequality, capacity, and politicians' incentives to spend strategically is far less understood. Our model studies how inequality and capacity, jointly, determine the use of local targeted goods as a mobilization strategy. In particular, we study how, given inequality, the institutional capacity of the state to monitor its elites shapes incentives to resort to targeted mobilization strategies. Low levels of monitoring capacity reinforce and facilitate elites' strategies to provide local targeted goods for political purposes and contribute to the political engagement of low income voters. In examining this connection, our argument complements recent contributions on the relationship between redistributive capacity and turnout inequality (Kasara and Suryanarayan, 2014).

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<sup>2</sup>These *targeted* efforts can take many forms, including clientelistic, interpersonal exchanges or the manipulation of budget allocations to what otherwise are labelled as targeted goods. In this sense, we see clientelism as a specific type of a more general phenomenon. For a more detailed analysis of different forms of non-programmatic politics consistent with the approach in this paper see Stokes et al. (2013).

<sup>3</sup>Our focus on the link between structural conditions and party strategies also enhances our understanding of elites' mobilization strategies (Magaloni, Diaz-Cayeros and Estévez, 2007; Diaz-Cayeros, Estevez and Magaloni, 2016; De La O, 2013) and contributes to the study of the politics of turnout buying (Nichter, 2008; Hidalgo and Nichter, 2016; Larreguy, Marshall and Querubin, forthcoming).

More generally, a better understanding of the connection between economic inequality, party strategies and capacity helps illuminate when bad equilibria (with high inequality and low state capacity) are likely to emerge and persist (Robinson and Verdier, 2013). As such, we offer a genuinely political mechanism behind the persistence of *perverse accountability* (Stokes, 2005), bad development equilibria and the self-reproduction of inequality, both economic and political. In high inequality contexts, elites magnify their political influence by conditioning the political voice of the poor as opposed to excluding them altogether from the political process, ultimately reproducing and anchoring economic inequality over time.

Our third contribution speaks to a central issue in the political economy of development: the impact of interventions designed to undermine the sort of persistent political underdevelopment described above. Empirically, we analyze the relationship between inequality, capacity, and participation from two perspectives: a characterization of the status quo equilibrium and a thorough study of the consequences a truly exogenous capacity shock on pre-existing political relationships. To this end, we leverage on micro-level information in a case, Brazil, where the status quo equilibrium of high economic inequality and low capacity is prevalent.

Exploiting information about turnout, economic inequality, and spending in Brazilian municipalities we show how the status quo patterns reflect our model's implications: (1) in contexts of high inequality and low capacity, incumbents do resort to higher levels of targeted spending; (2) these decisions, in turn, translate into higher levels of turnout. To explore the use of targeted goods we focus on primary education spending and the patterns of public employment at the local level. These are policy realms where local authorities have large amounts of discretion to alter the local labor force (via wages or employment (Calvo and Murillo, 2004)), privilege schools's resources by area (thus benefitting different pools of voters), and even manipulate access through the matching of facilities to specific subareas within the municipality (de Oliveira and Adrião, 2007). This form of inefficient redistribution Robinson and Verdier (2013), we argue, is part of a political strategy that yields higher levels of turnout.

In turn, the randomized anti-corruption audits of Brazilian municipalities, introduced in the early 2000s by the federal government, provide an opportunity to analyze how an exogenous capacity shock alters pre-existing politico-economic equilibria. We model the impact of these reforms as an exogenous change in the state's effort to monitor elites. The audits were directly designed as an effort from the federal government to curb down corruption and the misuse of federal funds by Brazilian municipalities (on average 70% of the local budget rests on federal transfers). By exploiting the randomized nature of the audits and their timing (before or after the 2004 election), we show that the exposure to an audit before the election depresses turnout rates (especially in rural contexts with pre-eminent low levels of education), leads to a reduction of local targeted goods in the subsequent period, and, by implication, reduces the incumbent's likelihood of re-election. No such process is observable in the municipalities that were not audited before 2004.

Our analysis provides a detailed case study of a major reform effort to undermine corruption and budgetary manipulation, and contributes to a growing literature on the impact of randomized audits as an instrument for political and economic development (Olken, 2007; Ferraz and Finan, 2008). Analytically, we predict that an increase in such efforts reduce elites' ability to politically manipulate targeted goods. And rather counterintuitively, we show that, under high inequality, exogenous increases in the state's monitoring capacity lead to a reduction of budgetary efforts to lure low income voters. Our results illustrate the complex effects of interventions aimed to sever this kind of strategy. Using primary education policy at the local level as a relevant case study, we show that exogenous attempts to control political machines may actually reduce the welfare of low income citizens in the short and medium run, especially in contexts where weak fiscal and state capacity prevents new incumbents to replace targeted benefits with programmatic public good provision (Nathan, 2016).

## 2 Theory: Inequality, Capacity, and Spending

Our central argument is as follows: observable patterns of turnout, in particular that of low income voters, reflect strategic decisions by governments about how to spend. These decisions shape electoral participation. The main analytical contribution of this paper is to theorize this choice as jointly determined by inequality and capacity. Unpacking this relationship is crucial to understand how economic inequality translates into political inequality.

In designing their budgets, parties prioritize the expected electoral returns of their spending priorities. Their strategic choice focuses on the scope of targeting to different income groups. At one end of the continuum, effort in public goods such as national defense, strictly defined as non-rivalrous and non-excludable, implies no targeting; at the other, the most personalistic version of clientelistic exchanges of political support for transfers of consumables or money to individuals implies full targeting (Kitschelt and Wilkinson, 2007). In between, tax funded, loosely defined, public goods vary on their expected distributive incidence of the policy (Stigler, 1970). Some policies such as higher education benefit disproportionately the top end of the income distribution (Ansell, 2010); others, such as primary education or universal primary health care, are relatively more redistributive.

In this section we model the manipulation of policy and spending choices as a mobilization device to attract low income voters. Parties have limited resources and must choose how much to devote to local targeted goods for low income voters ( $b_P$ ), how much to targeted goods for high income voters ( $b_R$ ), and how much to general public goods ( $g$ ). Within this framework we study how inequality shapes the conditions under which elites resort to the targeted mobilization of low income voters.

### 2.1 Model

We assume politics to be an activity initiated by elites at all ends of the ideological spectrum. In other words, we assume a sequential set-up in which the elites (the rich) move

first and the low-income voters move afterwards and where the rich have perfect information<sup>4</sup>. Accordingly, mobilization is a choice by different groups of rich citizens. The fundamental problem for any party is to maximize the utility of their base such that they attract the support of low income voters. That is the rich will optimize their policy selection in such a way that they (1) meet their budget constraint and (2) at least leave the poor indifferent between their policy offering and the offering that the poor would consider optimal.

To incorporate inequality into the analysis, we define<sup>5</sup>  $\delta$  and  $(1 - \delta)$  as the fraction of, respectively, rich and poor citizens in any given society. Similarly, we define  $\phi$  and  $(1 - \phi)$  as the share of income of, respectively, the rich and the poor. Using these simple definitions we can express the income of the rich ( $w_R$ ) and the poor ( $w_P$ ) as a function of inequality:

$$w_R = \frac{\phi\bar{w}}{\delta} \quad \text{and} \quad w_P = \frac{(1 - \phi)\bar{w}}{1 - \delta}$$

Finally, elites (rulers) face a standard budget constraint defined by  $t\bar{w} = b_P + b_R + g$ . To capture the variety of experiences in terms of state/fiscal capacity, we impose the assumption that a share,  $\lambda$ , of the income of the rich is non-taxable by low income voters.  $\lambda$  allows us to capture the role played by the state's ability to monitor and tax its citizens. It also allows us to analyze the predictions emerging from exogenous changes in such capacity. Accordingly, the budget constraint is defined as:

$$t\bar{w}(1 - \lambda\phi) = b_P + b_R + g \quad \text{for the share of citizens } (1 - \delta)$$

$$\text{and } t\bar{w} = b_P + b_R + g \quad \text{for the share of citizens } \delta$$

On the basis of these premises, we model the problem as a strategic interaction in which low income voters decide whether to vote (or not), and the elite parties choose which policy tool to concentrate their efforts on. Critically, we assume that the poor will vote if their utility threshold is satisfied by the offerings made by the party of the rich. Therefore, solving the model requires to take two steps, sequentially:

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<sup>4</sup>Given that the rich have perfect information and move first, they will exploit this advantage by optimizing their utility based on their information.

<sup>5</sup>We follow the notation in Acemoglu and Robinson (2006).

1. Identify the optimal values of taxes ( $t^*$ ), targeted goods ( $b_P^*$ ), and public goods ( $g^*$ ) for the poor, given the budget constraint. These values define the indifference threshold for the poor to turnout to vote. The problem for low income voters is defined as follows:

$$\begin{aligned}
& \underset{t, b_P, g}{\text{maximize}} && U_i(t, b_P, g) = (1 - t)w_P + \alpha \ln(b_P) + g \\
& \text{subject to} && t\bar{w}(1 - \lambda\phi) = b_P + b_R + g
\end{aligned} \tag{1}$$

Where  $\alpha$  capture the sensitivity of low income voters to targeted goods. As detailed in the Supplementary Appendix, this yields the following results:  $b_P^* = \alpha$ ;  $b_R^* = 0$ ;  $t^* = t^{max} \leq 1$ ; and  $g^* = t\bar{w}(1 - \lambda\phi) - \alpha$ . These in turn allow to define the poor voter's utility threshold for voting. Poor voters will vote under any combination of  $t$ ,  $b_P$ , and  $g$  that generates levels of utility *at least* similar to those defined by:

$$\bar{U}_P = (1 - t^{max})w_P + \alpha \ln(\alpha) + t\bar{w}(1 - \lambda\phi) - \alpha \tag{2}$$

This expression defines the level of reservation utility of the poor that the elites must meet with their policy offerings so that the latter turn out to vote. Importantly for our subsequent analysis, note that the reservation utility of the poor declines as capacity decreases ( $\lambda$  increases):  $\frac{\partial \bar{U}_P}{\partial \lambda} = -t\bar{w}\phi$

2. Identify the optimal values of taxes ( $t^*$ ), targeted goods ( $b_P^*, b_R^*$ ), and public goods ( $g^*$ ) for the elite. The elites, irrespective of their ideological leanings, need to choose a portfolio of targeted goods, public goods, and taxes that meets two constraints: (1) a budget constraint (recall that the poor had limited ability to tax the elite, but the elite has full capacity to tax itself); and, crucially, (2) a political constraint driven by the need to meet the mobilization threshold of low income voters defined previously in

(2). Accordingly, its maximization problem can be defined as:

$$\begin{aligned}
& \underset{t, b_r, g}{\text{maximize}} && U_i(t, b, g) = (1 - t)w_R + \beta \ln(b_R) + g \\
& \text{subject to} && t\bar{w} = b_P + b_R + g \\
& \text{and to} && (1 - t)w_P + \alpha \ln(b_P) + g \geq \bar{U}_P
\end{aligned} \tag{3}$$

Where  $\beta$  captures the sensitivity of high income voters to targeted goods for the elite and  $\bar{U}_P$  defines the low income voters' utility threshold as defined above.

## 2.2 Comparative Statics and Hypotheses

Solving the model<sup>6</sup> allows us to explore how the relationship between economic inequality and the elite's choice of local targeted goods for low income citizens shape turnout, especially turnout among low income voters. Recall from the set-up above that we proxy inequality from two angles: the proportion of low income citizens in society ( $1 - \delta$ ) and the share of income owned by high income citizens ( $\phi$ ). The model yields the following comparative statics between these two aspects of inequality and the choice of targeted ( $b_P^*$ ) goods:

$$\frac{\partial \ln(b_P^*)}{\partial(\phi)} = \frac{-\tau^{max} \bar{w} \lambda}{\alpha} \leq 0 \tag{4}$$

$$\frac{\partial \ln(b_P^*)}{\partial(1 - \delta)} = \frac{\tau^{max} w_P \lambda}{\alpha} \geq 0 \tag{5}$$

Inequality shapes the choice of  $b_P^*$  through the interaction of two mechanisms, one economic, one political. The economic mechanism concerns both the size of the pool of targetable voters ( $1 - \delta$ ) and the economic ability of the elite to finance such efforts ( $\phi$ ). On one hand, equation (4) implies that as the elites become wealthier they need less resources to meet the mobilization constraint of the poor since the low income are poorer and therefore more easily mobilized<sup>7</sup>. On the other, equation (5) means that the optimal level of targeted

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<sup>6</sup>step by step details are provided in the Supplementary Appendix

<sup>7</sup>This effect is stronger the higher the average income of society ( $\bar{w}$ ), reflecting the well known intuition that development undermines clientelism (Scott, 1969; Stokes et al., 2013)

goods towards the poor ( $b_P^*$ ) increases in the number of poorer voters, especially in contexts low capacity (high  $\lambda$ ).

The political mechanism concerns the incentives of the elites to meet the low income citizens' reservation utility constraint. Equation (5) above suggests that as the absolute income of the poor increases ( $w_P$ ), the level of targeted goods necessary to meet the poor's reservation utility also rises. In addition, the incentives to meet such a constraint, and therefore get the poor to vote, are also affected by the the ability of the state to monitor and tax its citizens, and accordingly, the ability of the elite to hide away part of their wealth and/or engage in mismanagement for political purposes ( $\lambda$ ).

Remember that a lower  $\lambda$  implies greater capacity and therefore an increase in the reservation utility of the poor to actually turnout to vote<sup>8</sup>. Substantively, this implies that as the capacity of the state increases it becomes more expensive to acquire the support of the poor by supplying targeted goods. Accordingly, the extent to which elites offer targeted goods in response to increases in their income or increases in the share of poor voters is moderated by the level of the state capacity. As the state's monitoring capacity declines, elites are better off using more targeted goods to meet the low income voters' reservation utility constraint. By contrast, as  $\lambda$  tends to 0, the connection between economic inequality (whether captured through the number of poor or the share of income by the rich) and the the optimal level of targeted goods weakens until the point in which  $\lambda = 0$ , when it actually disappears. This result uncovers a channel through which interventions to increase capacity, thus reducing  $\lambda$ , crucially affect elites' mobilization strategies.

The analysis above suggests that elites do not only react by mobilizing against the increasing revenue raising power of the state (as in Kasara and Suryanarayan (2014)). Rather, under conditions of high inequality and low state capacity, they strategically mobilize low income voters to secure their political position. These are the circumstances that render targeted spending towards the poor (including clientelism) both rational and self-enforcing<sup>9</sup>.

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<sup>8</sup>Recall that this is the case as, working from (2) above,  $\frac{\partial \overline{U}_P}{\partial \lambda} = -t\overline{w}\phi$

<sup>9</sup>For evidence consistent with this theoretical contention in the Brazilian case, see Timmons and Garfias (2015).

These results yield two sets of empirical expectations that will structure the empirical analysis in the rest of the paper. The first one concerns the steady state, equilibrium patterns in the relationship between inequality, capacity, strategic spending, and turnout; the second one concerns the political implications of *capacity shocks* through monitoring interventions to reduce the political manipulation of spending.

- **Status Quo:**

**Hypothesis 1.1:** Under high inequality and low capacity, elites should resort to a higher use of local targeted goods as a mobilization strategy of low income voters.

**Hypothesis 1.2:** Under high inequality and low capacity, we should observe higher rates of political participation especially in areas with more low income voters.

- **Capacity Shocks via Monitoring Interventions:**

Given a status quo of high economic inequality and low capacity, institutional reforms that increase the state's monitoring ability on incumbents (i.e. reduce  $\lambda$  in the model) imply:

**Hypothesis 2.1:** A reduction in the provision of local targeted goods towards lower income voters.

**Hypothesis 2.2:** A reduction in the level of turnout, as incumbents are less capable of meeting the low income voters' reservation utility to participate.

In addition, a corollary follows from hypotheses 2.1 and 2.2: external interventions that limit the ability of elites to manipulate spending to their electoral advantage should translate in a reduction in the probability of re-election of the incumbent, as her ability to secure the support of a large pool of voters declines. Overall, Hypothesis 1 concerns equilibrium levels of turnout and targeted spending under the status quo. Hypothesis 2 speaks to the implications of exogenous manipulation in the levels of capacity. We turn now to our empirical strategy.

## 3 Empirical Strategy: Design and Measurement

### 3.1 Research Design

To evaluate them, we join a recent stream of scholarship exploiting Brazilian municipalities to identify mechanisms driving the interaction between voters and politicians in contexts with a strong incidence of corruption, clientelism, and inequality (Hidalgo and Nichter, 2016; Brollo, 2012; Brollo et al., 2013). Our specific strategy focuses primarily on the random audits by the Brazilian government on its municipalities (Ferraz and Finan, 2008, 2011).

Beyond the obvious advantage of holding constant potential cross-national sources of heterogeneity, three reasons render Brazil a suitable case for our purposes. First, in 1997 Brazilian authorities successfully promoted a constitutional change to allow re-election of incumbents at the local level, a provision implemented from 2000 onwards (Ferraz and Finan, 2008, 2011). Second, Brazil is a democracy where voting is legally compulsory for individuals between 18 and 70 in all elections. This provision notwithstanding, there remains considerable variation in the levels of turnout across localities. For the localities in our sample, the range was between 65% and 96% in 2000 and 2004. In both instances the distribution was approximately normal, as shown in the Supplementary Appendix in Figure C.1. This reduced variation due to institutional constraints makes Brazil a harder case to test our hypotheses.

Second, the launch of a major anticorruption initiative in 2003, led by the *Controladoria General da União* (CGU) to scrutinize the use of federal funds by local authorities, offer an opportunity to identify the impact of increases in the state’s monitoring capacity. The audits are themselves a partisan effort by Lula’s PT to undermine the dependency by many of its potential voters from established clientelistic networks: they were part of a multidimensional policy strategy that included *Bolsa Família* and major infrastructural investments such as the program *Luz para Todos* (Aráujo and Beramendi, 2018). The goal was to reduce voters’ dependencies on the demand side (Bobonis et al., 2017) and, through the audits, limit the ability of local incumbents to manipulate the budget on the supply side. Interestingly,

when it became apparent that auditing all municipalities was unfeasible, the CGU turned to randomization to select its targets, thus creating a natural experiment. The audits are de facto an exogenous capacity shock on the ability by local incumbents to steer the budget to their electoral advantage<sup>10</sup>.

