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Non-democratic regimes and Elite Capture: Evidence from the Brazilian Dictatorship

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1 Introduction

The question of how the capture of the political process by special interest groups and elites can influence policies and economic outcomes has been studied in recent years both by theoretical and by empirical political economy literature.[1] It is surprising, however, that the empirical literature rarely addresses the question of capture at local levels of government, especially considering that theoretical models have identified a number of factors that may lead to greater capture at the local levels, such as the greater cohesiveness of local interest groups and higher levels of voter ignorance.[2]

This paper addresses this question by investigating the presence of elite capture at the local level in the context of the Brazilian military dictatorship. This is a particular interesting context because during the dictatorship the mayors of some Brazilian municipalities were appointed by the regime, while others were elected directly. This research, therefore, is interested in investigating if the presence of appointed mayors in a subset of municipalities during the Brazilian dictatorship led to elite capture. In this regard, it compares measures of inequality between municipalities that had appointed mayors with a subset of municipalities where mayors were elected directly.

The Brazilian military dictatorship is an interesting case study not only because it provides this unusual variation in political institutions at the local level but also because Brazil faced high rates of economic growth along with a concentration of income in this period. In particular, this period was characterized by a large number of ambitious projects conducted by the central government, such as the construction of roads, powerplants, and heavy industry. Large amounts of resources were spent on these projects, which allows to investigate the presence of practices related to capture.[3]

The selection of disenfranchised municipalities[4] is the main empirical challenge of this research since they were not randomly assigned, but rather chosen by the federal government for specific reasons and, therefore, should be expected to be different from municipalities where mayors were democratically elected. The empirical strategy employed combines a geographic regression discontinuity (GRD) design with matching techniques, thus resembling the strategy employed by Larreguy et al. (2014). The strategy relies on the hypothesis that the main source of selection (for some

[1] See Acemoglu and Robinson (2008) for a detailed discussion of the extent to which political institution can affect economic outcomes.

[2] The possibility of capture at the local level is known as the "Madisonian presumption", according to which "the lower the level of government, the greater is the extent of capture by vested interests, and the less protected minorities and the poor tend to be" (Bardhan and Mookherjee (2000)).

[3] See more about the projects conducted by the central government during the Brazilian dictatorship at `http://oglobo.globo.com/economia/obras-da-ditadura-do-brasil-grande-ao-brasil-do-ganho-de-eficiencia-11959341`.

[4] The expressions disenfranchised municipalities and municipalities with appointed mayors are used interchangeably in this paper.

disenfranchised municipalities) is geographic characteristics. Therefore, the empirical strategy uses matching techniques to compare municipalities that had appointed mayors with their most similar neighbor (in terms of the Mahalanobis distance).

The main results of this paper indicate income inequality increased more in municipalities that had mayors appointed by the regime. Moreover, the results suggest income inequality increased more in this group of municipalities as a result of an increase in the share of income earned by the richest. Although this research is not able to explore the channels through which this wealth concentration occurred due to lack of more detailed data, the evidence that economic growth privileged a few individuals at the top of the income distribution is consistent with the hypothesis of elite capture.

The empirical literature documenting evidence of elite capture at the local level is scarce. Araujo et al. (2008), studying social fund investment in Ecuador, find that poorer villages are more likely to receive projects that provide excludable goods to the poor, evidence that is consistent with the hypothesis of elite capture. Galasso and Ravallion (2005) find that the results of Bangladesh's Food-for-Education program are worse in communities with higher land inequality. They argue this reflects the greater capture of the benefits by the elite when the poor are less powerful.

The present paper contributes to at least three strands of the literature. First, it relates to the more general literature that investigates democratic capture by elites and other interest groups. While there has been substantial development in the theoretical literature (Acemoglu and Robinson (2008)), empirical works have focused on providing evidence on existing practices that are consistent with the story of capture (Bo and Tella (2003); Acemoglu et al. (2013)) rather than documenting in which situations elite capture is more likely to happen. This paper contributes to this stream of the literature by providing evidence consistent with elite capture in a particular situation and by enhancing the role of local officials as representatives of the central regime.

This research also relates to the literature that studies the legacies of non-democratic regimes and the outcomes of new democracies (Keefer (2007); Martinez-Bravo (2014); Martinez-Bravo and Mukherjee (2015)). It contributes to this literature by showing that the legacies of the Brazilian dictatorship were accentuated in municipalities that had less democratic institutions.

Finally, this research relates to several papers that discuss the incentives of appointed and elected representatives (Besley and Coate (2003); Alesina and Tabellini (2007); Martinez-Bravo et al. (2011)) and discuss whether the allocation of central resources is politically driven (Brollo and Nannicini (2012); Solé-Ollé and Sorribas-Navarro (2008); Khemani (2007); Arulampalam et al.

(2009); Leão (2011)).

The remaining of the paper is organized as follows. Section 2 describes the political system in Brazil during the dictatorship period as well as the main features of the macroeconomic policy at that time. Section 3 describes the datasets used in this paper. Section 4 details the empirical strategy employed. Sections 5 and 6 present the main empirical results. Section 7 concludes.

2 Institutional Background

2.1 Brazilian dictatorship and municipal elections

The military government began with the 1964 coup d'état led by the armed forces that deposed President João Goulart and put in charge Humberto Castelo Branco and it lasted for more than 20 years until José Sarney, elected by indirect elections, took office as president in 1985.

The Brazilian military dictatorship had a unique political system compared with other dictatorships, when the head of government is in power uninterruptedly, parties are forbidden to work, Congress is closed, and elections are suspended. During the majority of the years of the military government, military presidents and state governors were chosen by the National Congress and state legislative houses, respectively.[5] Senators, congressmen, state legislators, and city councilors, in turn, continued to be chosen by direct vote.

The choice of mayors was even more unusual. In the majority of municipalities, mayors were elected directly throughout the regime period. In three groups of municipalities, however, mayors were appointed by the state governors, namely in state capitals, in municipalities considered to be water resorts,[6] and in municipalities located in national security areas (NSAs).

State capitals started having mayors appointed in February 1966, after AI-3, Institutional Act Number 3, which stated that state governors should be chosen by the legislative houses and the mayors of state capitals should be nominated by the governor and endorsed by the legislative houses.

Water resorts, on their turn, began to have mayors appointed after Constitutional Amendment Number 1, from October 1969[7] which stated that mayors of municipalities considered to be water resorts would be nominated by the governor, as in the case of state capitals. Brazilian law states

[5] See AI-2, Institutional Act Number 2, from October 1965, available at `http://www.planalto.gov.br/ccivil_03/AIT/ait-02-65.htm`; see also AI-3, Institutional Act Number 3, from February 1966, available at `http://www.planalto.gov.br/ccivil_03/AIT/ait-03-66.htm`.

[6] or considered to be *Estâncias Hidrominerais*, to use the Portuguese expression.

[7] Available at `http://www.planalto.gov.br/ccivil_03/constituicao/Emendas/Emc_anterior1988/emc01-69.htm`.

that to be considered to be a water resort a municipality has to meet two conditions. First, it needs to have water sources that can be explored.[8] Second, it needs to be explicitly declared as a water resort by state law.[9]

Finally, mayors of municipalities in NSAs began to be appointed after law number 5449, from 1968,[10] which classified several municipalities under the condition of NSA and stated that the mayors of these municipalities should be nominated by the state governor and endorsed by the president. The criteria that led the government to classify municipalities in this way are unclear in the official documents; however, according to Nicolau (2012), these were basically border municipalities and municipalities in areas that had large state-owned enterprises. Section 4 presents a map with the distribution of disenfranchised municipalities and shows that the majority of municipalities located in NSAs are border municipalities.

Four rounds of mayoral elections happened during the dictatorship. The first round took place between 1965 and 1970, while the other three rounds happened in 1972, 1976, and 1982 in all states of the country simultaneously. In 1985, at the end of the dictatorship, elections for mayor happened in all Brazilian municipalities.

The partisan system in Brazil during the period analyzed in this paper should also be highlighted. The multi–party system created in 1946 was abolished in 1965 by Institutional Act Number 2, which created a two–party system, with ARENA (*Aliança Renovadora Nacional*), the ruling party, and MDB (*Movimento Democrático Brasileiro*) playing the role of the opposition. Until the end of the 1970s, these two political parties were the only ones officially registered and able to run for election. In 1979, however, law number 6767 extinguished both parties and created a multi-party system.[11] Among other things, the law instituted in 1979 stated that political parties should have the word party – *partido* in Portuguese – in their names. Therefore, MDB became PMDB (*Partido do Movimento Democrático Brasileiro*). ARENA, in turn, was recreated by its leaders as the *Partido Democrático Social* (PDS). Three other parties that obtained registration to run in the 1982 elections, *Partido Trabalhista Brasileiro*, *Partido Democrático Trabalhista* and *Partido dos Trabalhadores*, comprised politicians whose political rights had been revoked during the early years of the dictatorship in addition to other politicians returning from exile. Figure 1 illustrates the timeline of the relevant events and the years in which the mayors of some municipalities were

[8] See law n. 7841/1945 available at `http://www.planalto.gov.br/ccivil_03/decreto-lei/1937-1946/Del7841.htm`.

[9] See law n. 2661/1955 available at `http://www.planalto.gov.br/ccivil_03/leis/1950-1969/L2661.htm`.

[10] Available at `http://www.planalto.gov.br/ccivil_03/leis/1950-1969/L5449.htm`.

[11] Available at `http://www.planalto.gov.br/ccivil_03/leis/1970-1979/L6767.htm`.

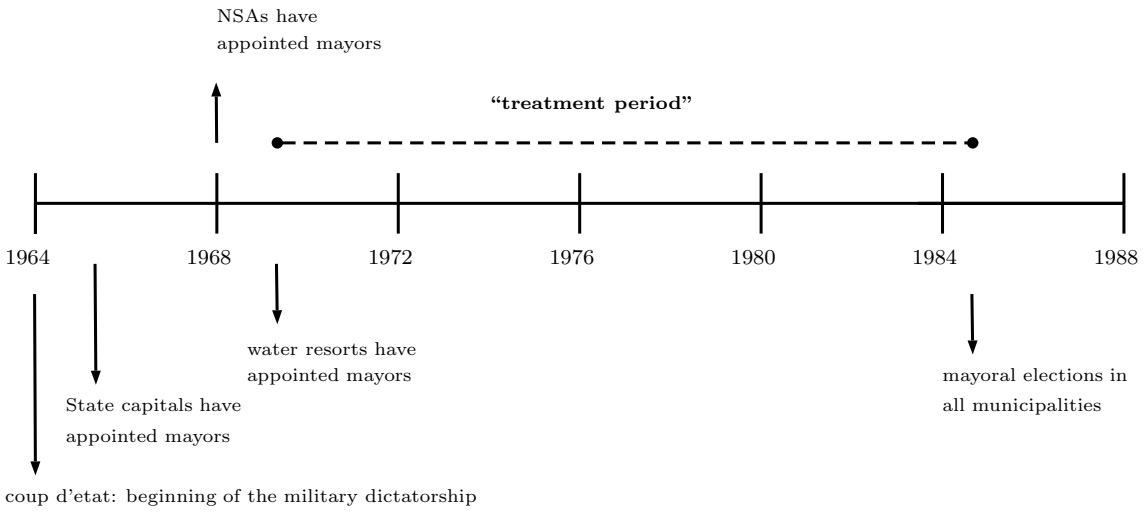

Figure 1: Timeline of the relevant events during the Brazilian dictatorship

appointed by the regime, while others were elected directly, refereed to as the "treatment period".

2.2 Economic growth and the rise in income inequality

The military dictatorship period was one of strong economic growth, especially the first half of the regime. It was also a period in which income inequality increased substantially. To understand how this process occurred, it is important to examine the main features of the Brazilian economy during the years of the regime.

During the mandate of the first military president, between 1964 and 1967, with the objective of transforming Brazil into a modern capitalist economy, a series of reforms aimed at reducing inflation and at modernizing capital markets were implemented. As a result of such reforms and problems associated with import substitution industrialization inherited from the democratic period, the Brazilian economy lost much of its dynamism until 1967.

After 1967, however, as a reflect of the reforms adopted years before, the government was able to adopt an expansionary policy, by increasing credit, especially for housing and durable goods, and by increasing investment in state-managed companies. As a result of this effort together with the state of the world economy, economic growth between 1968 and 1973 was very strong, with the GDP growing at an average rate of over 11% per year. Most importantly, this growth was achieved with a slightly decrease of the inflation rate.[12] This was possible for a number of reasons

[12]This period became to be known as the "Brazilian Miracle"

but mainly, and most importantly for the sake of this research, trough price and wage control[13], which disadvantaged the poorer part of the population and increased income inequality (Singer (2014)).