The implementation of the audits works as follows:

1. Through a sequence of lotteries, the CGU chose randomly about 8% of a total of 5500 Brazilian municipalities, including state capitals and coastal cities (N of audited municipalities=366). Once a municipality is chosen, the CGU gathers information on all federal funds received and sends a team of auditors to examine them. Auditors get information from the community and the local council members about any form of malfeasance or misuse of funds, as well as from the local documentation available.
2. Immediately, after the inspection (about a week long visit), a detailed report is sent back to the CGU, which in turn forwards it to the federal accounting auditor (*Tribunal de Contas da Uniao*), the judiciary, and all members of the local council. A summary with the key findings for each audited municipality is made publicly available.
3. Critically, we have information (thanks to Ferraz and Finan (2008, 2011)) on the date in which the reports were released to parties and voters. As a result we can exploit the contrast between those municipalities in which the audit results were released before the 2004 election and those in which they were not<sup>11</sup>.

The combination of the random selection of municipalities and the discontinuity around the 2004 election determine the nature and composition of the treatment and control groups.

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<sup>10</sup>This is important because endogenous relationships abound in the literature. Fergusson, Larreguy and Riano (2015) show how parties with a strategic advantage in clientelistic politics will oppose investments in state capacity, thus limiting pro-equality politics. Debs, Helmke et al. (2010) show that the left fares better under equality because voters are more likely to cling to pro-redistributive coalitions that in turn help contain inequality. Bursztyjn (2016) focuses in turn on voter's demand: it is the voters themselves who may not want more public goods.

<sup>11</sup>Given the short time span between selection, visit, and release randomization determines both which particular municipality is selected and when the information is released. There is no room for strategic manipulation of the information by parties or the federal government.

Since all the municipalities included in our sample have been investigated, the *treatment* is purely informational. The *treatment group* includes municipalities that have been audited and in which the results of the investigation have been released before the 2004 election, and the *control group* includes all the municipalities where the investigation took place and was released after the 2004 election. Table B.2 in the Supplementary Appendix includes the balance tables on relevant covariates for the treatment and control groups. For most of the covariates there are not significant differences across the two groups, except for some municipality size variables: e.g. population and number of legislators per voter.

### 3.2 Linking Model and Empirical Analyses: Measurement

Before discussing our estimation and specification choices, it is important to be clear about the link between our model parameters and the available measures in the sample of Brazilian municipalities. We proxy the *targeted goods towards low income voters* ( $b_P$ ) by local primary education spending. Its incidence on lower income groups relative to other forms of spending is well understood (Ansell, 2010) and, in the context of Brazil, is a policy in which incumbents have ample margin to engage in inefficient forms of redistribution via part-time employment (Bursztyn, 2016; Robinson and Verdier, 2013; Colonnelli, Teso and Prem, 2017). But importantly, for robustness, we also use the log of public employees at the municipality as an additional indicator of targeted spending.

The number of low income voters ( $(1 - \delta)$ ) and the wealth share of high income voters ( $\phi$ ) capture, respectively, the demand and the supply sides of political exchanges between elites and low income citizens. Conveniently, they are, by construction, two key dimensions of standard inequality measures. The shape of the distribution of income depends on the relative share of low versus high income citizens, and the relative share of income that accrues to each of them. This match between our theoretical model and the construction of the measure is what leads to use the Gini coefficient at the level of the municipality as our key independent variable of interest. For completion purposes, however, we show in the Supplementary Appendix that our main empirical results regarding the capacity shock are

robust to substituting the Gini measure for a poverty measure<sup>12</sup>.

Note that  $\lambda$  is defined in the model as the inverse of the state capacity to monitor its elites. A high  $\lambda$  implies an ability of elites to hide their wealth, which gives them a lot of political discretion. Our empirical measure, taken directly from the audits' reports, follows the same logic. We employ a measure of *audited local mismanagement* and that is defined as “the number of violations divided by the number of service items audited” (Ferraz and Finan, 2011, p.1284). These violations include the performance of uncompetitive bidding for local contracts, and various ways of turning public goods into targeted goods, most prominently the misuse of resources earmarked for other purposes (i.e. using resources intended for health to boost teachers salaries or, as typically recorded in individual municipal reports, to hire a larger pool of part-time teachers). The higher the reported audited mismanagement, the stronger the ability of incumbents to manipulate the use of federal funds for political gain. By implication, the higher the monitoring capacity of the state, the lower the chances that local incumbents can engage in such practices<sup>13</sup>. Accordingly, audit exposure is, as discussed above, a measure of a genuinely exogenous capacity shock on  $\lambda$ .

We match the key measures of our parameters of interest to census-based socio-demographic and economic information at the local level, as well as to detailed political information obtained from the Tribunal Superior Electoral (TSE), including the level of turnout in local elections, and data on budgetary choices by local governments. The latter allow to capture how local governments use policies such as primary education to manipulate salaries and employment opportunities in the public sector as part of their electoral strategy (Calvo and Murillo, 2004; Bursztyn, 2016). These three features allow us to test whether in equilibrium municipalities with a higher incidence of inequality and low capacity are associated with larger levels of budgetary commitments towards policies that can be targeted towards constituents.

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<sup>12</sup>See section I of the Supplementary Appendix

<sup>13</sup>Note that the measure does not include clerical or accounting errors or fiscal adjustments in year to year budgets.

## 4 Empirical Analysis: Findings

### 4.1 The Status Quo: Turnout Rates and Targeted Goods

Our first set of hypotheses (H1) states that under high inequality and low capacity, both targeted mobilization efforts towards low income voters (1.1) and turnout rates (1.2) will be higher. We evaluate this the cross-section on a subsample of audited municipalities for which our capacity measure is available. Recall that we assume that the higher the levels of local mismanagement (as captured by (Ferraz and Finan, 2011) the lower the capacity at status quo.

#### 4.1.1 Targeted Mobilization under the Status Quo

We begin by analyzing the conditions under which local political elites are more likely to resort to targeted mobilization. According to our theoretical expectations (H1.1), targeted mobilization should be higher when inequality is high and state capacity is low. As outlined above, to capture incumbents' budgetary effort during the 2000-2004 legislature towards low income voters, we use two measures: first, the (log) local spending in primary education. Specifically, we employ the data collected recently by Bursztyn (2016) on the amount of local public spending in primary education at the municipality level. And second, we analyze the log of total municipal public employees as our second proxy for the provision of targeted goods at the local level. To explore the determinants of municipal spending in targeted goods towards the poor under the status quo we estimate the following specification:

$$\text{LogSpending} = \alpha + \beta_1 \text{Ineq}_{m,s} + \beta_2 \text{Mism}_{m,s} + \beta_3 \text{Ineq}_{m,s} \times \text{Mism}_{m,s} + \eta X_{m,s} + v_s + \epsilon_{m,s} \quad (6)$$

For the controls, we follow a similar specification to the one in Bursztyn (2016) and Ferraz and Finan (2008). In addition to regional state level fixed effects ( $v_s$ )<sup>14</sup> models in Table 1 also include controls for the average municipality budget in all columns; controls

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<sup>14</sup>The inclusion of state level fixed effects aims to control for unobserved heterogeneity.

for the municipality median household income and for previous incumbent re-election in columns (2), (3) and (4); and the mayors' characteristics are included as controls in columns (3) and (4)<sup>15</sup>. Finally, but equally important, column (4) also include party fixed effects, since spending priorities might be of course responsive to parties' ideological concerns. The sample includes all the available observations, including mayors in their first and second term, with information regarding the level of audited mismanagement at the local level. All the standard errors presented are clustered at the regional state level.<sup>16</sup>

Table 1: Spending under the Status Quo

Targeted Goods Spending 2004	All Incumbents			
	<i>Public Employees</i>	<i>Public Employees</i>	<i>Primary Educ Spending</i>	<i>Primary Educ Spending</i>
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Inequality	-0.84 (0.60)	-0.83 (0.68)	-0.53* (0.30)	-0.49 (0.30)
Mismanagement	-0.31** (0.15)	-0.31* (0.15)	-0.19* (0.09)	-0.18* (0.10)
Inequality X Mismanagement	0.55** (0.27)	0.55* (0.27)	0.33** (0.15)	0.30* (0.16)
Constant	-0.25 (0.66)	-0.23 (0.77)	-1.22** (0.48)	-1.30** (0.54)
Municipality Controls	Yes	Yes	Yes	Yes
Incumbent Characteristics Controls	Yes	Yes	Yes	Yes
Regional State FEs	Yes	Yes	Yes	Yes
Party FEs	No	Yes	No	Yes
R-squared	0.88	0.89	0.97	0.97
N	366	366	306	306

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.

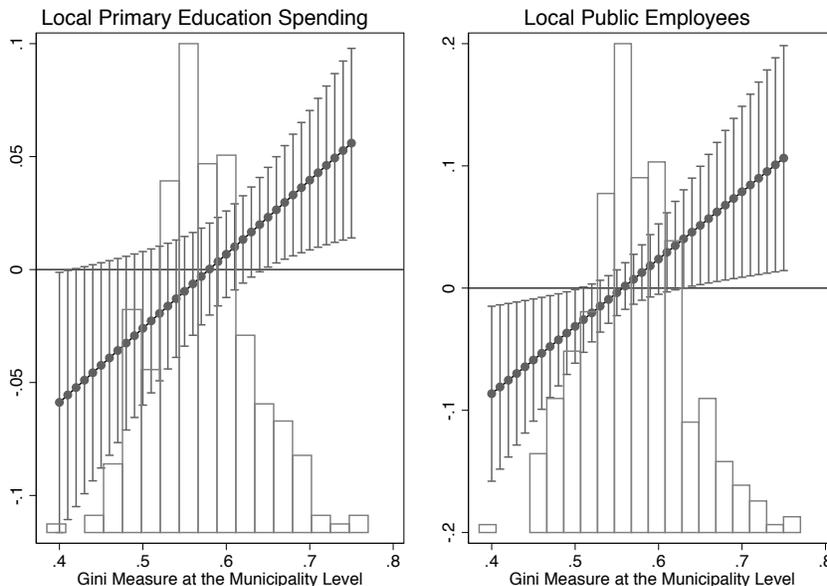
Consistent with our theoretical expectations, all models in table 1 report a positive and significant interaction between economic inequality and state capacity, which, again, is proxied by the level of mismanagement recorded during the audits. Regardless of the specific indicator of targeted spending used, our findings provide strong support to H1.1: both local

<sup>15</sup>Mayors' specific characteristics include age, gender, level of education, and past non-consecutive experience as a mayor or council member. Finally, we also include municipality electoral controls: the share of council members from the same party as the mayor, whether the mayor was from the same party as the governor, the effective number of parties in the 2000 election, and the margin of victory and the change in the electoral census.

<sup>16</sup>The clustering at the state level is designed to account for the geographical distribution of the units of observation.

primary education spending and the log of municipal employees reach the highest level when inequality is high and capacity is low. Figure 1 illustrates the marginal effect (based on column (4) in Table 1) of capacity on either type of spending. Unequivocally, low capacity enhances targeted mobilization precisely at high levels of inequality<sup>17</sup>.

Figure 1: Marginal Effects of Audited Mismanagement on Spending Decisions



Subsequent explorations of the interaction between mismanagement and economic inequality in two subsamples of urban and rural municipalities confirms that this effect is dominant in rural areas and completely absent in urban areas<sup>18</sup>. This additional evidence further reinforces the notion that strategic spending in primary education and public employment functions, in part, as an instrument to mobilize low income voters. A potential concern about these analyses, however, is that the use of local mismanagement as a proxy for capacity (or to be more precise, its inverse) conflates the capacity of the state to monitor elites' misbehavior and other potential motives to engage in mismanagement. To the extent

<sup>17</sup>See section D of the Supplementary Appendix for a robustness check of the linear interactions in which we provide a graphical illustration of both the bin and kernel estimates (Hainmueller, Mummolo and Xu, forthcoming)

<sup>18</sup>See the additional tables in the Supplementary Appendix E

that mismanagement is a good proxy for capacity, it should be the case that local elites exploit the room of maneuver to manipulate spending when they need it most. In table 2 we use the margin of victory in the 2000 elections as a moderator in the relationship between inequality, capacity, and spending. As the margin tends to zero, local incumbents' incentives to exploit the lack of capacity and further increase targeted mobilization grow stronger. We take this evidence to validate our strategy to use the past levels of local mismanagement as a proxy for local capacity under the status quo<sup>19</sup>. Crucially, it can also be shown that this electoral exploitation of the lack of capacity occurs only among mayors that are in their first term and therefore are facing re-election incentives but not among second term mayors<sup>20</sup>.

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<sup>19</sup>Note that all the models in table 2 include the lag measure of public employees in 2002. Therefore, the models that employ the log of public employees in 2004 as outcome variable can be interpreted as being dynamic models estimating the increase in public employment.

<sup>20</sup>See also section E.5 of the Supplementary Appendix.

Table 2: Spending under the Status Quo: Capacity and Incentives

Targeted Goods Spending 2004	First Term Incumbents			
	Public Employees (1) OLS	Public Employees (2) OLS	Primary Educ Spend (3) OLS	Primary Educ Spend (4) OLS
Inequality	-1.95*	-1.92*	-1.25	-1.48*
	(0.98)	(0.95)	(0.85)	(0.73)
Mismanagement	-0.55**	-0.51**	-0.40**	-0.44***
	(0.23)	(0.24)	(0.16)	(0.14)
Inequality X Mismanagement	0.97**	0.89**	0.65**	0.71***
	(0.42)	(0.43)	(0.28)	(0.23)
Winmargin	-7.72***	-7.89***	-4.36*	-5.02*
	(2.20)	(2.23)	(2.49)	(2.62)
Winmargin X Inequality	14.10***	14.30***	7.40*	8.58*
	(4.18)	(4.26)	(4.28)	(4.52)
Winmargin X Mismanagement	2.93**	2.74**	2.22**	2.36**
	(1.12)	(1.26)	(1.03)	(1.12)
Winmargin X Inequality X Mismanagement	-5.29**	-4.94**	-3.69**	-3.95*
	(2.03)	(2.29)	(1.79)	(1.95)
Constant	-3.27*	-4.05**	-1.98	-2.33
	(1.74)	(1.81)	(1.28)	(1.40)
Lag Public Employees	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes
Incumbent Characteristics Controls	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	No	Yes
Regional State FEs	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes
R-squared	0.96	0.96	0.98	0.98
N	200	200	169	169

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.

#### 4.1.2 Turnout under Status Quo

Once established that targeted mobilization is more pervasive under high inequality and low capacity, we turn to H1.2 The first two columns in Table 3 employ the turnout rates in 2000 as dependent variables of interest and the third and fourth columns the turnout rates in 2004. The audits started in 2003 and therefore the audited levels of mismanagement are actually ex-post measures in the first two columns. But regressing the turnout rates in 2000 as a function of the ex-post audited mismanagement is also interesting since political strategies are sticky. The key results though refer to columns (3) and (4) in which we explore

the turnout rates in 2004. Specifically, we estimate the following equation:

$$TurnoutRates_{04} = \alpha + \beta_1 Ineq_{m,s} + \beta_2 Mism_{m,s} + \beta_3 Ineq_{m,s} x Mism_{m,s} + \eta X_{m,s} + v_s + \epsilon_{m,s} \quad (7)$$

As before, all models in Table 3 include regional state fixed effects ( $v_s$ ) and standard errors clustered at the state level. Importantly, now models in columns (3) and (4) include controls for the levels of fiscal transfers whereas the other two do not include them since differences in transfers received by the municipalities might be an important confounder. We also introduce a similar set of major specific controls, and two additional sets at the municipality and party levels<sup>21</sup>. All models also include a dummy that takes value 1 if the incumbent faces re-election incentives and 0 otherwise. Finally, the model in column (4) also includes party FEs to account for potential behavioral heterogeneity among political parties.

Table 3: Turnout Rates under the Status Quo

Turnout Rates 2004	All Incumbents			
	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Inequality	-0.26*** (0.07)	-0.26*** (0.07)	-0.26*** (0.06)	-0.27*** (0.06)
Mismanagement	-0.05*** (0.02)	-0.06*** (0.02)	-0.06*** (0.02)	-0.05** (0.02)
Inequality X Mismanagement	0.09*** (0.03)	0.10*** (0.03)	0.10*** (0.03)	0.10** (0.04)
Constant	1.15*** (0.08)	1.17*** (0.08)	1.09*** (0.08)	1.09*** (0.08)
Municipality Controls	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	Yes
Fiscal Controls	No	No	Yes	Yes
Regional State FEs	Yes	Yes	Yes	Yes
Party FEs	No	No	No	Yes
R-squared	0.53	0.54	0.54	0.55
N	366	366	366	366

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

<sup>21</sup>Municipal level characteristics include: the area, the log of population, the share of urban population within the municipality, the (log) local GDP per capita, the change in the level of population between censuses, the share of population over 18 with at least secondary education, whether the municipality is new and the number of active public employees. We also add controls for specific political and judicial institutions at the municipality level: presence of a judicial district, use of participatory budgeting during the period 2001-2004, and the seats to voters' ratio within each municipality.

The results in Table 3 lend support to our theoretical expectations. All models report a positive and significant coefficient for the interaction term between the Gini measure and the audited mismanagement at the municipality level <sup>22</sup>. The estimated coefficients remains highly stable with the gradual introduction of the controls, even with the inclusion of party fixed effects. Figure 2 shows the marginal effect (based on column (4) in Table 3) of the audited mismanagement on turnout rates conditional on the levels of economic inequality at the municipality<sup>23</sup>. The effects are substantively important. Under high inequality, the marginal effect of audited mismanagement is associated with between 1 and 2 more percentage points in turnout rates, whereas under very low inequality the marginal effect of audited mismanagement amounts to a 1.5 percentage points reduction in participation. Unfortunately we do not have individual level data available, but we can explore the heterogeneous effects depending on the share of urban population as a measure of urban versus rural areas, where the prevalence of low income voters is higher. To do so, we simply divide the sample according to municipalities that are above or below the median share of urban population. Additional results<sup>24</sup> show that, as expected, the positive and significant effect of mismanagement on turnout is mostly prevalent in rural areas with high inequality.

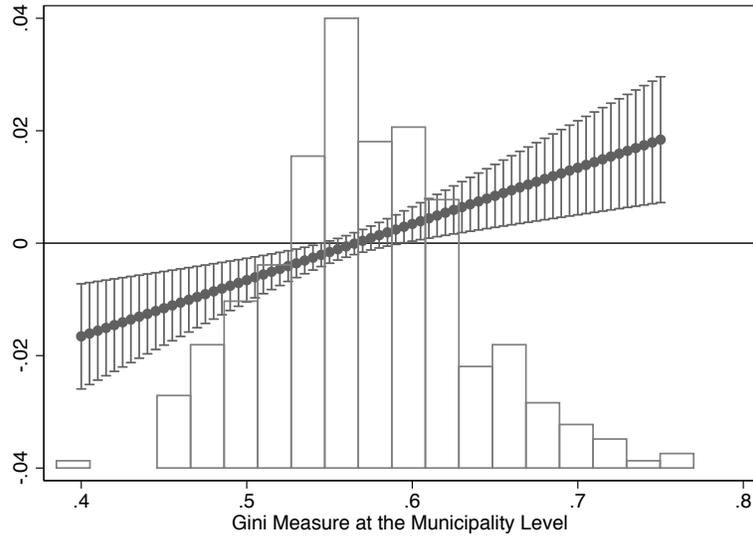
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<sup>22</sup>Interestingly, the levels of audited mismanagement are also associated with higher levels of turnout rates when inequality is high and capacity is low in the preceding local elections in the year 2000.

<sup>23</sup>In section D of the Supplementary Appendix we provide a graphical illustration of the interaction with bin and kernel estimates

<sup>24</sup>See additional results in Supplementary Appendix E)

Figure 2: Marginal Effects of Audited Mismanagement on Turnout



## 4.2 Capacity Shock

### 4.2.1 Audits Exposure and Changes in Local Targeted Goods

We turn to study exogenous changes in the monitoring ability of the state and their consequences. We begin evaluating the consequences of audits' releases before 2004 on the provision of local spending in primary education (H2.1). As developed above, we expect an exogenous shock that increases the monitoring ability at the municipality level to be associated with a decline in the provision of targeted goods towards low income voters.

We study the determinants of the change in the average levels of local spending in primary education by comparing the (log) averages between the 2000-2004 and the 2004-2008

legislature terms<sup>25</sup>:

$$\begin{aligned}
\Delta SpendingEduc &= LogSpending_{08-04,Leg} - LogSpending_{04-00,Leg} = \\
&+ \alpha + \beta_1 Ineq_{m,s} + \beta_2 Mism_{m,s} + \beta_3 Ineq_{m,s} x Mism_{m,s} \\
&+ \beta_4 Exp_{m,s} + \beta_5 Exp_{m,s} x Ineq_{m,s} + \beta_6 Exp_{m,s} x Mism_{m,s} \\
&+ \beta_7 Exp_{m,s} x Ineq_{m,s} x Mism_{m,s} \\
&+ \eta X_{m,s} + v_s + l_t + \epsilon_{m,s}
\end{aligned} \tag{8}$$

All the models in Table 4 control for the log change in the total municipality budget as well as the median household income level during the 2000-2004 legislature. Also important, all models except the one in column (1) include regional fixed effects ( $v_s$ ) to account for unobserved heterogeneity. The columns reported gradually incorporate controls for the incumbent and party characteristics during the 2004-2008 legislature: mayor previous re-election, mayor characteristics<sup>26</sup> and party fixed effects. The last column in Table 4 also includes lottery fixed effects ( $l_t$ ). And as usual, all models report clustered standard errors at the state level. The novelty is that now we only include mayors that were in their first term when localities were audited. We do so to sharpen the comparison and to make sure that subsequent changes in spending priorities are not driven by ex-ante differences in re-election incentives<sup>27</sup>.

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<sup>25</sup>The data comes from Bursztyn (2016) and refers to the averaged and deflated spending levels across the 2001-2004 years (for the first legislature) and the years 2005-2008 (for the subsequent legislature).

<sup>26</sup>The controls for the mayor characteristics include: gender, age, age squared, marriage and education.

<sup>27</sup>Importantly, in the Supplementary Appendix section H.1 we provide evidence that mayors in their first mandate tend to resort less mismanagement compared to second term mayors, and specially so under high levels of inequality.

Table 4: Audits Exposure and Changes in Local Targeted Goods

Primary Educ Spending Change	First Term Incumbents					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.31 (0.51)	-1.12*** (0.39)	-1.14*** (0.37)	-1.14** (0.46)	-1.74*** (0.43)	-2.14*** (0.42)
Mismanagement	-0.33** (0.15)	-0.42*** (0.15)	-0.46*** (0.14)	-0.44*** (0.14)	-0.51** (0.19)	-0.52** (0.20)
Ineq X Mismanagement	0.54** (0.24)	0.69*** (0.23)	0.76*** (0.22)	0.74*** (0.24)	0.87** (0.32)	0.90** (0.34)
Exposed	-0.33 (0.30)	-0.64** (0.25)	-0.64** (0.25)	-0.57 (0.33)	-0.96** (0.39)	-1.00** (0.41)
Exp X Inequality	0.56 (0.53)	1.07** (0.44)	1.07** (0.43)	0.96 (0.57)	1.63** (0.67)	1.87*** (0.66)
Exp X Mismanagement	0.34* (0.17)	0.45** (0.18)	0.48*** (0.17)	0.45** (0.18)	0.56** (0.23)	0.55** (0.23)
Exp X Ineq X Mismanagement	-0.56* (0.28)	-0.73** (0.28)	-0.78*** (0.27)	-0.75** (0.30)	-0.94** (0.39)	-0.93** (0.39)
Constant	0.19 (0.31)	0.63** (0.23)	0.64*** (0.21)	0.74 (0.50)	1.07* (0.58)	1.15** (0.50)
Municipality Budget Controls	Yes	Yes	Yes	Yes	Yes	Yes
Re-Elected Incumbent Control	No	No	Yes	Yes	Yes	Yes
Incumbent's Controls	No	No	No	Yes	Yes	Yes
Party FEs	No	No	No	No	Yes	Yes
Lottery FEs	No	No	No	No	No	Yes
Regional State FEs	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.66	0.75	0.75	0.77	0.80	0.82
N	145	145	145	145	145	145

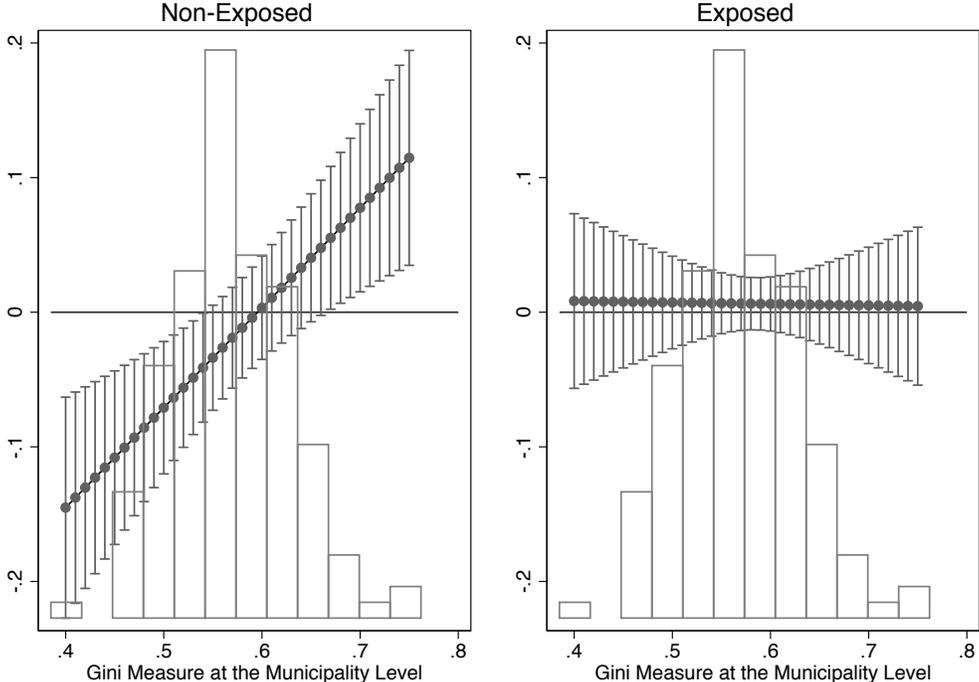
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

To directly illustrate the results, Figure 3 plots the marginal effects of audited mismanagement on the log change in primary education spending for non-exposed (control) and exposed (treated) municipalities. As expected, for the control group the status quo persists and mismanagement continues to exert a positive effect on the dynamics of spending in primary education when inequality is high. Interestingly, and in full alignment with our theoretical predictions, this relationship completely vanishes for the treated municipalities (i.e. exposed municipalities to the shock of audits release). In the latter the levels of audited mismanagement bear no effect on the dynamics of primary education spending at the local level.

The fact that the relationship between audited mismanagement and the mid-term changes in local spending in primary education vanishes for exposed municipalities is highly consistent

with the model results. According to our theoretical expectations, as the monitoring capacity of the state increases ( i.e. lower values of  $\lambda$ ) elites are better off relying less on local targeted goods to meet the low income voter’s reservation utility. And as  $\lambda$  tends to 0, arguably what happens under exposure, the relationship between economic inequality, capacity and provision of targeted goods is expected to disappear. Thus, our findings suggest that efforts to curb down clientelism may have constraining effects on outcomes such as spending on primary education, thus contributing to an incipient literature on the potential detrimental effects of anti-corruption programs in Brazil (Lichand, Lopes and Medeiros, N.d.). We resume this discussion in the conclusion.

Figure 3: Marginal Effect of Audited Mismanagement on Education Spending Change



**4.2.2 Audits Exposure and Changes in Political Participation**

We turn now to study the impact of randomized audits on turnout (H2.2). We model the determinants of the change in the levels of turnout between 2000 and 2004 as a function

of the interaction between: economic inequality, capacity, and a dummy capturing whether the municipality belongs to the treatment or the control group (exposure before versus after 2004). And again, to keep the comparison as sharp as possible we limit the sample to majors who seek re-election for the first time<sup>28</sup>. By restricting the sample this way, we avoid the confounding effect of the term in office. Thus, we estimate the following equation for the sample of audited municipalities with mayors in its first term mandate during the 2000-2004 legislature:

$$\begin{aligned}
\Delta Turnout &= LogTurnout_{2004} - LogTurnout_{2000} = \\
&+ \alpha + \beta_1 Ineq_{m,s} + \beta_2 Mism_{m,s} + \beta_3 Ineq_{m,s} x Mism_{m,s} \\
&+ \beta_4 Exp_{m,s} + \beta_5 Exp_{m,s} x Ineq_{m,s} + \beta_6 Exp_{m,s} x Mism_{m,s} \\
&+ \beta_7 Exp_{m,s} x Ineq_{m,s} x Mism_{m,s} \\
&+ \eta X_{m,s} + v_s + l_t + \epsilon_{m,s}
\end{aligned} \tag{9}$$

All models include now lottery fixed effects (FEs), ( $l_t$ ) to account for different timing in the audit release. Columns (1) and (2) do not include regional fixed effects ( $v_s$ ), whereas all the other columns include them. Since we know that clientelism is geographically concentrated among certain areas, the inclusion of regional FEs is important. Finally, the last two columns exclude those municipalities in which the mayor was member of the PMDB. In contexts where clientelistic parties are hegemonic incumbents have the potential to activate compensating mechanisms that mute the political consequences of the federal audits. Since the PMDB is widely recognized as one of the parties with powerful clientelistic machines, we want to assess how sensitive our findings to the inclusion/exclusion of municipalities under its control are.

We have also checked if the correlation between the levels of audited mismanagement and inequality is the same across exposed (treatment) and non-exposed (control) municipalities. Interestingly, the correlation between audited mismanagement and inequality is negative and

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<sup>28</sup>Recall that we have motivated the exclusion of the second term mayors on the basis of reported differences in audited mismanagement. However, the results do not depend on excluding the second term mayors. In the Supplementary Appendix, section F.5, we show that the results hold when including both first and second term mayors (i.e. all the incumbents)

significant in the treatment group, but insignificant in the control group. This is the case because under high inequality the levels of audited mismanagement are lower in exposed municipalities. If any, this negative correlation runs contrary to the possibility of finding significant results in our main specification. To test the robustness of the results we have rerun the models excluding extreme upper values for the mismanagement variable and the results do not change.<sup>29</sup>

The findings reported in Table 5 are robust to different specifications and consistent with our theoretical expectations. Audits information release have a negative effect on turnout especially when both the levels of audited mismanagement and economic inequality are high. Interestingly, though, the results are specially strong once the incumbents who are members of the PMDB are excluded in the last two columns of Table 5. This may reflect the fact that mayors in areas where clientelism is hegemonic have a wider array of exonerative strategies against the impact of the audits at their disposal.

In the Supplementary Appendix<sup>30</sup> we provide two additional robustness checks that are important regarding the effects of the capacity shock on changes in turnout: (i) we show that the demobilization effects were specially significant in municipalities where the incumbent was not re-elected; and (ii) we also illustrate that if we substitute the mismanagement variable for the log of public employees in 2002 at the municipality level as an alternative proxy for local capacity the results are exactly the same. The first one is reassuring in terms of providing evidence that the electoral demobilization went hand in hand with the loss of credibility of the incumbent. The second one is key since employing an alternative measure of capacity, the stock of public employment before the audits, yields similar results<sup>31</sup>.

To facilitate the interpretation of the results, Figure 4 compares the marginal effect of audited mismanagement on changes in municipal levels of turnout in the control and treatment groups at various levels of inequality. Given high levels of inequality, in those

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<sup>29</sup>Specifically, see Table F.1 in the Supplementary Appendix.

<sup>30</sup>See section F in the Supplementary Appendix to see the additional robustness checks

<sup>31</sup>Recall that before we already showed that the increase in the number of employees might be itself understood as a targeted good, whereas here we employ the stock of public employment as a proxy for capacity.

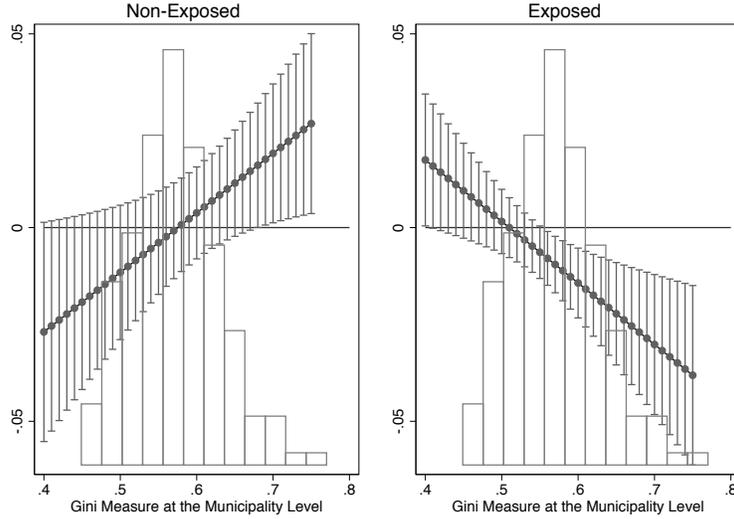
municipalities where the external audits were not released, the more incumbents misuse federal funds, the higher the increase in turnout. By contrast, in those municipalities where the audit took place and was released before the 2004 election, the same strategy triggers a reduction in electoral participation of a similar magnitude. In other words, the status quo persisted in the control group but was radically transformed in the treatment group. Consistent with Hidalgo and Nichter (2016), the findings here contributes to an emerging agenda on the impact of audits on turnout. Our results, though, emphasize that the negative effects of external audits on turnout changes were highly conditional to the pre-existing levels of economic inequality and capacity.

Table 5: Audits Exposure and Changes in Turnout Rates

Turnout Change	First Term Incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Inequality	-0.19 (0.14)	-0.24 (0.16)	-0.13 (0.11)	-0.19 (0.15)	-0.33* (0.17)	-0.47** (0.17)
Mismanagement	-0.05 (0.03)	-0.06 (0.04)	-0.03 (0.03)	-0.04 (0.04)	-0.06 (0.05)	-0.09* (0.05)
Ineq X Mismanagement	0.09 (0.06)	0.11 (0.06)	0.04 (0.05)	0.07 (0.07)	0.11 (0.08)	0.15* (0.08)
Exposed	-0.17* (0.10)	-0.20* (0.11)	-0.15 (0.10)	-0.18 (0.11)	-0.28* (0.14)	-0.35** (0.14)
Exp X Inequality	0.30 (0.18)	0.35* (0.19)	0.27 (0.17)	0.33* (0.19)	0.54** (0.25)	0.67** (0.24)
Exp X Mismanagement	0.10** (0.05)	0.11** (0.05)	0.08* (0.05)	0.10* (0.05)	0.14* (0.07)	0.17** (0.07)
Exp X Ineq X Mismanagement	-0.19** (0.08)	-0.21** (0.09)	-0.16* (0.08)	-0.19* (0.09)	-0.26** (0.12)	-0.31** (0.12)
Constant	0.33** (0.15)	0.35** (0.15)	0.08 (0.14)	0.09 (0.14)	0.21 (0.20)	0.27 (0.21)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral and Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent Characteristics Controls	No	Yes	No	Yes	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	Yes	Yes	Yes	Yes
PMDB Incumbents Excluded	No	No	No	No	Yes	Yes
R-squared	0.54	0.55	0.68	0.70	0.62	0.66
N	203	203	203	203	165	165

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 4: Marginal Effect of Audited Mismanagement on Turnout Change



### 4.2.3 Corollary: Audits Exposure and Re-Election Probability

Our analysis of the capacity shock implies a stop in the used of targeted mobilization towards low income voters (H2.1) and an attendant reduction in turnout rates (H2.2). While our theoretical model does not directly theorize electoral survival, a direct implication from these two effects is the reduction in the incumbent’s electoral survival. If the incumbent’s ability to meet the low income voters’ political constraint declines, due to an increase in the monitoring capacity of the state, *ceteris paribus* so should her probability of survival.