The economic growth in 1964–1973 was followed by an increase in the dependence of the Brazilian economy from foreign economies, especially relating to the import of capital goods and oil.[14] Therefore, when oil prices rose in 1973, the government was forced to change its economic policy towards a model that decreased dependence on foreign economies. Facing political pressure and high liquidity in the international market fuelled by petrodollars, the Brazilian government adopted a non-recessionary adjustment model, encouraging sectors that were identified as the main sources of the external dependency, namely infrastructure, energy, and capital goods (Castro and Souza (2004)).

Owing to the 1979 oil crisis, it was not possible to continue with non-recessionary adjustment. The cost incurred by the country was high, and despite attempts to prevent it, recessionary adjustment had to be adopted. Between 1981 and 1983, GDP growth was -2.2% per year on average. From mid-1984 onwards, Brazil's economy started to grow moderately under a hyperinflation process, which obliged the government to adopt a number of economic plans and measures that contemplated price and wage controls and traditional recessionary measures, increasing income inequalities further still (Castro (2005)).

3 Data

The main dataset used in this paper was constructed from historical files from the Federal Electoral Authority, the *Tribunal Regional Eleitoral*, which contains information on mayors appointed during the 1970s and 1980s in Brazil. In addition to this dataset, this paper also uses data from the 1970, 1980, and 1991 Demographic Censuses[15] provided by the *Instituto Brasileiro de Geografia e Estatística* (IBGE), which are used to construct socioeconomic variables at the municipality level. This paper also uses information on municipalities neighbors in 1970, constructed from the *shapefile* of Brazilian municipalities in 1970, also provided by the IBGE.

As previously mentioned, three groups of municipalities had appointed mayors between 1967

[13] Wages were not allowed to rise above certain thresholds established by the federal government

[14] Oil imports between 1967 and 1973 jumped from 59% of total consumption in the country to 81% Herman (2005).

[15] The Demographic Census that was supposed to be carried out in 1990 was conducted in 1991 because of administrative issues.

and 1985: state capitals, municipalities considered to be water resorts, and municipalities located in NSAs. Table 1 presents the number of municipalities classified into each of these categories in 1972, 1976, and 1982, the three municipal elections for which data are available.[16]

Table 1: Municipalities with appointed mayors

	1972	1976	1982
NSAs	100	109	102
Water resorts	37	34	16
State capitals	24	25	25
Total	161	168	143

Municipalities located in NSAs represent the majority of municipalities that had appointed mayors in the analyzed period, with 100 municipalities being classified into this category in 1972. Between 1972 and 1976, ten other municipalities were classified as such, four in Bahia State, two in Para State, and another four located in four different states. In the same period, one municipality, in Rio de Janeiro State, which was considered to be in an NSA, was reclassified as a water resort.[17] Between 1976 and 1982, eight new municipalities were considered to be located in an NSA, the majority of them located in Santa Catarina State. In the same period, one municipality considered to be in an NSA in the previous election had direct mayoral elections.

The number of municipalities considered to be water resorts also changed over time. In 1972, 37 municipalities were classified as such. This number decreased to 34 in the 1976 election, as two municipalities were reclassified as located in an NSA, two were reclassified and had mayoral elections, and one that was considered to be located in an NSA was now considered to be a water resort. In 1982, the municipalities classified as water resorts in São Paulo State (and four others in different states) were allowed to elect their mayors by direct elections.

Although not constant in Table 1, the number of state capitals does not change in this period. Table 1 presents an increase in state capitals that had appointed mayors between 1972 and 1976 because Curitiba, the capital of Parana State, elected its mayor directly in 1972. Further, between 1972 and 1976, the capital of Rio de Janeiro State changed from Niteroi to Rio de Janeiro.

To have clearer treatment and control groups, this paper analyzes only those municipalities

[16] As previously mentioned, there was a municipal election in 1970 but data for this election are unavailable. Therefore, it is not possible to credibly identify which municipalities had appointed mayors and why.

[17] The information in this table was obtained from hand-written documents that contain the results of the 1972, 1976, and 1982 municipal elections. Therefore, it is possible that the change in municipality classification is a reflection of mistakes in these documents.

that either had appointed mayors in all three mentioned municipal elections or elected mayors in the same three elections. Municipalities partially disenfranchised, that is, those municipalities that had appointed mayors in only one or two of the referred elections are therefore excluded from this analysis.

4 Empirical strategy

Identifying the effects of a change in political institutions such as having appointed mayors during almost two decades in income distribution is not straightforward. Municipalities that had appointed mayors were not randomly chosen; they were selected by the federal government for specific reasons and therefore should be expected to be different from the rest of the country in many dimensions. Table 2 presents the baseline characteristics of Brazilian municipalities by different groups. The first column reports the characteristics of all municipalities present in the 1970 Demographic Census. Columns (2) and (3) present the characteristics of those municipalities that had appointed mayors in at least one of the three elections between 1972 and 1982.

Table 2: Mean of the baseline (1970) socioeconomic characteristics

	(1) All municipalities	(2) Municipalities with appointed mayors	(3) Municipalities with appointed mayors (except state capitals)
Inequality (Theil index)	35.12	40.19	38.19
Share of pop. living in urban areas	32.16	43.99	36.17
log(population)	9.39	10.25	9.81
Population density (inhabitants/km^2)	59.55	269.60	69.08
Share of illiteracy	43.64	30.64	32.35
Average years of schooling	1.39	2.27	2.01
Income per capita (in minimum wages)	0.35	0.55	0.48
Log(number of households)	7.69	8.54	8.10
Life expectancy	51.26	53.25	53.61
Share of pop. occupied	30.60	31.46	31.78
Share of households with sanitation	4.97	10.18	8.95
Share of households with piped water	14.57	26.73	22.72
Share of households with electricity	24.00	37.24	31.72
Share of pop. living in poverty	83.96	70.80	73.90
Number of municipalities	3951	172	146

Note: all the differences in columns (1) and (2) are significant at the 1% level. All the differences in columns (1) to (3) are significant at the 1% level, except Share of pop. living in urban areas (p-value=0.02) and Population density (p-value=0.70). The differences in columns (2) and (3) are all significant at the 1% level, except Share of pop. occupied (p-value=0.04) and Share of households with sanitation (p-value=0.03).

As expected, column (2) shows that municipalities with appointed mayors are different in numerous ways from the rest of the country. In particular, they are more urbanized, more populated,

wealthier, present better measures of schooling, and their citizens have higher life expectancy. Therefore, simply comparing this group of municipalities with the rest of the country would not be possible to assess the effect of having appointed mayors on economic outcomes.