To evaluate this empirical corollary of our argument, we estimate the re-election probability in the 2004 local elections of incumbents in their first term<sup>32</sup> as a function of the interaction between the audited levels of mismanagement and the shock associated to the exposure of the audits results before the 2004 elections. We anticipate that when there is no exposure, greater mismanagement should be associated with a higher re-election probability. In contrast, when the audits reports were released before the 2004 elections, the re-election probability should decline sharply. To explore this prediction we estimate the

<sup>32</sup>Note that again we limit the sample to municipalities with mayors in their first term.

following equation:

$$Reelected = \alpha + \beta_1 Exp_{m,s} + \beta_2 Mism_{m,s} + \beta_3 Exp_{m,s}xMism_{m,s} + \eta X_{m,s} + v_s + l_t + \epsilon_{m,s} \quad (10)$$

We run the models in this table with a logit specification since the dependent variable is dichotomous (1 if re-elected and 0 otherwise) but the results are the same if instead we implement a linear probability model. All models reported in Table 6 include lottery fixed effects ( $l_t$ ), since the proximity to the 2004 elections might be associated with unobserved heterogeneity in the incumbents' ability to circumvent the audit results. As in the previous analyses, the reported standard errors are clustered at the state level and all models include regional fixed effects. Model in columns (1) do not include the fiscal transfers controls, whereas models in columns (2), (3) and (4) do so. In addition, models in columns (3) and (4) incorporate further controls and account for the incumbent characteristics. Finally, model in column (4) also includes party fixed effects.

Table 6 provides strong evidence consistent with our expectations. Low capacity (high mismanagement) enhances the likelihood of re-election in non-exposed municipalities. By contrast, in localities where the audits results were released before the elections, higher levels of audited mismanagement actually lead to a much lower re-election probability <sup>33</sup>.

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<sup>33</sup>To provide further evidence of the relationship between targeted spending and expected re-election probability we have also run municipality fixed effects models, following Bursztyn (2016), with the entire sample of Brazilian municipalities that show that party re-election probability was indeed higher in municipalities with both greater spending in primary education and higher inequality. Specifically, in the Supplementary Appendix (section H.3) we show that spending in primary education exerts a positive effect on party re-election probability, under mid and high development levels, when inequality is high.

Table 6: Re-Election Probability and Audits Exposure

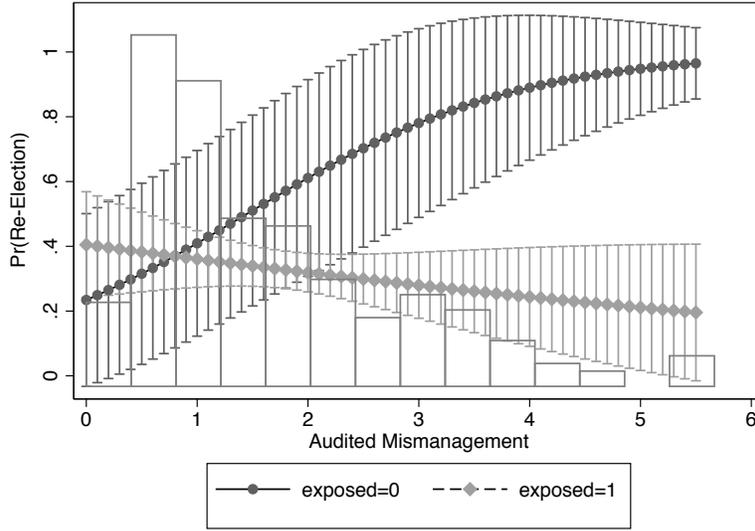
Incumbent Re-Election in 2004	First Term Incumbents			
	(1) Logit	(2) Logit	(3) Logit	(4) Logit
Exposed	0.90 (1.04)	0.80 (1.10)	1.79 (1.50)	3.59* (1.99)
Mismanagement	0.64 (0.42)	0.82* (0.44)	0.99* (0.53)	1.33** (0.61)
Exposed X Mismanagement	-0.85** (0.41)	-1.01** (0.47)	-1.12** (0.57)	-1.47** (0.66)
Constant	0.64 (2.76)	-3.98 (10.97)	1.56 (14.63)	10.04 (21.18)
Municipality Controls	Yes	Yes	Yes	Yes
Electoral and Fiscal Controls	No	Yes	Yes	Yes
Incumbent Characteristics Controls	No	No	Yes	Yes
Party FEs	No	No	No	Yes
Regional State FEs	Yes	Yes	Yes	Yes
Lottry FEs	Yes	Yes	Yes	Yes
R-squared	0.159	0.260	0.421	0.484
N	200	200	200	194

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 5 provides a graphical representation of the scale of the effects. The re-election probability declined dramatically in those municipalities in which the release of the audit results occurred before the 2004 elections. For this particular subgroup, the predicted drop in the probability of re-election across is 50 percent or more depending on the existing levels of mismanagement. The contrast with the incumbency advantage provided by the continuation of pre-existing practices in the control group is striking. This result is in line with previous contributions on the negative effect of the audits on the probability of re-election. At the same time, it offers a novel perspective since it shows that the negative effect was especially severe at very low levels of capacity -specifically, under high levels of mismanagement. Finally, we have also explored the heterogeneous effects of the drop in re-election probabilities. In the Supplementary Appendix we show<sup>34</sup> that the drop in re-election rates was specially high in exposed municipalities with both high levels of mismanagement and economic inequality.

<sup>34</sup>See section H.2 in the Supplementary Appendix

Figure 5: Audits Exposure, Mismanagement and Re-Election Probability



### 4.3 Exploring the Mechanism

#### 4.3.1 Heterogeneous effects

Our empirical strategy rests on the premise that in contexts such as Brazilian municipalities turnout rates provide relevant information on the behavior of low income voters. Unfortunately, there are no micro data available within municipalities to fully validate this premise. In support of our approach, recent findings by Cepaluni and Hidalgo (forthcoming) suggest that the type of voters affected very much depends on the type of intervention (and their associated penalties) being evaluated. When the penalties associated with the intervention affect services with access primarily reserved to middle and upper income groups, changes in turnout rates will reflect the elasticity of these groups to the intervention (in their case, age related enforcement of compulsory voting). Yet when the intervention affects instruments such as mismanagement of cash funds or access to basic social services, the expectation is that aggregate turnout rates trace in large part responsiveness by lower income strata.

The data, however, allow us to go further in support of the notion that the demobilization

effects concentrate primarily around areas with a higher share of low income voters. The theory suggests that the randomized audits exogenously reduced the effectiveness of targeted mobilization in low capacity contexts. If the mechanism operates in this way, the impact of audits on the relationship between economic inequality and turnout changes should be more apparent in areas with a higher share of low income voters. To the extent that the effects are stronger in these areas, this would suggest a stronger demobilization impact in localities with a larger presence of low income voters. Figure 6 reports the heterogeneous effect of audits in urban versus rural areas, whereas Figure 7 compares the heterogeneous effects of audits exposure in areas with low and high prevalence of education.<sup>35</sup>

Taken altogether, Figures 6-7 lend considerable support to the claim that the federal audits worked to undermine the effectiveness of clientelism as a mobilization strategy and triggered an exogenous change in the linkage between economic inequality and turnout. The disruption of the pre-existing political equilibrium took place most visibly in rural areas, where the ex-ante capacity was particularly low and with a higher density of low education, low-income voters. These are the areas in which the demobilization effect triggered by the audits emerges most strongly.

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<sup>35</sup>The corresponding Tables for the exploration of the heterogeneous effects are reported in the Supplementary Appendix section G.

Figure 6: Turnout Change: Rural versus Urban Municipalities

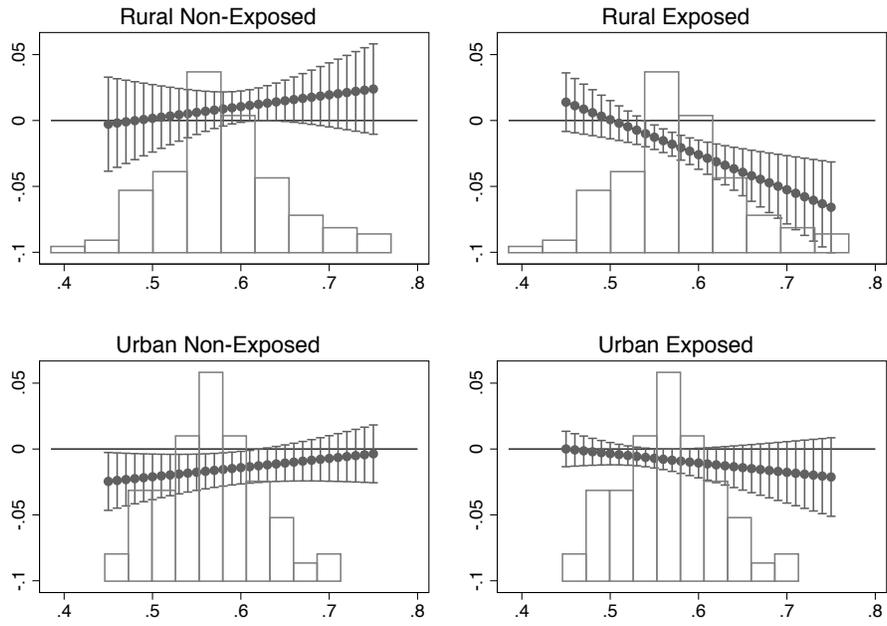
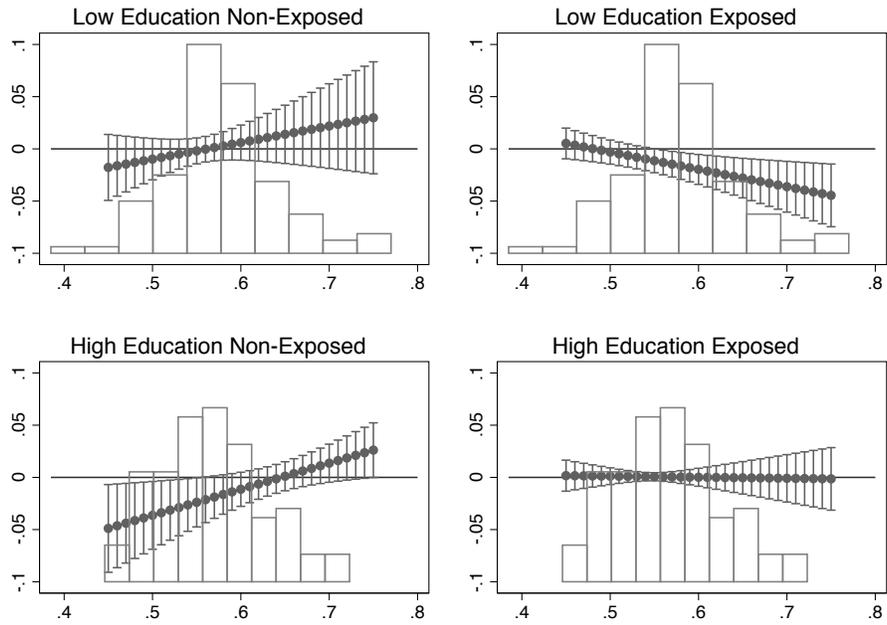


Figure 7: Turnout Change: Low Education versus High Education Municipalities



### 4.3.2 Precinct-Level Results

To further overcome concerns about a potential ecological fallacy, we take one additional step. Using data on turnout and educational levels of the population at the precinct level (*seção eleitoral*) we re-estimate the relationship of interests. We use the distribution of educational qualifications within precincts as a proxy for income distributions at each precinct, which allows us to come one step closer to having income data within the municipality. Thus we can model how the impact of audits is moderated by inequality, the scope of mismanagement, and the distribution of education (our proxy for income here) within municipalities. We perform both a precinct-level analysis with municipality fixed effects (columns 1 and 2), and a series of hierarchical linear models (columns 3 and 4). Again, we limit the analysis to first term mayors, and include several specifications, the more demanding ones including all relevant municipality controls. Figure 8 reports the main insights from the analysis.

Table 7: Turnout Change at the Precinct-Level

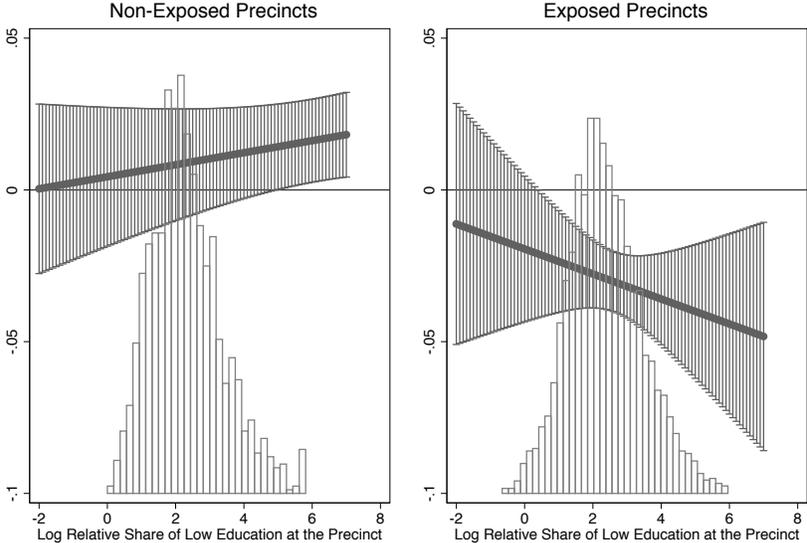
DV: Change in Turnout. Precinct-level	(1)	(2)	(3)	(4)
Loweduc X Gini	-0.11** (0.05)	-0.10** (0.05)	-0.11** (0.05)	-0.10* (0.05)
Loweduc X Mismanagement X Gini	0.04* (0.02)	0.04** (0.02)	0.04* (0.02)	0.04* (0.02)
Exposed X Loweduc X Gini	0.10* (0.06)	0.10* (0.06)	0.10* (0.06)	0.10* (0.06)
Exposed X Loweduc X Mismanagement X Gini	-0.07** (0.03)	-0.07** (0.03)	-0.07** (0.03)	-0.07** (0.03)
Change log aptos	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)
Relative to municipality loweduc		0.01** (0.01)		0.01** (0.01)
First term	Yes	Yes	Yes	Yes
Other interaction terms	Included	Included	Included	Included
Municipality controls	No	No	Yes	Yes
Model	Mun FEs	Mun FEs	HLM	HLM
N (Precincts)	7451	7451	7451	7451
N (Localities)	163	163	163	163

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

To simplify the discussion of the key results, figure 8 presents the marginal effect of mismanagement across the share of citizens with low education at the precinct level for both control (non-exposed) and treatment (exposed) groups, given a high level of inequality

(Gini=0.7). In line with the theoretical predictions and with earlier results, a higher share of uneducated citizens in the control group enhances the mobilization effect of audited mismanagement. By contrast, in treated precincts the relationship reverses. Indeed, the relationship switches more strongly the higher the share of uneducated voters at the precinct level. In other words, the demobilization effect is particularly strong precisely in those precincts where the political payoffs of targeted mobilization were at the highest level prior to the audits.

Figure 8: Marginal Effects of Audited Mismanagement on Turnout under High Inequality



## 5 Conclusion

Democracy works differently under very high inequality. Contrary to the standard view based on rich democracies, we have shown how high economic inequality and low capacity jointly facilitate a self-enforcing equilibrium in which both turnout and budgetary commitments to targetable policies emerge as an optimal strategy. In this equilibrium high formal (but not factual) political equality is achieved through elite’s strategic spending directed towards low-income voters with malleable targeted goods. In this type of political equilibrium a pervasive form of accountability prevails in which economic inequality is linked to greater turnout of low-income voters thanks to the strategic behavior of incumbents that benefit

from the low monitoring capacity of the state. We have also studied the consequences of exogenous disruptions of such equilibrium: as external interventions increase capacity, both the budgetary use of targeted goods and turnout decline, and with them so does the incumbents' likelihood of re-election. Perhaps counter intuitively, then, in context of high economic inequality such interventions might be associated with negative welfare consequences for low income voters. Our findings point to two complementary lines of research.

The first one concerns welfare effects of capacity enhancing interventions. Our findings suggest that an increase in the monitoring ability of the state against clientelism may reduce subsequent budgetary efforts in targeted goods whose budgets can be manipulated politically, particularly under conditions of high inequality and low capacity. In addition to our findings, recent evidence suggests that local politicians adjust to both changes in their institutional constraints (De La O and Garcia, 2015; Cheibub Figueiredo, 2005) and modifications in their budget constraint (Bhavnani and Lupu, 2016). Taken together, these analyses point to a discussion about the short and long run effects of reforms in contexts of low institutional capacity, about what works and does not across democracies with varying levels of institutional capacity (Harding and Stasavage, 2014). The results in this paper suggest that targeted mobilization (including clientelism) emerges as a second best strategy in low capacity contexts: some people incur in welfare losses <sup>36</sup> as a result of efforts to transition towards more programmatic politics. These are, however, short-run effects. More work is needed to establish the length of the valley of tears associated with such transition.

The second line for future research concerns the political conditions under which capacity itself evolves over time. Capacity enhancing would appear suboptimal for local incumbent elites under conditions of high inequality. Our analysis suggest that these local elites have much to gain from preventing large scale capacity investments. Challengers, on the other hand, have the opposite incentives. Understanding the conditions under which this contest results in more programmatic, tax-compliant democracies is a fruitful avenue for future research.

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<sup>36</sup>In an analysis complementary to the one in this paper, Amat and Beramendi (under contract) show that following the introduction of audits poverty actually increases in the treated municipalities.

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# Appendices

## Contents

<b>A Theory</b>	<b>3</b>
A.1 Set-up: The Choice for Low Income Voters and the Elite’s Calculus . . . . .	3
A.2 Inequality and the Elite Choice of Targeted Goods for the Low-Income Voters	4
<b>B Descriptives Statistics</b>	<b>6</b>
B.1 Descriptive Statistics . . . . .	6
B.2 Balance Table . . . . .	7
<b>C Descriptive Figures</b>	<b>8</b>
C.1 Turnout Rates at the Local Level . . . . .	8
C.2 Primary Education Spending at the Local Level . . . . .	9
C.3 Public Employees at the Local Level . . . . .	10
C.4 Gini at the Local Level . . . . .	11
C.5 Audited Mismanagement at the Local Level . . . . .	12
<b>D Robustness Checks for Linear Interactions</b>	<b>13</b>
<b>E Heterogeneous Effects under the Status Quo</b>	<b>16</b>
E.1 Primary Education Spending: Rural versus Urban . . . . .	16
E.2 Primary Education Spending: Low versus High Win Margin . . . . .	18
E.3 Public Employees: Low versus High Winmargin . . . . .	20
E.4 Turnout: Rural versus Urban . . . . .	22
E.5 Incentives and Capacity: First Term Incumbents . . . . .	24
<b>F Robustness Tests for the Capacity Shock Models</b>	<b>25</b>
F.1 Turnout Change Excluding Extreme Mismanagement Values . . . . .	25
F.2 Turnout Change: Public Employment as Capacity Measure . . . . .	26
F.3 Turnout Change: Re-Elected versus Non Re-Elected . . . . .	27
F.4 Turnout Change: Alternative Exposure Measure . . . . .	28
<b>G Heterogeneous Effects of the Capacity Shock</b>	<b>29</b>
G.1 Turnout Change: Urban versus Rural Areas . . . . .	29
G.2 Turnout Change: Low versus High Education . . . . .	30
G.3 Turnout Change: Radio versus Non-Radio Localities . . . . .	31
G.4 Turnout Change: Low versus High Win Margin . . . . .	32
<b>H Further Checks: Exclusion of Second Term and Re-Election Models</b>	<b>33</b>
H.1 Motivation for the Exclusion of Second Term Incumbents . . . . .	33
H.2 Heterogeneous Effects of Re-Election . . . . .	34

H.3	Re-Election with Complete Brazilian Sample . . . . .	36
<b>I</b>	<b>Models Employing Poverty Measures</b>	<b>38</b>
I.1	Poverty and Turnout Change . . . . .	38
I.2	Poverty and Primary Education Spending Change . . . . .	39

# A Theory

## A.1 Set-up: The Choice for Low Income Voters and the Elite's Calculus

We begin by defining the offerings elites need to provide such that low income voters turn out to vote. The problem for low income voters is defined as follows:

$$\begin{aligned} & \underset{t,b,g}{\text{maximize}} && U_i(t, b, g) = (1 - t)w_P + \alpha \ln(b_P) + g \\ & \text{subject to} && t\bar{w}(1 - \lambda\phi) = b_P + b_R + g \end{aligned} \quad (1)$$

Where  $\alpha$  captures the sensitivity of low income voters to targeted goods. The analysis yields the following results:

1.  $b_P^* = \alpha$
2.  $b_R^* = 0$
3.  $t^* = t^{max} \leq 1$  since utility is linear in  $t$
4.  $g^* = t\bar{w}(1 - \lambda\phi) - \alpha$

Accordingly, poor voters will vote under any combination of  $t$ ,  $b$ , and  $g$  that generates levels of utility *at least* similar to those defined by:

$$\bar{U}_P = (1 - t^{max})w_P + \alpha \ln(\alpha) + t\bar{w}(1 - \lambda\phi) - \alpha \quad (2)$$

This expression defines the level of utility of the poor that the elites must meet with their policy offerings so that the latter turn out to vote. The elites, irrespective of their ideological leanings, need to choose a portfolio of targeted goods, public goods, and taxes that meets two constraints: (1) a budget constraint (recall that the poor have limited ability to tax the elite, but the elite has full capacity to tax itself); and (2) a political constraint driven by the need to meet the mobilization threshold of low income voters defined in (2). Accordingly, their maximization problem can be defined as:

$$\begin{aligned} & \underset{t,b,g}{\text{maximize}} && U_i(t, b, g) = (1 - t)w_R + \beta \ln(b_R) + g \\ & \text{subject to} && t\bar{w} = b_P + b_R + g \\ & \text{and to} && (1 - t)w_P + \alpha \ln(b_P) + g \geq \bar{U}_P \end{aligned} \quad (3)$$

Where  $\beta$  captures the sensitivity of high income voters to targeted goods and  $\bar{U}_P$  defines the low income voters' utility threshold as defined above.

The Lagrangian is defined as:

$$\begin{aligned} \mathcal{L} = & (1 - t)w_R + \beta \ln(b_R) + t\bar{w}(1 - \lambda\phi) - b_P - b_R + \\ & + \mu[(1 - t)w_P + \alpha \ln(b_P) + t\bar{w}(1 - \lambda\phi) - b_P - b_R - \bar{U}] \end{aligned} \quad (4)$$

From here it follows that:

$$t_R^* = t^{max} \leq 1 \quad \text{since utility is linear in } t \quad (5)$$

$$\frac{\partial \mathcal{L}}{\partial b_P} = -1 + \mu\alpha \frac{1}{b_P} - \mu = 0 \quad (6)$$

$$\frac{\partial \mathcal{L}}{\partial b_R} = \beta \frac{1}{b_R} - 1 - \mu = 0 \quad (7)$$

$$\mu[(1-t)w_P + \alpha \ln(b_P) + t\bar{w} - b_P - b_R - \bar{U}] = 0 \quad (8)$$

From [4]-[6], it follows that:

$$b_P^* = \frac{\alpha\mu}{1+\mu} \quad (9)$$

$$\mu = \frac{b_P}{\alpha - b_P} \quad (10)$$

$$b_R^* = \frac{\beta}{1+\mu} = \frac{(\alpha - b_P)\beta}{\alpha} \quad (11)$$

Substituting (10) into the complementary slackness condition (CSC) (8), which must be binding given that  $\mu > 0$ , we obtain:

$$\alpha \ln(b_P) + b_P \left( \frac{\beta}{\alpha} - 1 \right) + t^{max} \bar{w} - \beta + (1 - t^{max}) w_P = \bar{U} \quad (12)$$

## A.2 Inequality and the Elite Choice of Targeted Goods for the Low-Income Voters

From [11] and [12] and assuming  $\alpha = \beta$  it follows that

$$\alpha \ln b_P^* = \beta - \alpha + \alpha \ln \alpha - t^{max} \bar{w} \lambda \phi \quad (13)$$

Which yields

$$\ln b_P^* = \frac{\beta}{\alpha} - 1 + \ln \alpha - \frac{t^{max} \bar{w} \lambda \phi}{\alpha} \quad (14)$$

From which it follows that

$$b_P^* = e^m, \quad \text{where } m = \frac{\beta}{\alpha} - 1 + \ln(\alpha) - \frac{\tau^{max} \bar{w} \lambda \phi}{\alpha} \quad (15)$$

Which in turn allows us to establish the following comparative statics on the impact of inequality on the level of targeted goods towards citizens in the lower half of the distribution:

1. With respect to the share of income of those above the mean ( $\phi$ ), it follows:

$$\frac{\partial \ln(b_p^*)}{\partial(\phi)} = \frac{-\tau^{max}\bar{w}\lambda}{\alpha} \leq 0 \quad (16)$$

Note as well that:

$$\underbrace{\frac{\partial \ln(b_p^*)}{\partial \phi}}_{\leq 0} = \underbrace{\frac{\partial \ln(b_p^*)}{\partial b_p}}_{> 0} \frac{b_p^*}{\partial \phi}$$

From which it follows that:

$$\frac{\partial b_p^*}{\partial \phi} < 0$$

2. With respect to the share of voters below median income ( $1 - \delta$ ):

Substituting  $\phi = 1 - \frac{w_P(1-\delta)}{\bar{w}}$  into [13] and differentiating with respect to  $(1 - \delta)$  produces:

$$\frac{\partial \ln(b_p^*)}{\partial(1 - \delta)} = \frac{\tau^{max}w_P\lambda}{\alpha} \geq 0 \quad (17)$$

Note as well that:

$$\underbrace{\frac{\partial \ln(b_p^*)}{\partial(1 - \delta)}}_{\geq 0} = \underbrace{\frac{\partial \ln(b_p^*)}{\partial b_p}}_{> 0} \frac{b_p^*}{\partial(1 - \delta)}$$

From which it follows up that:

$$\frac{b_p^*}{\partial(1 - \delta)} > 0$$

## B Descriptives Statistics

### B.1 Descriptive Statistics

Table 1: Descriptive Statistics

<b>Descriptive Statistics</b>	Mean	Standard Dev	Min	Max	N
Turnout Rate 2004	0.87	0.05	0.66	0.99	475
log Public Employees 2002	6.10	0.85	4.01	9.36	476
log Public Employees 2004	6.24	0.86	2.94	9.39	476
log Primary Education Spending 2004	14.60	0.89	12.79	18.26	402
log Change Turnout	0.02	0.06	-0.17	0.27	475
log Change Primary Education Spending	0.32	0.18	-0.13	1.51	335
Incumbent Re-Elected in 2004	0.22	0.42	0.00	1.00	476
Inequality - Gini IPEA	0.57	0.06	0.38	0.78	476
Poverty - IPEA	0.49	0.22	0.03	0.90	476
Audited Mismanagement	1.62	1.16	0.00	8.00	366
log Municipality GDP pc	-1.07	0.84	-2.77	2.77	476
log Population	9.59	0.99	7.15	13.02	476
Share Urban Population	0.60	0.23	0.12	1.00	476
Share Population with Secondary Education	0.23	0.10	0.00	0.65	476
Indicator New Municipality Area	0.24	0.42	0.00	1.00	476
Comarca Judiciary District	1.96	5.60	0.02	72.96	476
Number of vereadores elected	0.55	0.50	0.00	1.00	476
Vereadores pc	14.63	11.81	0.67	75.95	476
Transfers pc	0.00	0.01	0.00	0.05	476
log Total Current Transfers	576.28	316.60	193.72	3383.84	476
Mayor in First Term	15.85	0.84	14.25	19.58	476
Effective Number of Legislative Parties 2000	0.57	0.50	0.00	1.00	476
Winmargin 2000	4.27	1.67	1.00	12.57	476
Same Party Governor 1998	0.18	0.19	0.00	1.00	476
Indicator for participatory budgeting	0.26	0.44	0.00	1.00	476
Ratio Eligible 2004-2000	0.03	0.16	0.00	1.00	476
Incumbent Gender	1.09	0.15	0.34	1.72	475
Incumbent Age	0.95	0.21	0.00	1.00	476
Incumbent Experience	47.34	9.18	26.00	75.00	476
Incumbent Schooling	0.22	0.42	0.00	1.00	476
Incumbent Party Proportion Vereadores	6.08	1.88	2.00	8.00	476
log Mean Budget 2004	0.31	0.16	0.00	1.00	476
Municipality Median Income 2004	15.80	0.89	14.25	19.51	422
Indicator Party Previously Re-Elected	91.70	55.11	14.56	526.92	469
	0.35	0.48	0.00	1.00	469

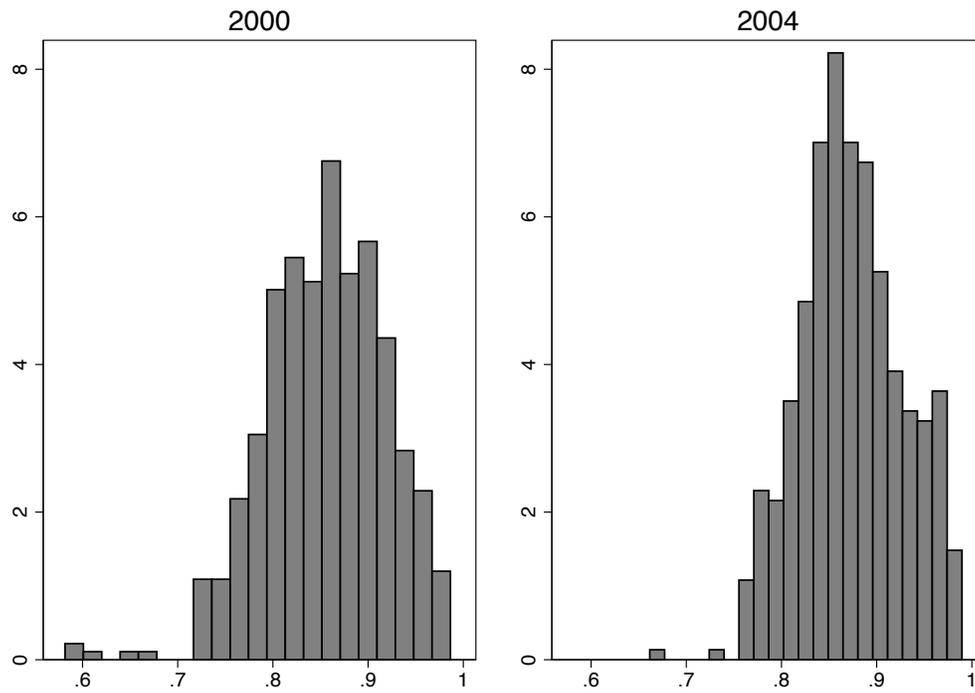
## B.2 Balance Table

<b>Balance Table</b>	Non Exposed	Exposed	Difference	P-Value
Turnout Rate 2004	0.87	0.87	-0.00	0.78
log Public Employees 2002	6.29	6.06	0.24	0.05
log Public Employees 2004	6.48	6.16	0.31	0.01
log Primary Education Spending 2004	14.79	14.50	0.29	0.03
log Change Turnout	0.03	0.02	0.01	0.11
log Change Primary Education Spending	0.30	0.33	-0.03	0.36
Incumbent Re-Elected in 2004	0.42	0.39	0.03	0.66
Inequality - Gini IPEA	0.58	0.57	0.01	0.31
Poverty - IPEA	0.49	0.47	0.02	0.49
Audited Mismanagement	1.84	1.63	0.21	0.32
log Municipality GDP pc	-0.94	-1.06	0.12	0.28
log Population	9.83	9.54	0.29	0.03
Share Urban Population	0.63	0.62	0.01	0.73
Share Population with Secondary Education	0.24	0.25	-0.00	0.89
Indicator New Municipality Area	0.19	0.22	-0.03	0.58
Comarca Judiciary District	2.14	2.25	-0.12	0.90
Number of vereadores elected	0.61	0.55	0.06	0.40
Vereadores pc	11.60	15.20	-3.60	0.02
Transfers pc	0.00	0.00	-0.00	0.09
log Total Current Transfers	513.39	570.30	-56.91	0.14
Mayor in First Term	16.01	15.79	0.22	0.07
Effective Number of Legislative Parties 2000	0.99	0.99	-0.01	0.43
Winmargin 2000	4.75	4.49	0.27	0.30
Same Party Governor 1998	0.15	0.14	0.01	0.74
Indicator for participatory budgeting	0.20	0.24	-0.04	0.53
Ratio Eligible 2004-2000	0.01	0.03	-0.02	0.49
Incumbent Gender	1.07	1.10	-0.03	0.13
Incumbent Age	0.97	0.94	0.03	0.32
Incumbent Experience	46.78	48.26	-1.47	0.25
Incumbent Schooling	0.28	0.31	-0.03	0.59
Incumbent Party Proportion Vereadores	6.28	6.00	0.27	0.29
log Mean Budget 2004	0.25	0.28	-0.03	0.16
Municipality Median Income 2004	15.95	15.74	0.20	0.13
Indicator Party Previously Re-Elected	94.11	96.57	-2.46	0.74
	0.10	0.09	0.01	0.79

# C Descriptive Figures

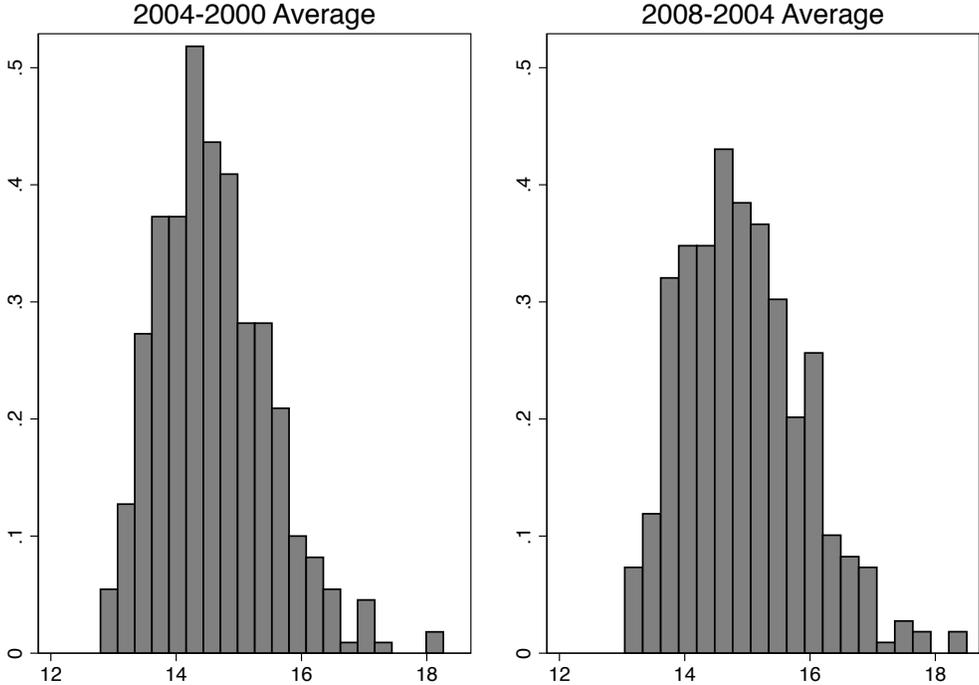
## C.1 Turnout Rates at the Local Level

Figure 1: Turnout Rates for Municipal Elections in Brazil, 2000 and 2004



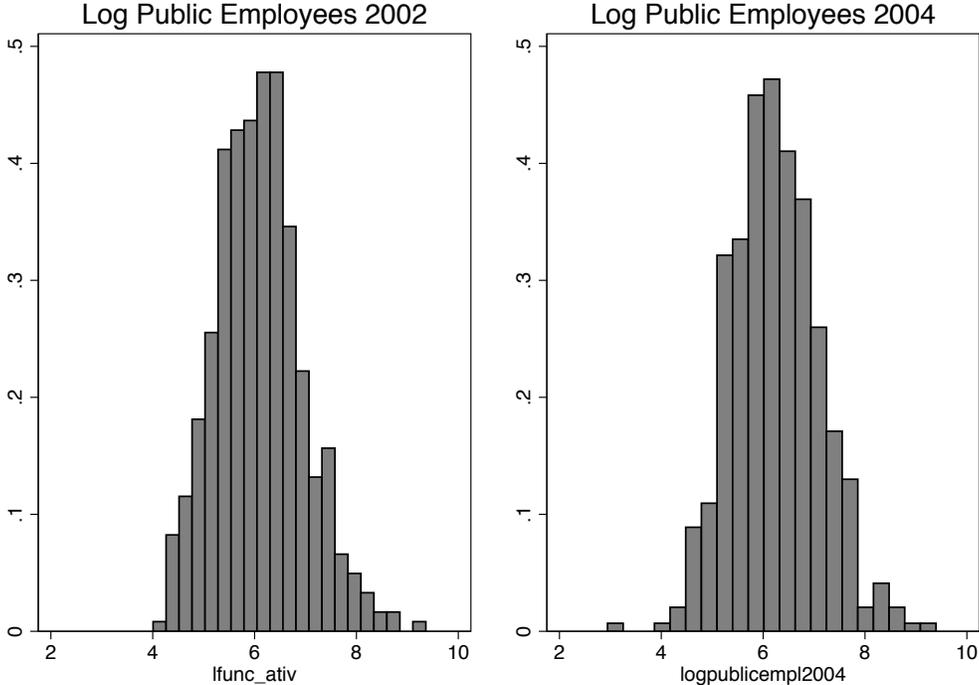
## C.2 Primary Education Spending at the Local Level