Since state capitals are likely to be different from the rest of the municipalities, column (3) presents the characteristics of municipalities considered to be water resorts and those located in NSAs. This shows that even excluding state capitals, municipalities that had appointed mayors are very different from the rest of the country. Despite this notable difference, however, it is reasonable to believe, based on what has been exposed in previous sections, that these two groups of municipalities were chosen by the federal government to have appointed mayors mainly because of their geographic characteristics (i.e. availability of explorable water and proximity to borders). Figure 2 illustrates this fact by showing the distribution of municipalities that had appointed mayors in at least one of the three elections between 1972 and 1982. This figure shows that, especially for municipalities located in NSAs, the selection was mainly driven by geographic characteristics, namely being located on the border of the country. Although spread over the country, to be considered a water resorts, a municipality had to meet a clearly geographical requirement (i.e having explorable water sources). Therefore, to minimize the concern with (political and economic) selection, this paper focuses its analysis on municipalities located in NSAs and those considered to be water resorts. By doing so, the issue of the selection of municipalities based on political and economic characteristics is substantially reduced. In other words, by using this subset of municipalities that, arguably, were selected mainly by their geographic characteristics, the main source of endogeneity becomes known and, therefore, it is possible to develop a strategy to deal with it.

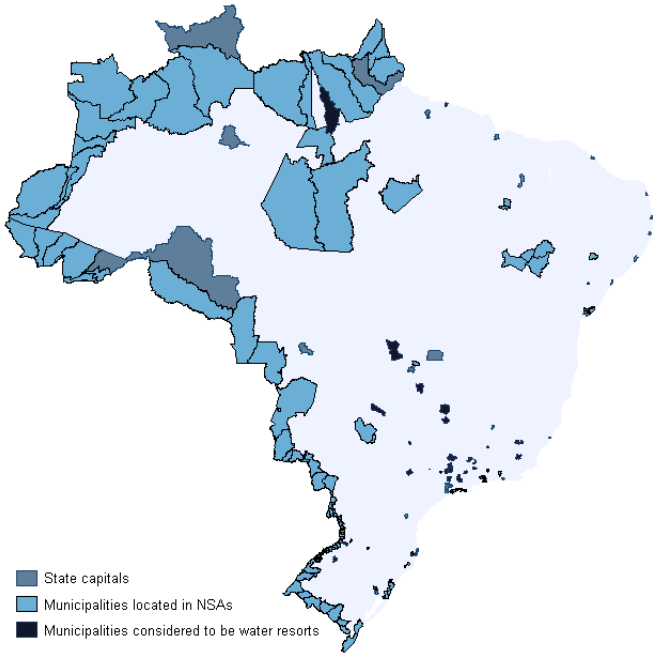

Figure 2: Municipalities with appointed mayors

Even by restricting the analysis to this subset of municipalities, it is still not possible to simply compare municipalities with appointed mayors with the rest of the country to assess the effect of this variation in political institutions. An alternative approach would be to use geography as an instrument for disenfranchised municipalities (i.e. a dummy equal to one if the municipality is in the border and/or a measure of the amounts of explorable water in the municipality). The issue with this strategy, however, is the well documented influence of geography on economic institutions and economic outcomes, thereby violating the exclusion restriction.[18]

The empirical strategy employed in this paper resembles that proposed by Keele et al. (2015) and used by Larreguy et al. (2014), which can be understood as a combination of geographic regression discontinuity (GRD) design with matching techniques. By claiming that one of the main sources of selection is the municipality's location, the strategy employed compares each municipality that had appointed mayors with its most similar neighbor in terms of the Mahalanobis distance.

As for non-geographic regression discontinuity designs, causal effects are identified under the assumption that potential outcomes are continuous in all other variables at the geographic discontinuity. Although it is not quite necessary, achieving balance across the treatment and control groups is sufficient for continuity to hold Imbens and Lemieux (2008). This motivates the decision

[18]For more on the debate about the relation between geography and economic institutions, see (Acemoglu et al. (2002)).

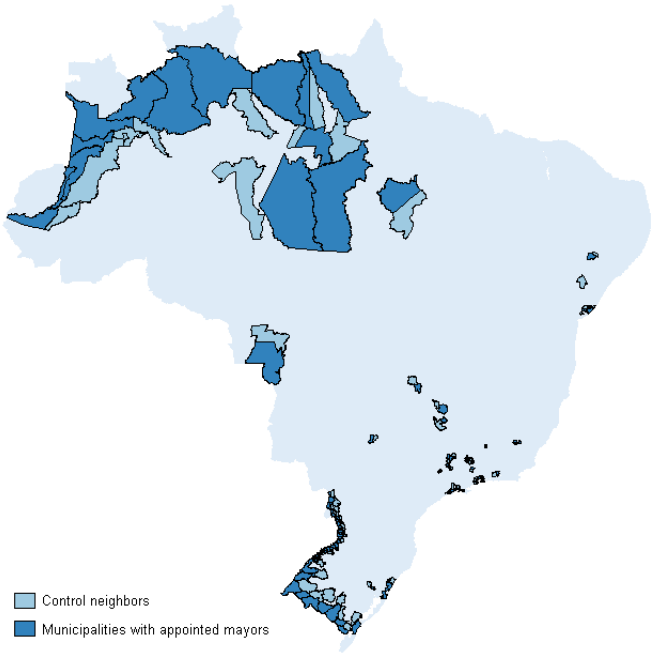

Figure 3: Municipalities with appointed mayors in the sample and neighbors used as the control group

to match over a set of covariates and select the most similar non-treated municipalities in terms of the Mahalanobis distance.

More specifically, the construction of the sample entails the following procedure:

1. Identify potential matches. For each municipality with appointed mayors i, the set of possible matches is restricted to the set of neighboring municipalities j that had mayors elected directly in 1972, 1976, and 1982. This set of potential matching is denoted $J(i)$.

2. Calculate the Mahalanobis distance $D(X_i, X_j) = \sqrt{(X_i - X_j)' C^{-1} (X_i - X_j)}$ between municipality i and each possible match $j \in J(i)$ using the vector X_i of 14 covariates and the full sample covariance matrix C.

3. Finally, for each treated municipality i, choose the control municipality taking the nearest match in the set $J(i)$.

Figure 3 illustrates the sample from the algorithm described above.

With the sample constructed, Equation 1 is estimated to assess the effect of appointed mayors on the economic outcome of interest y in the municipality i:

$$y_i = \delta \cdot y_{i,1970} + \gamma \cdot appointed_i + \mathbf{X_i}\beta + \varepsilon_i \tag{1}$$

where *appointed* is a dummy variable that takes the value one for municipalities that had appointed mayors in the analyzed period. Since this research is interested in the changes in economic outcomes y_i after a municipality had appointed mayors, the baseline variable $y_{i,1970}$ is also included in the regression. Although the results from balance checks reported in Section 5.1 show that control and treatment groups are balanced in baseline covariates constructed from the 1970 Demographic Census, the complete specification of Equation 1 is estimated including the vector of covariates $\mathbf{X_i}$ as a control.

Equation 1 is run for the measures of inequality constructed from the 1980 and 1991 Demographic Censuses. Since the municipalities in the treatment group had appointed mayors between the end of the 1960s and 1985, measuring the effects of appointed mayors with minimum noise and avoiding confounding the effects with, for instance, possible heterogeneous effects of redemocratization among treatment and control groups would ideally require estimating the effects on inequality (or any other possible outcomes of interest) immediately after the treatment has ended (i.e. immediately after redemocratization in 1985 when all municipalities had direct mayoral elections). Unfortunately, this is not possible since detailed socioeconomic data at the municipality level such as income distribution measures were only collected in 1980 and 1991. Therefore, to provide evidence that the results are indeed driven by having appointed mayors, this research tests for differences in economic outcomes both in 1980 and in 1991. If appointed mayors affect the income distribution, one should expect to see this effect increase over time. Moreover, it seems unreasonable to believe that redemocratization would have different effects in municipalities in the treatment and control groups, especially in terms of redistribution and in such a short period. If anything, using measures of income inequality in 1991 introduces some noise into the estimates.

One concern with the empirical strategy described is with confounding treatments, a concern that naturally arises with strategies that rely on geography. By comparing municipalities in specific locations with their neighbors, the effect identified may not only be the effect of having appointed mayors per se but also be the effect of being in that specific geographic area. In other words, proximity to a border or having large amounts of explorable water may explain the findings presented in this paper. To provide evidence that this is not the case, a placebo exercise comparing non-treated neighbors as if they were treated with their own non-treated neighbors is reported in Section 6.

To show that the main results are robust to the matching algorithm, in appendix A the same exercises of the following section are reproduced; however, instead of using the matching algorithm to identify each disenfranchised municipality's closest neighbor, all non-disenfranchised neighbors

are used as counterfactual. In contrast to employing a combination of geographic regression discontinuity design and matching, balance among the control and treatment groups is not achieved. The main results, however, are qualitatively unchanged when municipality-level controls are included. The results are presented in Tables 10–12. Figure 6 illustrates the sample constructed without the matching algorithm.

5 Results

5.1 Balance check

This section begins by showing evidence that the empirical strategy described in the previous section results in a control group that is similar to the group of treated municipalities in a number of relevant predetermined characteristics. Table 3 reports the same statistics presented in Table 2. In contrast to that table, however, it shows the mean characteristics only for the subset of municipalities that had appointed mayors considered in the analysis (i.e. state capitals and partially disenfranchised municipalities are not considered). The comparison group also differs from that in Table 2 by including only the closest neighbor of each disenfranchised municipality measured by the Mahalanobis distance.

Table 3: Balance check of the baseline characteristics between municipalities with appointed mayors and the control group

	(1) Mun. with appointed mayors	(2) Control municipalities	(3) p-value
Inequality (Theil index)	38.04	36.97	0.51
Share of pop. living in urban areas	32.69	29.65	0.41
log(population)	9.98	9.83	0.35
Population density (inhabitants/km^2)	65.24	57.28	0.75
Share of illiteracy	29.65	31.89	0.33
Average years of schooling	2.15	1.95	0.13
Income per capita (in minimum wages)	0.49	0.43	0.12
Log(number of households)	8.27	8.11	0.35
Life expectancy	54.07	53.79	0.68
Share of pop. occupied	32.70	31.54	0.18
Share of households with sanitation	8.91	7.18	0.48
Share of households with piped water	22.98	19.53	0.36
Share of households with electricity	31.48	28.45	0.44
Share of pop. living in poverty	73.70	77.54	0.14
Number of municipalities	81	66	-

As seen in columns (1) and (2), even when restricting the comparison to municipalities with

appointed mayors and their closest neighbors, the former present higher average measures of urbanization, wealth, schooling, inequality, and population size. These differences, however, are now not statistically significant at the usual levels.

To ensure that this balance in covariates is not simply a mechanical result of the matching algorithm implemented, Table 4 reproduces the statistics and tests reported in the previous table using samples constructed from a slightly different matching algorithm. For each one of the 14 covariates of the vector $\mathbf{X_i}$, a sample was constructed using the remaining 13 covariates to match each treated municipality with the closest neighbor. More specifically, the procedure for each covariate $x_s \in \mathbf{X_i}$ is as follows:

1. Identify the set $J(i)$ of potential matches for each municipality with appointed mayors i.

2. Define the set $X_{i,-s}$ that contains all the covariates except for x_s.

3. Calculate the Mahalanobis distance $D(X_{i,-s}, X_{j,-s})$ between municipality i and each possible municipality match $j \in J(i)$ using the vector $X_{i,-s}$ of the remaining 13 covariates and the full sample covariance matrix C.

4. For each treated municipality i, choose the control municipality by taking the nearest match in the set $J(i)$.

This procedure results in 14 potentially different samples with which it is possible to test for the differences in each of the covariates between the treatment and control groups. The results presented in Table 4 show that the covariates are balanced, except for per capita income, which is slightly higher in treated municipalities, with the difference significant at the 10% level. The results not only show that the balance between the treatment and control groups is not simply a mechanical result of the matching algorithm implemented but also suggest that the treatment and control groups might be balanced in other relevant (unobservable) characteristics.

5.1.1 Effects on income distribution

This section reports the main results of this paper, the effect of having appointed mayors for almost two decades on income distribution. Figure 4 illustrates the evolution of the Theil index in municipalities that had appointed mayors during the dictatorship and in neighboring control municipalities. In both groups of municipalities, inequality increased substantially during the years of the military dictatorship, consistent with the discussion in the previous sections.