Figure 2: Primary Education Spending Levels across Municipalities



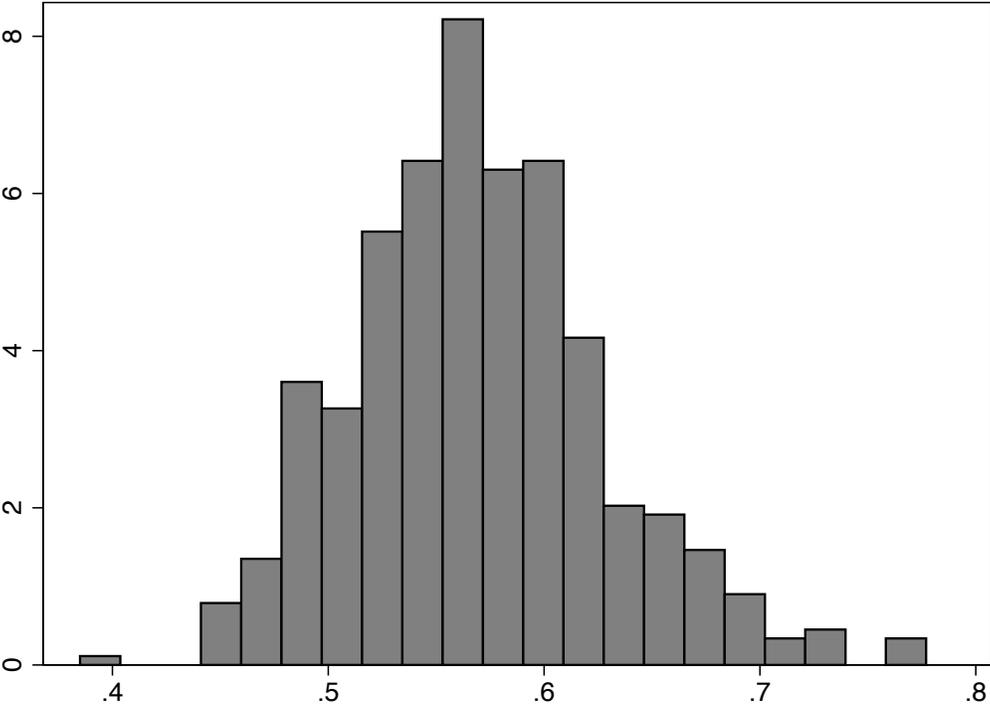
### C.3 Public Employees at the Local Level

Figure 3: Number of Public Employees across Municipalities, 2004 and 2004



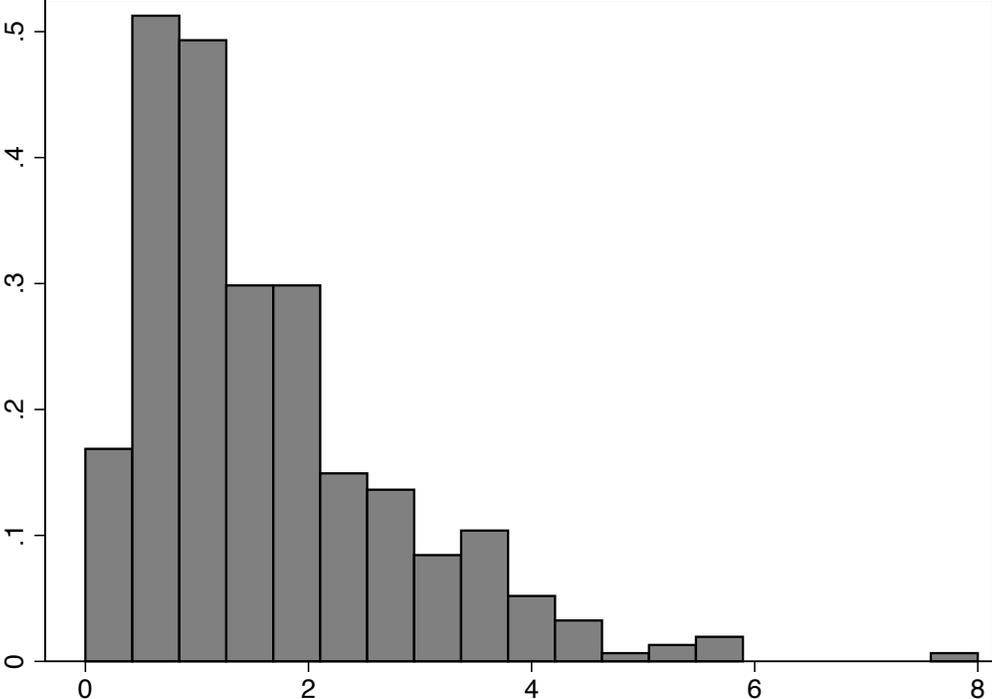
### C.4 Gini at the Local Level

Figure 4: Gini Measure across Municipalities



# C.5 Audited Mismanagement at the Local Level

Figure 5: Audited Mismanagement across Municipalities



## D Robustness Checks for Linear Interactions

Figure 6: Effects of Mismanagement on Turnout: Bin Estimates

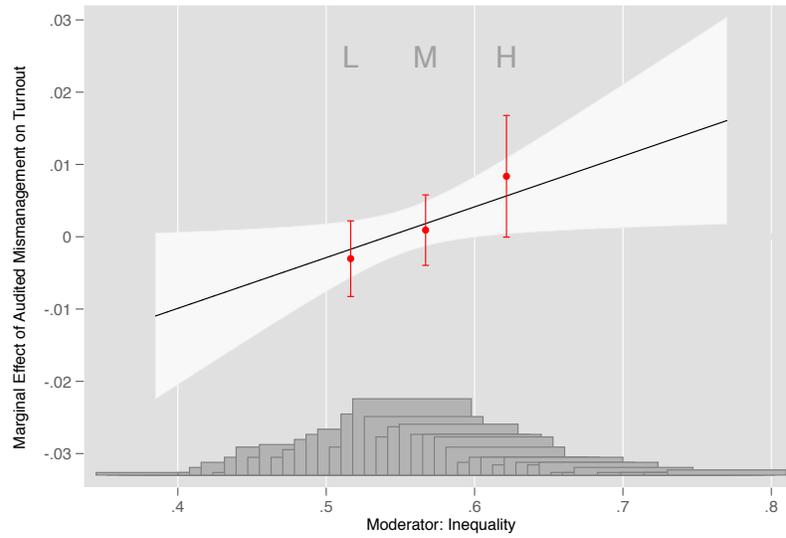


Figure 7: Effects of Mismanagement on Turnout: Kernel Estimates

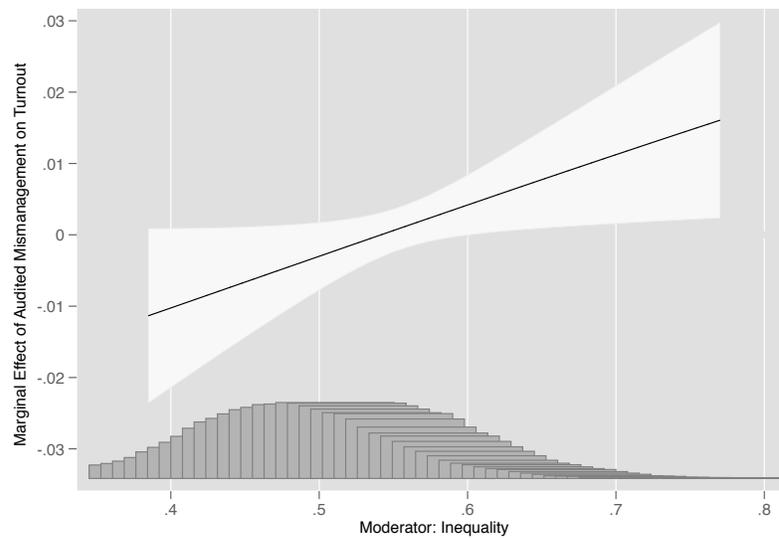


Figure 8: Effects of Mismanagement on Primary Education Spending: Bin Estimates

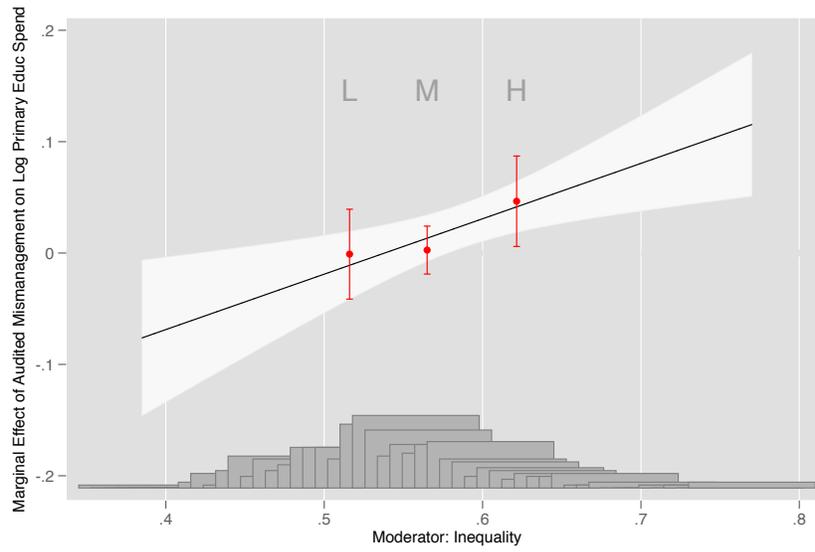


Figure 9: Effects of Mismanagement on Primary Education Spending: Kernel Estimates

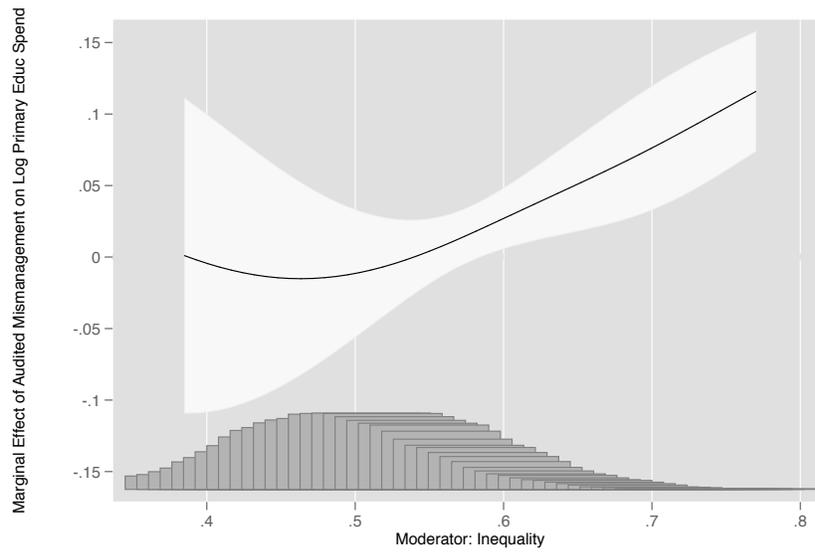


Figure 10: Effects of Mismanagement on Public Employees: Bin Estimates

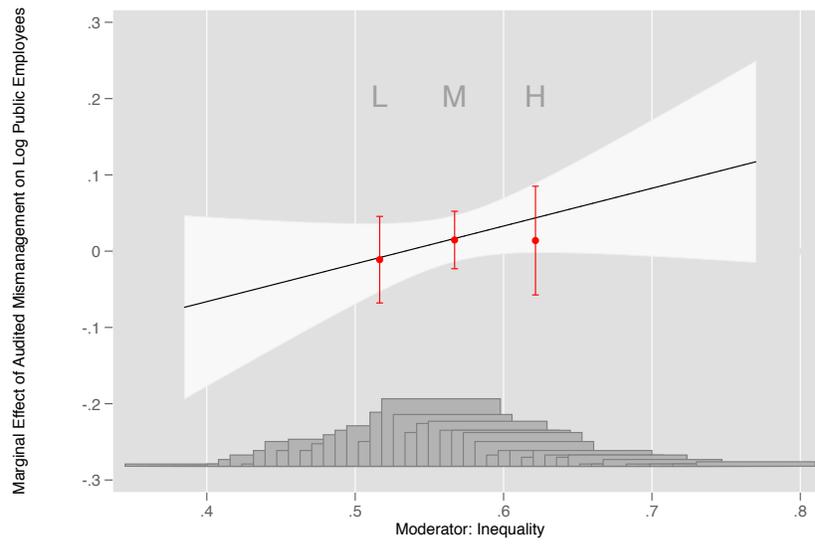
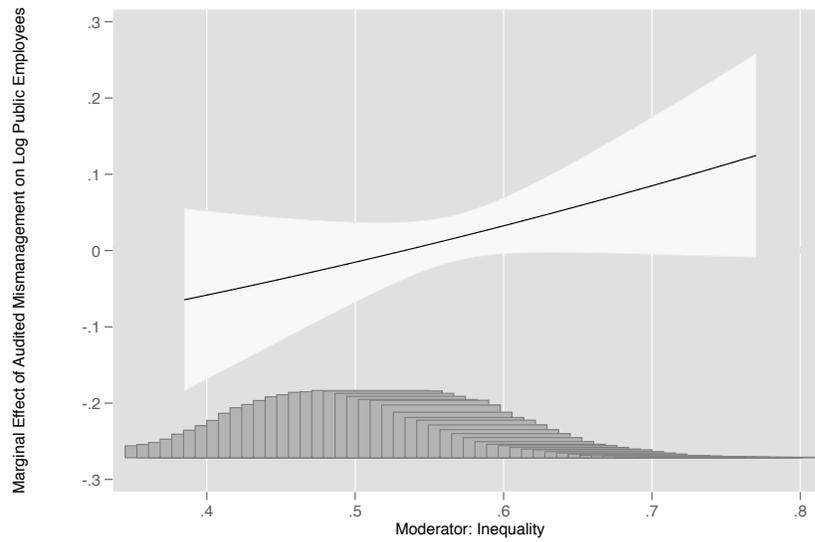


Figure 11: Effects of Mismanagement on Public Employees: Kernel Estimates



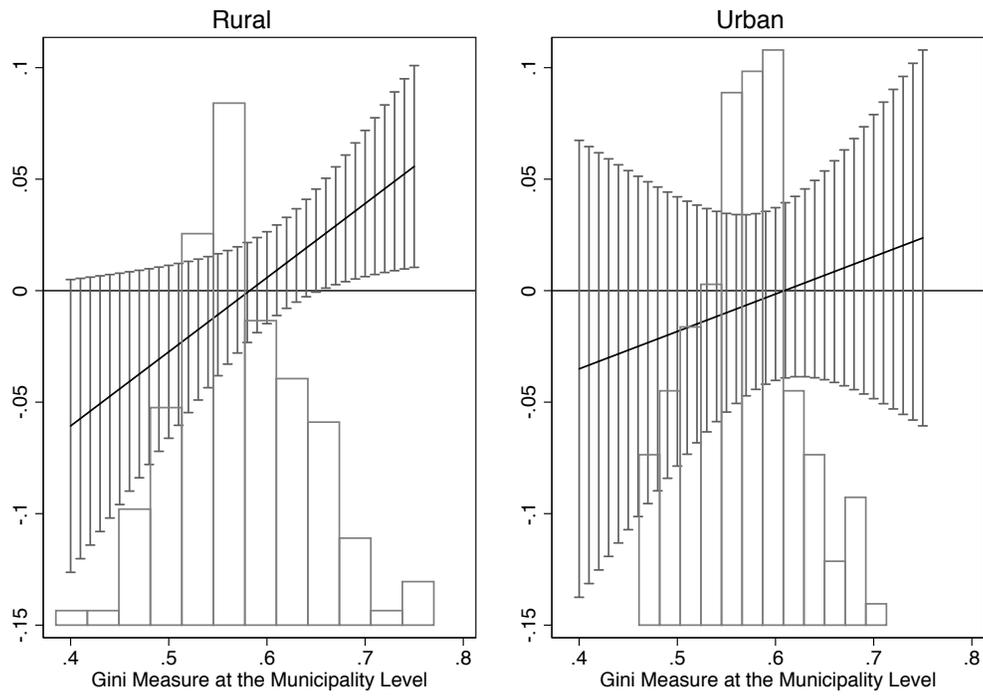
## E Heterogeneous Effects under the Status Quo

### E.1 Primary Education Spending: Rural versus Urban

Log Primary Education Spending 2004	All Incumbents					
	Rural Areas			Urban Areas		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Inequality	-0.45 (0.31)	-0.83* (0.43)	-0.74* (0.39)	-0.08 (0.52)	-0.08 (0.54)	-0.13 (0.61)
Mismanagement	-0.21** (0.09)	-0.21** (0.10)	-0.19* (0.11)	-0.11 (0.18)	-0.11 (0.17)	-0.10 (0.17)
Ineq X Mismanagement	0.35** (0.14)	0.35** (0.16)	0.33* (0.17)	0.18 (0.29)	0.18 (0.28)	0.17 (0.28)
Constant	-3.07*** (0.75)	-2.58*** (0.64)	-2.76*** (0.82)	-0.83 (0.60)	-0.82 (0.58)	-0.89 (0.67)
Mean Budget Control	Yes	Yes	Yes	Yes	Yes	Yes
Median Income and Re-Election	No	Yes	Yes	No	Yes	Yes
Incumbent Characteristics Controls	No	No	Yes	No	No	Yes
Regional State FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.95	0.96	0.96	0.98	0.98	0.98
N	150	150	150	156	156	156

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 12: Primary Education Spending in the Status Quo: Rural versus Urban Areas