Figure 4 also evidences that the increase in inequality is accentuated in disenfranchised municipalities. Table 5 formalizes these results by showing the estimates of Equation 1. The dependent

Table 4: Balance check of the baseline characteristics between municipalities with appointed mayors and the control group (using N-1 covariates to match)

	Mun. with appointed mayors		Control municipalities		p-value
	mean	obs	mean	obs	
	(1)	(2)	(3)	(4)	(5)
Inequality (Theil index)	38.04	81	37.14	65	0.59
Share of pop. living in urban areas	32.69	81	29.31	67	0.35
log(population)	9.98	81	9.84	66	0.38
Population density (inhabitants/km^2)	65.24	81	93.20	66	0.52
Share of illiteracy	29.65	81	31.80	63	0.37
Average years of schooling	2.15	81	1.97	63	0.21
Income per capita (in minimum wages)	0.49	81	0.42	64	0.06
Log(number of households)	8.27	81	8.12	66	0.38
Life expectancy	54.07	81	53.99	64	0.91
Share of pop. occupied	32.70	81	31.54	65	0.18
Share of households with sanitation	8.91	81	6.52	68	0.31
Share of households with piped water	22.98	81	19.45	67	0.35
Share of households with electricity	31.48	81	29.67	67	0.65
Share of pop. living in poverty	73.70	81	77.60	67	0.13

Notes: each line presents the statistics of the test of the mean difference between the municipalities with appointed mayors and the control group. To construct the control group in this exercise, each municipality was matched to its most similar neighbor according to a set of N-1 covariates and the control group, with the omitted covariate being the variable tested for the difference in each line. Each line, therefore, may have a different control group.

variable *appointed mayor* is a dummy variable that takes the value one if the municipality had appointed mayors in all three municipal elections between 1972 and 1982 and zero if the municipality had mayors elected democratically. The dependent variable is the *Theil index* measured in 1980 and 1991 and it is given by:

$$Theil\ index = \frac{1}{N}\sum_{i=1}^{N}\left(\frac{x_i}{\overline{x}} \cdot \ln \frac{x_i}{\overline{x}}\right) \tag{2}$$

where x_i is the income of each individual and $\overline{x}$ is the mean of x. If everyone has the same income, then $x_i = \overline{x}, \forall i$ and the Theil index equals zero. On the contrary, if one person has all the income, the index equals $ln(N)$. The index is normalized to be in the interval $[0, 1]$.

Although the balance checks show that the control and treatment groups are balanced in a number of dimensions (including inequality), the baseline Theil index is included in all regressions to ensure that the variation in the index from one period to another is being estimated. Columns (1) and (3) report the estimates of the effect of having appointed mayors on the Theil index in 1980 and 1991, respectively, without including the baseline controls. Columns (2) and (4) present the results of similar regressions but with the inclusion of the vector of baseline controls $\mathbf{X_i}$.

Table 5: Effect on the Theil index

Dependent variable: Theil index in year t; covariates measured in t=1970

	t=1980		t=1991	
	(1)	(2)	(3)	(4)
Appointed mayor	1.3974	1.3327	4.1105*	4.3194**
	(1.9138)	(1.8442)	(2.1142)	(2.0024)
Inequality (Theil index)	0.6540***	0.8013***	0.2671***	0.4151**
	(0.0888)	(0.1507)	(0.0955)	(0.1608)
Share of pop. living in urban areas		0.1521		0.1001
		(0.1023)		(0.0836)
Log(population)		-9.8572		22.9578
		(15.9397)		(16.4989)
Population density		-0.0076		-0.0035
		(0.0083)		(0.0069)
Share of illiteracy		-0.3938**		0.1045
		(0.1700)		(0.2320)
Average years of schooling		-6.0890*		0.0194
		(3.1299)		(3.9098)
Income per capita (in minimum wages)		-11.9974		-21.8445*
		(13.1049)		(12.9617)
Log(number of households)		10.8757		-21.3136
		(15.8127)		(16.7353)
Life expectancy		0.3571		1.2109***
		(0.3612)		(0.4240)
Share of pop. occupied		-2.4322		5.3908
		(21.1955)		(26.1852)
Share of households with sanitation		0.2134**		0.1862*
		(0.0984)		(0.0978)
Share of households with piped water		-0.2062**		-0.1032
		(0.0936)		(0.0897)
Share of households with electricity		-0.2870**		-0.0517
		(0.1294)		(0.1227)
Share of pop. in poverty		-0.3860		-0.1497
		(0.2480)		(0.2127)
Observations	147	147	147	147
R-squared	0.24	0.41	0.07	0.28

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

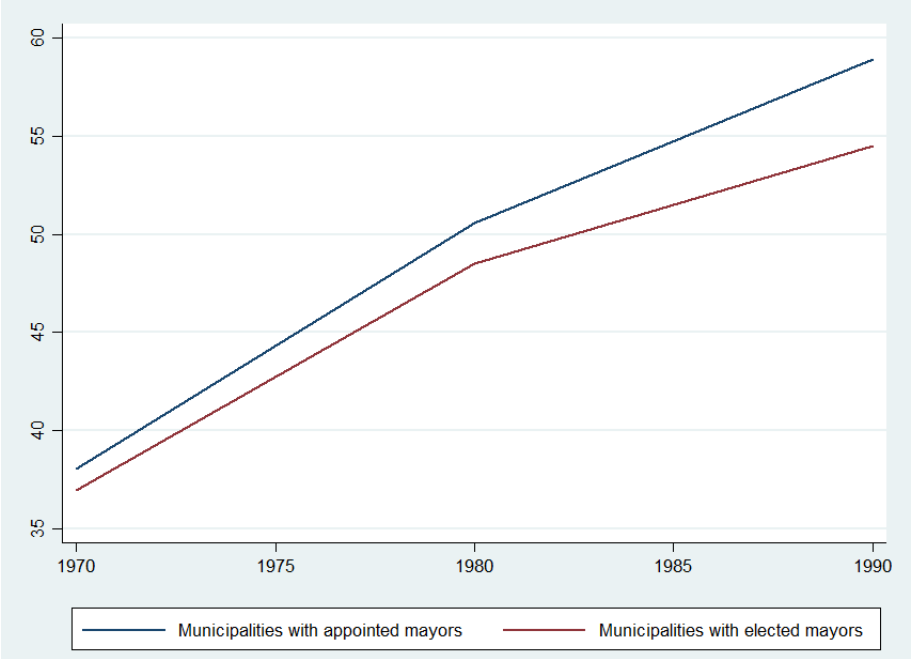

Figure 4: Theil index in municipalities with appointed and with elected mayors

Table 5 shows that the difference in the increase in the Theil index between municipalities with appointed mayors and the control group, although positive, is not significant in 1980. The difference estimated in 1991, however, is not only positive but highly significant. Further, both sets of results are robust to the inclusion of municipality–level controls. Indeed, the point estimates do not change with the inclusion of these controls; they only become more precise, providing further evidence the sample is fairly well balanced. The results show that on average municipalities that had appointed mayors present a Theil index around 4 points higher than their neighbors in 1991.[19] In the same period, the Theil index in Brazil went from 68 in 1970 to 78 in 1991. That is to say, having mayors appointed by the dictatorship regime is associated with an increase in inequality similar to 40% of the rise the country experienced during those two decades.

Although this research focuses on studying the presence of elite capture by measuring income inequality, it is convenient to look at how other variables evolved during this period in disenfranchised municipalities compared with their control neighbors. Tables 13 and 14 in appendix A show that the vast majority of the other socioeconomic variables did not present significant differences between the treatment and control groups in 1980 and 1991. The only exceptions are the number of households and size of population, which are larger in the treated municipalities, suggesting that not only inequality increased in disenfranchised municipalities but they also become larger

[19] in 1970, as seen in Table 3, this difference was balanced between the two groups.

compared with the control group.

The results presented thus far show that having appointed mayors is associated with a significant increase in inequality. However, this increase in inequality cannot yet be interpreted as the presence of elite capture. To shed light on the reasons behind the effects reported in the previous table, Table 6 presents the results of the estimates of Equation 1 on the other measures of income distribution. The dependent variables used in these regressions were constructed from the 1991 Demographic Census and show the share of the municipality income earned by different quintiles of the population. In the first column, the dependent variable is the share of income earned by the 20% poorest; in the second column, the share earned by the 40% poorest; in the third column, the share earned by the 60% poorest; in the fourth, the share earned by the 20% richest (or one minus the share earned by the 80% poorest); and in the last column, the share earned by the 10% richest (or one minus the share earned by the 90% poorest).

As discussed in Section 2, the understanding in the economic history literature is that the concentration of wealth in this period was mainly through a decrease in real wages, which ended up punishing the lower classes of the population to a greater extent. Therefore, if the increase in inequality in disenfranchised municipalities was simply a magnification of the distributional effects that took place across the country, strong negative effects on the share of the income earned by the poorest should be expected. However, according to the estimates in Table 6, inequality increased in municipalities that had appointed mayors more than it did in the control group mainly due to an increase in the share of income earned by the richest. In other words, the situation of the poor in municipalities that had appointed mayors and in neighboring municipalities changed similarly between 1970 and 1991. In the same period, the situation of the rich, on the contrary, improved dramatically in municipalities that had appointed mayors compared with neighboring municipalities.

These results are consistent with a story of elite capture in these municipalities, especially considering that this was a period of intense investment by the central government and that, from the evidence documented in the political economy literature,[20] these municipalities were more likely to receive (larger) federal investment because of their political alignment. Ideally, this hypothesis could be better investigated by looking at expenditure data. However, as such data are unavailable, this research relies on the latter findings and on the results presented in Tables 13 and 14 in the

[20]See Brollo and Nannicini (2012); Solé-Ollé and Sorribas-Navarro (2008); Khemani (2007); Arulampalam et al. (2009); and Leão (2011).

appendix A, which imply that disenfranchised municipalities increased more during this period and are, therefore, consistent with the hypothesis that these municipalities received more investment.

Table 6: Effect on income distribution in 1991

	(1) Share of income earned by the 20% poorest	(2) Share of income earned by the 40% poorest	(3) Share of income earned by the 60% poorest	(4) Share of income earned by the 20% richest	(5) Share of income earned by the 10% richest
Appointed mayor	-0.2713*	-0.7943**	-1.4112**	2.0369**	2.2260**
	(0.1620)	(0.3361)	(0.5498)	(0.8104)	(0.9508)
Inequality (Theil index)	-0.0134	-0.0497	-0.1020**	0.1631**	0.1548*
	(0.0152)	(0.0307)	(0.0479)	(0.0674)	(0.0824)
Share of pop. living in urban areas	-0.0080	-0.0258	-0.0536**	0.0855**	0.0982**
	(0.0079)	(0.0156)	(0.0249)	(0.0371)	(0.0432)
Log(population)	-1.3292	-3.5221	-6.6941	10.0303	9.5739
	(1.1748)	(2.6386)	(4.3120)	(6.3429)	(7.7164)
Population density	0.0004	0.0010	0.0021	-0.0027	-0.0028
	(0.0006)	(0.0013)	(0.0022)	(0.0031)	(0.0037)
Share of illiteracy	-0.0534***	-0.0952**	-0.1355**	0.1526*	0.1100
	(0.0200)	(0.0366)	(0.0581)	(0.0878)	(0.1077)
Average years of schooling	-0.5164*	-0.8055	-0.8791	0.1118	-1.1463
	(0.3033)	(0.6161)	(1.0326)	(1.5561)	(1.8527)
Income per capita (in minimum wages)	2.0793*	4.4765*	7.4798*	-10.1511*	-9.1960
	(1.1607)	(2.4255)	(3.8631)	(5.4541)	(6.3424)
Log(number of households)	1.0985	3.0327	5.9493	-9.3118	-9.0531
	(1.1984)	(2.6896)	(4.4027)	(6.4713)	(7.8648)
Life expectancy	-0.1131***	-0.2494***	-0.4091***	0.5816***	0.6056***
	(0.0345)	(0.0727)	(0.1160)	(0.1666)	(0.1914)
Share of pop. occupied	-1.2846	-3.0793	-5.0793	5.7785	1.6415
	(1.9954)	(3.9446)	(6.3587)	(9.5255)	(11.9249)
Share of households with sanitation	-0.0074	-0.0212	-0.0447	0.0786*	0.0944**
	(0.0072)	(0.0164)	(0.0290)	(0.0421)	(0.0472)
Share of households with piped water	0.0107	0.0218	0.0379	-0.0588	-0.0800*
	(0.0067)	(0.0141)	(0.0242)	(0.0390)	(0.0482)
Share of households with electricity	-0.0028	0.0023	0.0064	0.0070	0.0368
	(0.0081)	(0.0180)	(0.0319)	(0.0508)	(0.0613)
Share of pop. in poverty	0.0164	0.0320	0.0516	-0.0594	-0.0273
	(0.0201)	(0.0395)	(0.0627)	(0.0894)	(0.1037)
Observations	147	147	147	147	147
R-squared	0.24	0.29	0.31	0.30	0.25

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Placebo exercise

Since part of the empirical strategy employed in this paper relies on geography, one possible concern is that the effects documented in the previous sections are not (entirely) related to having appointed mayors but are (also) a result of being in a specific geographical area. This section reports the results of a placebo exercise conducted to reject this hypothesis. The exercise considers as treated all non-