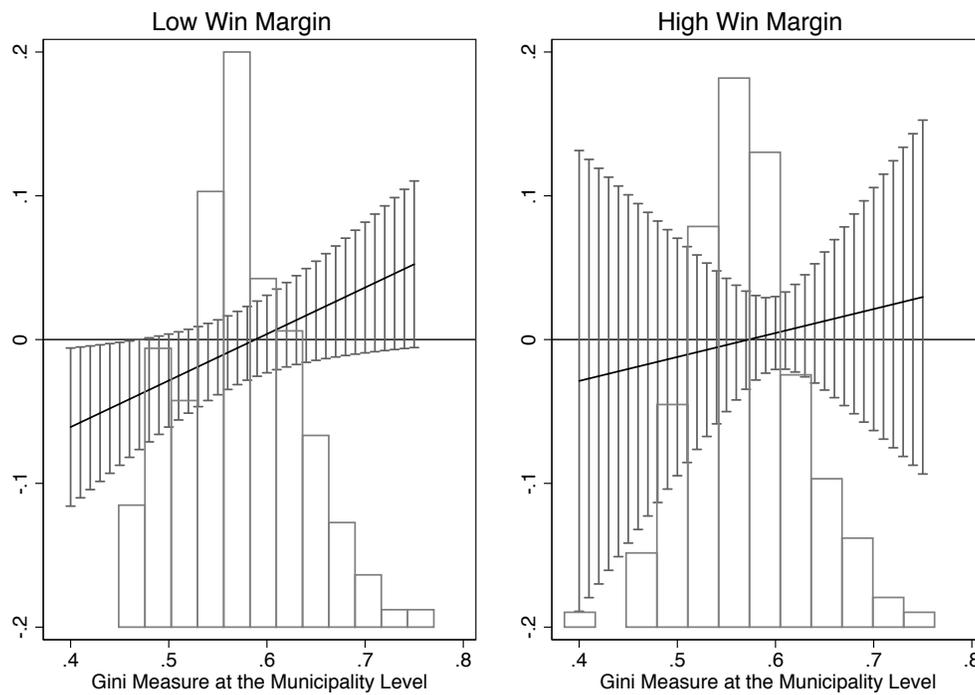


## E.2 Primary Education Spending: Low versus High Win Margin

Log Primary Education Spending 2004	All Incumbents					
	Low Win-margin			High Win-margin		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Inequality	-0.24 (0.51)	-0.32 (0.49)	-0.27 (0.50)	-0.43 (0.67)	-0.64 (0.73)	-0.56 (0.77)
Mismanagement	-0.24** (0.10)	-0.19* (0.10)	-0.19* (0.10)	-0.10 (0.28)	-0.10 (0.28)	-0.10 (0.27)
Ineq X Mismanagement	0.40** (0.18)	0.32* (0.17)	0.33* (0.17)	0.17 (0.47)	0.17 (0.47)	0.17 (0.46)
Constant	-0.76 (0.58)	-1.04 (0.65)	-1.56* (0.82)	-0.90* (0.47)	-1.06** (0.49)	-0.98 (0.61)
Mean Budget Control	Yes	Yes	Yes	Yes	Yes	Yes
Median Income and Re-Election	No	Yes	Yes	No	Yes	Yes
Incumbent Characteristics Controls	No	No	Yes	No	No	Yes
Regional State FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.97	0.98	0.98	0.97	0.97	0.97
N	153	153	153	153	153	153

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 13: Primary Education Spending in the Status Quo: Low versus High Win Margin

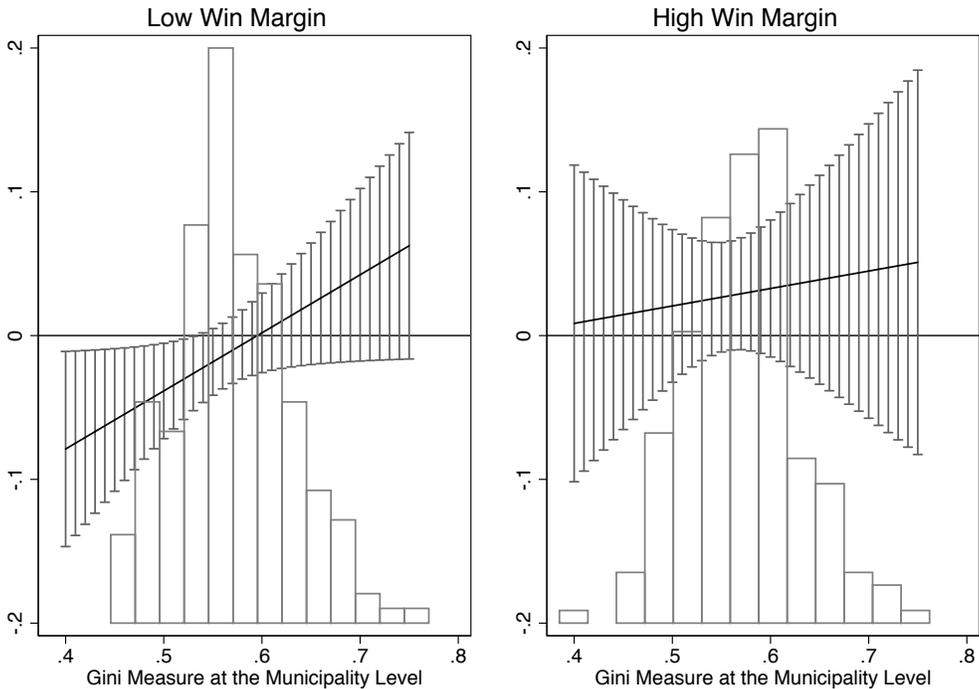


### E.3 Public Employees: Low versus High Winmargin

Log Public Employees 2004	All Incumbents			
	Low Win-margin (1) OLS	Low Win-margin (2) OLS	Low Win-margin (3) OLS	Low Win-margin (4) OLS
Inequality	0.10 (0.46)	-0.35 (0.60)	0.53 (0.60)	0.06 (0.58)
Mismanagement	-0.20* (0.11)	-0.24* (0.13)	0.15 (0.19)	-0.04 (0.22)
Ineq X Mismanagement	0.34* (0.20)	0.40* (0.23)	-0.23 (0.34)	0.12 (0.39)
Constant	-1.51 (0.97)	-2.10* (1.06)	-4.45*** (1.20)	-4.74*** (1.15)
Lag Public Employees	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes
Lottery FEs	Yes	Yes	Yes	Yes
Regional State FEs	No	Yes	No	Yes
R-squared	0.95	0.96	0.90	0.95
N	183	183	183	183

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 14: Public Employees in the Status Quo: Low versus High WinMargin

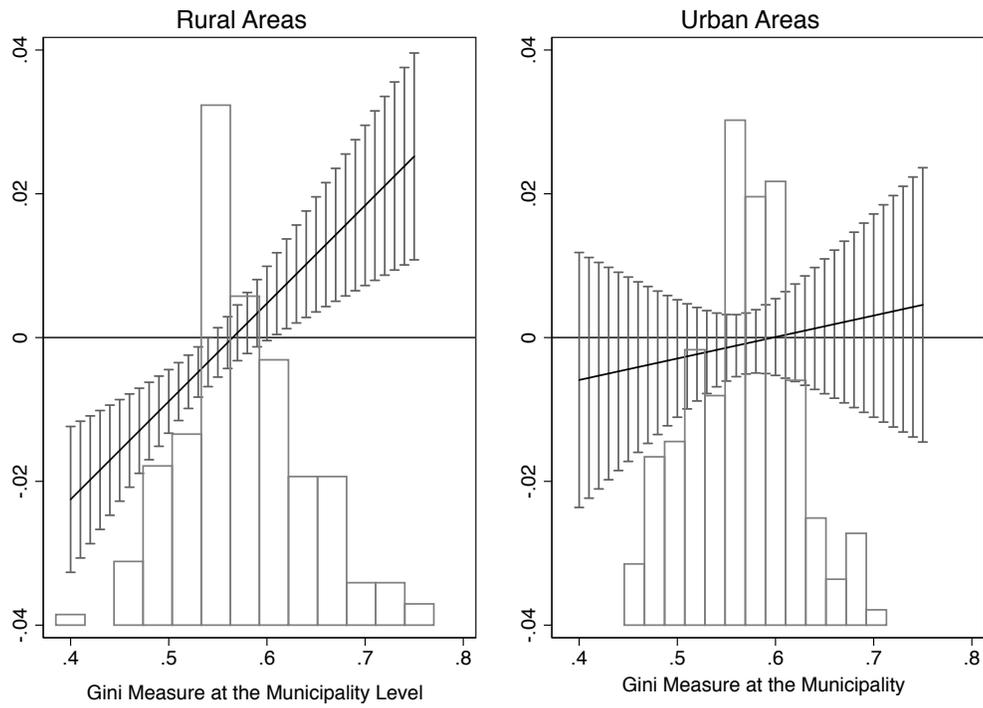


## E.4 Turnout: Rural versus Urban

Turnout Rates in 2004	All Incumbents					
		Rural Areas		Urban Areas		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.23*	-0.21	-0.22	-0.32*	-0.33*	-0.32*
	(0.13)	(0.15)	(0.14)	(0.17)	(0.17)	(0.17)
Mismanagement	-0.08***	-0.07***	-0.08***	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)
Ineq X Mismanagement	0.14***	0.13***	0.14***	0.03	0.03	0.03
	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
Constant	1.37***	1.38***	1.39***	1.17***	1.18***	1.13***
	(0.14)	(0.13)	(0.25)	(0.17)	(0.16)	(0.19)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	No	Yes	Yes
Fiscal Transfers Controls	No	No	Yes	No	No	Yes
Regional State FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.59	0.60	0.60	0.57	0.57	0.58
N	183	183	183	183	183	183

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

Figure 15: Turnout Rates in the Status Quo: Rural versus Urban Areas



## E.5 Incentives and Capacity: First Term Incumbents

Log Public Employees 2004	All Incumbents			First Term Incumbents		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.27 (0.56)	-0.53 (0.56)	-0.60 (0.60)	-1.03 (0.84)	-1.34 (0.87)	-1.53* (0.80)
Mismanagement	-0.20 (0.14)	-0.23* (0.13)	-0.25* (0.13)	-0.43** (0.19)	-0.43** (0.17)	-0.43** (0.20)
Ineq X Mismanagement	0.34 (0.26)	0.41* (0.23)	0.45* (0.24)	0.76** (0.35)	0.73** (0.31)	0.75** (0.36)
Winmargin	-1.13 (1.31)	-0.99 (1.39)	-1.36 (1.33)	-5.07** (2.07)	-5.91** (2.15)	-7.27*** (2.21)
Winmargin X Inequality	1.97 (2.55)	1.69 (2.67)	2.47 (2.58)	9.53** (3.92)	10.50** (4.21)	13.24*** (4.30)
Winmargin X Mismanagement	0.71 (0.60)	0.67 (0.64)	0.86 (0.60)	2.01* (1.17)	1.90 (1.21)	2.38* (1.21)
Winmargin X Ineq X Mism	-1.20 (1.17)	-1.08 (1.22)	-1.50 (1.16)	-3.75* (2.13)	-3.25 (2.24)	-4.30* (2.21)
Constant	-1.99* (0.97)	-2.56** (1.13)	-2.97*** (1.00)	-1.17 (1.16)	-2.21* (1.15)	-2.91** (1.14)
Lag Public Employees	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	Yes	Yes	No	Yes	Yes
Party FEs	No	No	Yes	No	No	Yes
R-squared	0.92	0.93	0.94	0.89	0.93	0.96
N	366	366	366	204	204	204

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## F Robustness Tests for the Capacity Shock Models

### F.1 Turnout Change Excluding Extreme Mismanagement Values

Turnout Change	Fist Term Incumbents		
	(1) Complete OLS	(2) Excluding 1% OLS	(3) Excluding 25% OLS
Inequality	-0.19 (0.15)	-0.18 (0.15)	-0.24 (0.16)
Mismanagement	-0.04 (0.04)	-0.04 (0.04)	-0.04 (0.05)
Ineq X Mismanagement	0.07 (0.07)	0.06 (0.07)	0.01 (0.08)
Exposed	-0.18 (0.11)	-0.19 (0.12)	-0.40*** (0.11)
Exp X Inequality	0.33* (0.19)	0.32 (0.20)	0.60*** (0.18)
Exp X Mismanagement	0.10* (0.05)	0.10* (0.06)	0.20** (0.08)
Exp X Inequality X Mismanagement	-0.19* (0.09)	-0.18* (0.10)	-0.31** (0.12)
Constant	0.09 (0.14)	0.10 (0.14)	0.18 (0.17)
Municipality Controls	Yes	Yes	Yes
Electoral and Fiscal Controls	Yes	Yes	Yes
Incumbent Characteristics Controls	Yes	Yes	Yes
Lottery FEs	Yes	Yes	Yes
Regional State FEs	Yes	Yes	Yes
R-squared	0.70	0.70	0.76
N	203	200	152

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## F.2 Turnout Change: Public Employment as Capacity Measure

Turnout Change	First Term Incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Inequality	-0.31 (0.43)	-0.41 (0.46)	-0.72 (0.49)	-0.80 (0.55)	-1.25* (0.66)	-1.32 (0.79)
Log Public Employees 2002	-0.03 (0.04)	-0.04 (0.04)	-0.07 (0.04)	-0.08 (0.05)	-0.12* (0.06)	-0.13* (0.07)
Ineq X Log Public Employees	0.06 (0.07)	0.07 (0.07)	0.11 (0.08)	0.12 (0.09)	0.20* (0.11)	0.21 (0.13)
Exposed	-0.48 (0.29)	-0.53* (0.30)	-0.68* (0.37)	-0.72* (0.37)	-0.96** (0.44)	-1.01** (0.49)
Exp X Inequality	0.69 (0.52)	0.79 (0.55)	1.14* (0.67)	1.21* (0.67)	1.63** (0.77)	1.72* (0.86)
Exp X Log Public Employees	0.08 (0.05)	0.09* (0.05)	0.11* (0.06)	0.11* (0.06)	0.15** (0.07)	0.16* (0.08)
Exp X Ineq X Log Public Employees	-0.12 (0.09)	-0.13 (0.09)	-0.19 (0.11)	-0.20* (0.11)	-0.27** (0.13)	-0.28* (0.14)
Constant	0.46* (0.24)	0.50* (0.25)	0.50 (0.31)	0.52 (0.35)	0.85* (0.44)	0.87 (0.52)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent Controls	No	Yes	No	Yes	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	Yes	Yes	Yes	Yes
PMDB Mayors Excluded	No	No	No	No	Yes	Yes
R-squared	0.52	0.53	0.65	0.66	0.59	0.60
N	266	266	266	266	213	213

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

### F.3 Turnout Change: Re-Elected versus Non Re-Elected

Turnout Change	All First Term (1) OLS	First Term (2) OLS	First Term Re-Elected (3) OLS	Frst Term Not Re-Elected (4) OLS	(5) OLS	(6) OLS
Inequality	-0.13 (0.11)	-0.19 (0.15)	-0.42 (1.16)	-0.60 (1.32)	-0.24 (0.16)	-0.37* (0.21)
Mismanagement	-0.03 (0.03)	-0.04 (0.04)	-0.10 (0.19)	-0.15 (0.20)	-0.08* (0.04)	-0.13** (0.06)
Ineq X Mismanagement	0.04 (0.05)	0.07 (0.07)	0.16 (0.34)	0.25 (0.36)	0.12* (0.07)	0.21** (0.10)
Exposed	-0.15 (0.10)	-0.18 (0.11)	-0.22 (0.69)	-0.33 (0.68)	-0.22 (0.18)	-0.29 (0.17)
Exp X Inequality	0.27 (0.17)	0.33* (0.19)	0.37 (1.21)	0.58 (1.13)	0.39 (0.29)	0.51* (0.27)
Exp X Mismanagement	0.08* (0.05)	0.10* (0.05)	0.11 (0.21)	0.15 (0.22)	0.12 (0.07)	0.18** (0.08)
Exp X Ineq X Mismanagement	-0.16* (0.08)	-0.19* (0.09)	-0.20 (0.38)	-0.25 (0.38)	-0.23* (0.13)	-0.32** (0.14)
Constant	0.08 (0.14)	0.09 (0.14)	-0.05 (0.58)	-0.30 (0.95)	0.03 (0.32)	0.13 (0.30)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent Controls	No	Yes	No	Yes	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.68	0.70	0.82	0.85	0.72	0.75
N	203	203	81	81	122	122

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## F.4 Turnout Change: Alternative Exposure Measure

Turnout Change	All Incumbents					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.19 (0.14)	-0.20 (0.15)	-0.14 (0.14)	-0.14 (0.14)	-0.12 (0.14)	-0.14 (0.15)
Mismanagement	-0.05 (0.03)	-0.05 (0.04)	-0.05 (0.04)	-0.04 (0.04)	-0.05 (0.04)	-0.05 (0.04)
Ineq X Mismanagement	0.10* (0.06)	0.09 (0.06)	0.08 (0.06)	0.07 (0.06)	0.08 (0.07)	0.08 (0.08)
Alt Exposed	-0.09 (0.09)	-0.09 (0.09)	-0.13 (0.08)	-0.12 (0.08)	-0.11 (0.09)	-0.12 (0.09)
Alt Exp X Ineq	0.17 (0.16)	0.16 (0.16)	0.22 (0.15)	0.21 (0.16)	0.21 (0.18)	0.21 (0.18)
Alt Exp X Mismanagement	0.07* (0.04)	0.06 (0.04)	0.07 (0.05)	0.07 (0.05)	0.09 (0.05)	0.09 (0.06)
Alt Exp X Ineq X Mismanagement	-0.13* (0.07)	-0.11 (0.07)	-0.14* (0.08)	-0.13 (0.08)	-0.17* (0.10)	-0.17* (0.10)
Constant	0.43*** (0.15)	0.41** (0.15)	0.20 (0.13)	0.20 (0.13)	0.19 (0.15)	0.20 (0.15)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent Controls	No	Yes	No	Yes	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	Yes	Yes	Yes	Yes
PMDB Mayors Excluded	No	No	No	No	Yes	Yes
R-squared	0.38	0.39	0.53	0.54	0.54	0.55
N	366	366	366	366	291	291

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## G Heterogeneous Effects of the Capacity Shock

### G.1 Turnout Change: Urban versus Rural Areas

Turnout Change	Fist Term Incumbents					
	Urban Localities			Rural Localities		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Inequality	0.08	0.06	-0.05	-0.28	-0.16	-0.25
	(0.11)	(0.11)	(0.12)	(0.47)	(0.35)	(0.36)
Mismanagement	-0.04	-0.04	-0.06	-0.04	-0.02	-0.04
	(0.04)	(0.04)	(0.04)	(0.10)	(0.08)	(0.08)
Ineq X Mismanagement	0.04	0.05	0.07	0.08	0.06	0.09
	(0.07)	(0.07)	(0.07)	(0.17)	(0.13)	(0.13)
Exposed	-0.04	-0.06	-0.10	-0.28	-0.20	-0.27
	(0.10)	(0.09)	(0.09)	(0.31)	(0.25)	(0.23)
Exp X Inequality	0.01	0.06	0.11	0.55	0.44	0.57
	(0.18)	(0.17)	(0.15)	(0.52)	(0.41)	(0.40)
Exp X Mismanagement	0.07	0.08	0.09	0.17	0.15	0.18
	(0.06)	(0.05)	(0.06)	(0.13)	(0.11)	(0.10)
Exp X Ineq X Mismanagement	-0.12	-0.13	-0.14	-0.35	-0.31*	-0.35*
	(0.11)	(0.10)	(0.11)	(0.21)	(0.18)	(0.18)
Constant	0.10	0.16	0.21	-0.11	-0.13	-0.09
	(0.17)	(0.16)	(0.18)	(0.35)	(0.27)	(0.30)
Municipality and Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	No	Yes	Yes
Incumbent's Controls	No	No	Yes	No	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	No	No	No	No
R-squared	0.57	0.57	0.59	0.67	0.70	0.72
N	101	101	101	102	102	102

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## G.2 Turnout Change: Low versus High Education

Turnout Change	Fist Term Incumbents					
	High Education Localities			Low Education Localities		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.32 (0.21)	-0.31 (0.21)	-0.36** (0.16)	-0.59* (0.34)	-0.50 (0.32)	-0.44 (0.44)
Mismanagement	-0.19* (0.09)	-0.17* (0.10)	-0.16** (0.08)	-0.10 (0.07)	-0.10 (0.06)	-0.09 (0.09)
Ineq X Mismanagement	0.29* (0.14)	0.26* (0.15)	0.25** (0.12)	0.19 (0.12)	0.17 (0.12)	0.16 (0.16)
Exposed	-0.24 (0.14)	-0.24 (0.14)	-0.28** (0.12)	-0.36* (0.19)	-0.32 (0.18)	-0.30 (0.27)
Exp X Inequality	0.35 (0.23)	0.36 (0.24)	0.40** (0.19)	0.64* (0.34)	0.58* (0.33)	0.55 (0.49)
Exp X Mismanagement	0.19* (0.11)	0.18 (0.11)	0.17* (0.10)	0.18** (0.08)	0.17** (0.07)	0.17* (0.10)
Exp X Ineq X Mismanagement	-0.30* (0.17)	-0.28 (0.18)	-0.26 (0.16)	-0.35** (0.13)	-0.32** (0.13)	-0.32* (0.17)
Constant	0.18 (0.18)	0.18 (0.19)	0.22 (0.17)	0.11 (0.32)	0.15 (0.32)	0.18 (0.42)
Municipality and Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	No	Yes	Yes
Incumbent's Controls	No	No	Yes	No	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	No	No	No	No
R-squared	0.62	0.63	0.65	0.64	0.67	0.68
N	101	101	101	102	102	102

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

### G.3 Turnout Change: Radio versus Non-Radio Localities

Turnout Change	Fist Term Incumbents					
	No Radio Station			Radio Station		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	-0.24 (0.36)	-0.28 (0.34)	-0.28 (0.39)	-0.18 (0.22)	-0.12 (0.27)	-0.30 (0.28)
Mismanagement	-0.05 (0.08)	-0.08 (0.07)	-0.08 (0.07)	-0.15* (0.08)	-0.14* (0.08)	-0.18** (0.08)
Ineq X Mismanagement	0.10 (0.14)	0.14 (0.12)	0.14 (0.14)	0.23* (0.11)	0.22* (0.12)	0.28** (0.13)
Exposed	-0.20 (0.21)	-0.20 (0.22)	-0.20 (0.25)	-0.37* (0.19)	-0.36 (0.21)	-0.42* (0.23)
Exp X Inequality	0.38 (0.40)	0.38 (0.40)	0.38 (0.44)	0.57* (0.32)	0.56 (0.34)	0.68* (0.38)
Exp X Mismanagement	0.12 (0.09)	0.13 (0.08)	0.13 (0.09)	0.21* (0.10)	0.22* (0.11)	0.23** (0.11)
Exp X Ineq X Mismanagement	-0.23 (0.16)	-0.26 (0.15)	-0.26 (0.16)	-0.36** (0.17)	-0.36* (0.18)	-0.37* (0.18)
Constant	0.15 (0.29)	0.27 (0.27)	0.22 (0.26)	0.25 (0.26)	0.22 (0.28)	0.19 (0.20)
Municipality and Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	No	Yes	Yes
Incumbent's Controls	No	No	Yes	No	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	No	No	No	No
R-squared	0.58	0.61	0.62	0.65	0.68	0.80
N	148	148	148	55	55	55

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## G.4 Turnout Change: Low versus High Win Margin

Turnout Change	Fist Term Incumbents					
	Low Win-margin			High Win-margin		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Inequality	0.19 (0.22)	0.18 (0.22)	0.10 (0.31)	-0.64 (0.44)	-1.13** (0.46)	-0.99* (0.50)
Mismanagement	0.07 (0.05)	0.06 (0.05)	0.04 (0.07)	-0.21* (0.11)	-0.28** (0.11)	-0.26** (0.10)
Ineq X Mismanagement	-0.11 (0.08)	-0.10 (0.08)	-0.06 (0.12)	0.37* (0.19)	0.49** (0.19)	0.45** (0.18)
Exposed	0.06 (0.14)	0.04 (0.13)	0.02 (0.16)	-0.44* (0.25)	-0.68*** (0.23)	-0.60** (0.24)
Exp X Inequality	-0.11 (0.24)	-0.05 (0.23)	-0.01 (0.28)	0.83* (0.47)	1.30*** (0.45)	1.13** (0.48)
Exp X Mismanagement	-0.01 (0.07)	-0.01 (0.06)	0.02 (0.07)	0.20 (0.13)	0.29** (0.11)	0.25** (0.10)
Exp X Ineq X Mism	-0.02 (0.12)	-0.03 (0.10)	-0.07 (0.12)	-0.36 (0.23)	-0.51** (0.18)	-0.45** (0.17)
Constant	0.03 (0.17)	0.01 (0.17)	0.04 (0.20)	0.38 (0.32)	0.97** (0.37)	0.84* (0.44)
Municipality and Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	No	Yes	Yes
Incumbent's Controls	No	No	Yes	No	No	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	No	No	No	No
R-squared	0.49	0.52	0.55	0.74	0.78	0.79
N	127	127	127	76	76	76

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.

## H Further Checks: Exclusion of Second Term and Re-Election Models

### H.1 Motivation for the Exclusion of Second Term Incumbents

Audited Mismanagement	All Incumbents			
	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Inequality	1.88 (1.28)	1.81 (1.28)	1.85 (1.23)	1.86 (1.20)
Incumbent in First Term	3.19*** (1.14)	3.12*** (1.12)	2.92** (1.14)	2.90** (1.17)
Ineq X Incumbent in First Term	-5.39** (2.03)	-5.34** (2.00)	-5.04** (2.04)	-5.05** (2.09)
Constant	0.52 (2.81)	0.29 (3.07)	0.24 (3.02)	1.31 (2.99)
Municipality Controls	Yes	Yes	Yes	Yes
Electoral Controls	No	Yes	Yes	Yes
Incumbent's Controls	No	No	Yes	Yes
Lottery FEs	Yes	Yes	Yes	Yes
Regional State FEs	Yes	Yes	Yes	Yes
Party FEs	No	No	No	Yes
R-squared	0.42	0.43	0.43	0.46
N	366	366	366	366

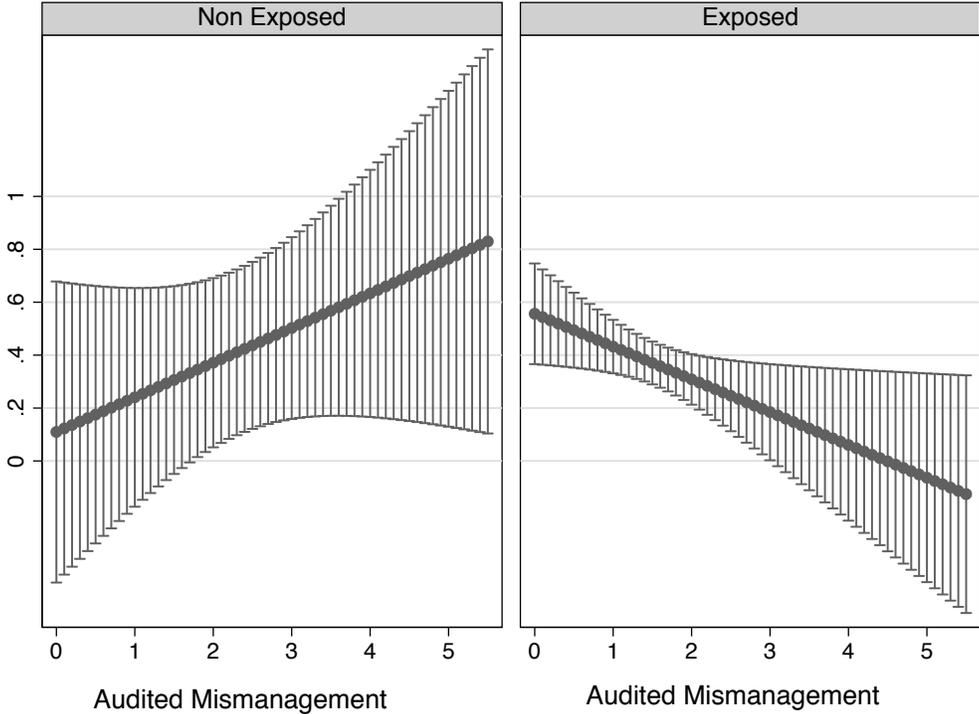
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors are clustered at the State-level.

## H.2 Heterogeneous Effects of Re-Election

Incumbent Re-Election in 2004	Fist Term Incumbents		
	(1)	(2)	(3)
	OLS	OLS	OLS
Inequality	-1.48 (2.23)	-2.53 (2.45)	-4.07 (3.23)
Mismanagement	-0.87 (0.53)	-1.00* (0.57)	-1.24 (0.77)
Inequality X Audited Mismanagement	1.46 (0.89)	1.72* (0.95)	2.11 (1.30)
Exposed	-1.32 (1.36)	-2.04 (1.50)	-3.06 (1.86)
Exposed X Inequality	2.36 (2.42)	3.70 (2.65)	5.39 (3.26)
Exposed X Audited Mismanagement	1.14* (0.58)	1.35** (0.61)	1.71** (0.79)
Exposed X Inequality X Audited Mismanagement	-2.00* (1.02)	-2.40** (1.05)	-3.02** (1.32)
Constant	2.26 (2.13)	3.58 (2.33)	4.90* (2.81)
Municipality Controls	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes
Fiscal Controls	Yes	Yes	Yes
Incumbent Controls	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes
Lottery FEs	No	Yes	Yes
Regional State FEs	No	No	Yes
R-squared	0.38	0.42	0.50
N	199	199	199

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.

Figure 16: Re-Election Probability, Mismanagement and Exposure under High Inequality

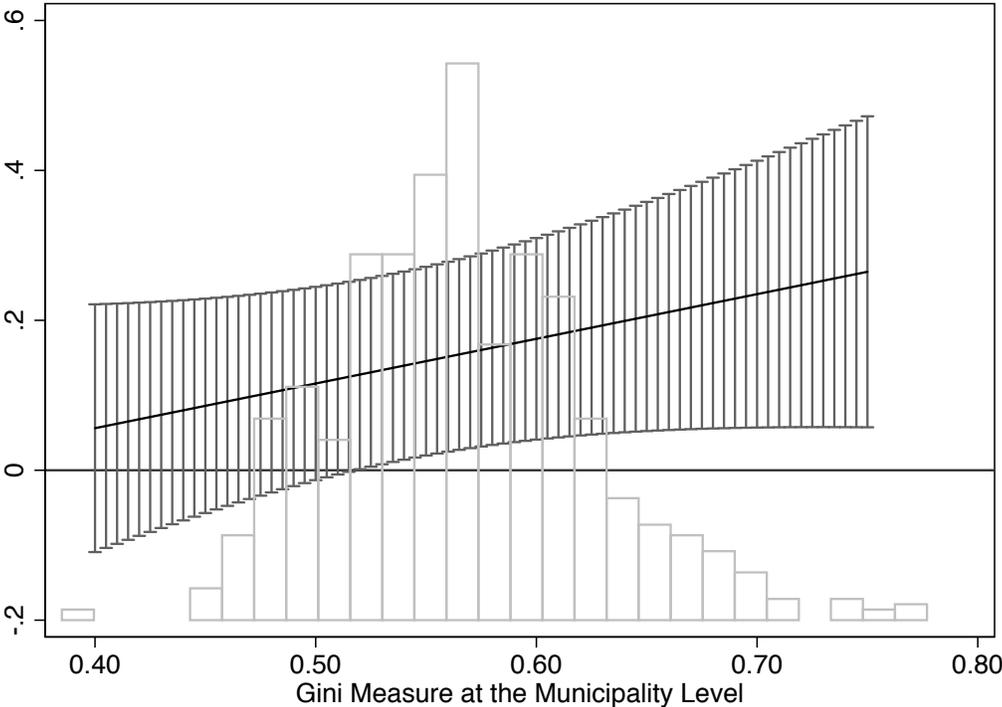


### H.3 Re-Election with Complete Brazilian Sample

Incumbent Party Re-Election, Complete Dataset	(1)	(2)	(3)
	OLS	OLS	OLS
Log Mean Budget	0.03 (0.09)	0.06 (0.10)	0.06 (0.10)
Log Educ Spending	-1.10*** (0.21)	0.05 (0.32)	3.91** (1.63)
Log Educ Spending X Log Median Income	0.26*** (0.05)	0.02 (0.07)	-0.89** (0.36)
Inequality X Log Educ Spending			-6.82** (2.68)
Inequality X Log Educ Spending X Log Median Income			1.62*** (0.61)
Constant	-0.90 (1.12)	-1.77 (1.22)	-1.75 (1.25)
Mayor Controls	Yes	Yes	Yes
Party in Power in Previous Term	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Party Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Year X Region Fixed Effects	No	Yes	Yes
R-squared	0.69	0.70	0.70
N	9153	9153	9153

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Figure 17: Marginal Effects of Primary Education Spending on Re-Election Probability Across Full Set of Brazilian Municipalities, for Medium Income Levels



# I Models Employing Poverty Measures

## I.1 Poverty and Turnout Change

Turnout Change	First Term Incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Poverty	-0.03 (0.09)	-0.04 (0.09)	-0.02 (0.10)	-0.02 (0.10)	-0.02 (0.12)	-0.03 (0.13)
Mismanagement	-0.03* (0.02)	-0.03* (0.02)	-0.02 (0.02)	-0.02 (0.02)	-0.01 (0.02)	-0.02 (0.02)
Poverty X Mismanagement	0.05 (0.04)	0.06 (0.04)	0.03 (0.03)	0.04 (0.04)	0.03 (0.04)	0.03 (0.04)
Exposed	-0.07 (0.05)	-0.08 (0.05)	-0.04 (0.05)	-0.05 (0.05)	-0.03 (0.07)	-0.03 (0.07)
Exp X Poverty	0.14 (0.09)	0.16 (0.10)	0.09 (0.09)	0.11 (0.09)	0.10 (0.12)	0.11 (0.12)
Exp X Mismanagement	0.03 (0.02)	0.03* (0.02)	0.01 (0.02)	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)
Exp X Poverty X Mismanagement	-0.07* (0.04)	-0.08* (0.04)	-0.04 (0.04)	-0.05 (0.04)	-0.04 (0.05)	-0.05 (0.05)
Constant	0.22* (0.13)	0.22* (0.12)	0.04 (0.13)	0.03 (0.14)	0.04 (0.19)	0.04 (0.21)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent Controls	No	Yes	No	Yes	No	Yes
Fiscal Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lottery FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional State FEs	No	No	Yes	Yes	Yes	Yes
PMDB Mayors Excluded	No	No	No	No	Yes	Yes
R-squared	0.56	0.57	0.68	0.70	0.61	0.64
N	203	203	203	203	165	165

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.

## I.2 Poverty and Primary Education Spending Change

Log Primary Education Spending Change	First Term Incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Poverty	0.19 (0.14)	-0.18 (0.25)	-0.17 (0.24)	-0.18 (0.28)	-0.06 (0.28)	-0.07 (0.27)
Mismanagement	-0.05 (0.04)	-0.09** (0.04)	-0.10** (0.04)	-0.09** (0.04)	-0.07 (0.05)	-0.05 (0.04)
Poverty X Mismanagement	0.06 (0.05)	0.13** (0.06)	0.14** (0.06)	0.16* (0.08)	0.11 (0.09)	0.08 (0.08)
Exposed	-0.01 (0.07)	-0.11 (0.08)	-0.10 (0.09)	-0.10 (0.10)	-0.06 (0.12)	0.07 (0.14)
Exp X Poverty	-0.02 (0.16)	0.19 (0.15)	0.18 (0.16)	0.19 (0.21)	0.07 (0.27)	0.01 (0.26)
Exp X Mismanagement	0.08* (0.04)	0.13** (0.04)	0.13*** (0.05)	0.13*** (0.04)	0.10** (0.05)	0.09* (0.05)
Exp X Poverty X Mismanagement	-0.10 (0.06)	-0.19*** (0.06)	-0.20*** (0.07)	-0.22** (0.09)	-0.17 (0.10)	-0.16 (0.11)
Constant	-0.23* (0.13)	0.18 (0.21)	0.16 (0.20)	0.28 (0.43)	0.18 (0.45)	0.18 (0.37)
Municipality Budget Controls	Yes	Yes	Yes	Yes	Yes	Yes
Re-Election Control	No	No	Yes	Yes	Yes	Yes
Incumbent's Controls	No	No	No	Yes	Yes	Yes
Regional State FEs	No	Yes	Yes	Yes	Yes	Yes
Party FEs	No	No	No	No	Yes	Yes
Lottery FEs	No	No	No	No	No	Yes
R-squared	0.66	0.75	0.75	0.77	0.80	0.82
N	145	145	145	145	145	145

\*\*\* p<0.01, \*\* p<0.05, \* p<0.11. Standard errors are clustered at the State-level.