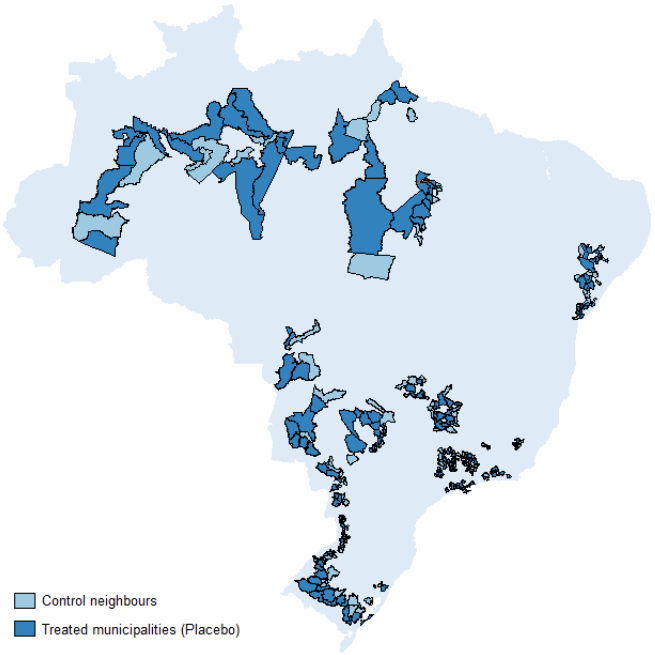

Figure 5: Placebo exercise: Treated municipalities and neighbors used as the control group

disenfranchised neighbors of disenfranchised municipalities (considered in the previous analysis) and compares them with their closest non-disenfranchised neighbors, using a matching algorithm similar to that described in Section 4.[21]

If the effect documented in the previous section is (partially) driven by being close to the border of the country – in the case of municipalities located in NSAs – or in an area with large amounts of explorable water – in the case of municipalities considered to be water resorts – similar results should be expected when estimating equation 1 in this particular sample. This would not be the case in the unlikely hypothesis that the effect associated with being in a specific geographic area changes discretely. In such a scenario, it would be impossible to disentangle both effects with the strategy employed.

Figure 5 illustrates the placebo exercise. Non-disenfranchised neighbors of disenfranchised municipalities are considered to be treated in this case. A similar matching algorithm is then carried out with their non-disenfranchised neighbors to identify the closest neighbor to be used as the control.

To provide evidence that the strategy employed is able to construct a placebo group that is similar to its respective control group, Table 7 reports the results of a balance check exercise,

[21] A more natural alternative would be to consider as treated only the closest neighbor of each treated municipality. This alternative, however, would result in a smaller sample, which could lead to non-significant results due to low power.

similar to that in Table 3 for the main sample. The results show that the only variable that is not balanced across the placebo and control group is the share of population living in poverty. All the other covariates have a non-significant difference between both groups.

Table 7: Placebo: balance check of the baseline characteristics between treated municipalities and the control group

	(1) Placebo	(2) Control	(3) p-value
Inequality (Theil index)	36.69	36.92	0.85
Share of pop. living in urban areas	31.49	28.67	0.15
log(population)	9.49	9.47	0.86
Population density (inhabitants/km$\hat{2}$)	47.62	63.67	0.51
Share of illiteracy	37.08	37.16	0.96
Average years of schooling	1.67	1.65	0.80
Income per capita (in minimum wages)	0.43	0.39	0.10
Log(number of households)	7.79	7.78	0.90
Life expectancy	52.33	52.43	0.83
Share of pop. occupied	31.04	30.92	0.78
Share of households with sanitation	7.46	6.68	0.61
Share of households with piped water	19.92	17.81	0.35
Share of households with electricity	28.65	27.11	0.55
Share of pop. living in poverty	77.87	80.73	0.10
Number of municipalities	195	133	-

Tables 8 and 9 reproduce the main regression of the paper using the placebo sample described above. There is no significant effect in the Theil index measured in 1980 and 1991, nor in the share of income earned by different percentiles of the population, strengthening the hypothesis that the effect is unrelated to geographic characteristics and rather associated with the regime appointing mayors for almost two decades.

Table 8: Placebo: effect on the Theil index

Dependent variable: Theil index in year t; covariates measured in t=1970

	t=1980		t=1991	
	(1)	(2)	(3)	(4)
Placebo	-1.4369	-1.7202	0.5651	0.4807
	(1.4630)	(1.3381)	(1.4391)	(1.3907)
Inequality (Theil index)	0.4994***	0.4392***	0.2384***	0.1896**
	(0.0698)	(0.0831)	(0.0694)	(0.0875)
Share of pop. living in urban areas		0.0680		0.0172
		(0.0622)		(0.0658)
Log(population)		-1.8002		23.2242**
		(9.9337)		(10.3205)
Population density		-0.0098***		-0.0084***
		(0.0035)		(0.0027)
Share of illiteracy		-0.2386		0.2293**
		(0.1521)		(0.1104)
Average years of schooling		1.7761		9.8316***
		(2.5963)		(2.3586)
Income per capita (in minimum wages)		-7.0393		3.9113
		(12.3962)		(13.3618)
Log(number of households)		3.0282		-21.9814**
		(9.9530)		(10.2413)
Life expectancy		0.2897		0.2669
		(0.2352)		(0.2341)
Share of pop. occupied		-8.7884		-23.2466
		(28.2077)		(20.1244)
Share of households with sanitation		-0.0275		-0.0841
		(0.0847)		(0.0944)
Share of households with piped water		-0.0282		-0.0685
		(0.0733)		(0.0793)
Share of households with electricity		-0.2392***		-0.1128
		(0.0818)		(0.0789)
Share of pop. in poverty		-0.2157		0.0584
		(0.1796)		(0.1843)
Observations	328	328	328	328
R-squared	0.17	0.32	0.04	0.19

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Placebo: effect on income distribution in 1991

	(1) Share of income earned by the 20% poorest	(2) Share of income earned by the 40% poorest	(3) Share of income earned by the 60% poorest	(4) Share of income earned by the 20% richest	(5) Share of income earned by the 10% richest
Placebo	-0.0377	-0.0638	-0.0738	0.0751	-0.0040
	(0.0959)	(0.2277)	(0.3991)	(0.6089)	(0.7196)
Inequality (Theil index)	-0.0037	-0.0197	-0.0417	0.0776*	0.0918**
	(0.0063)	(0.0148)	(0.0258)	(0.0396)	(0.0459)
Share of pop. living in urban areas	-0.0032	-0.0052	-0.0125	0.0162	0.0182
	(0.0047)	(0.0114)	(0.0200)	(0.0306)	(0.0352)
Log(population)	-2.4119***	-4.5505**	-7.1881**	8.8067*	9.0558*
	(0.8265)	(1.9174)	(3.2623)	(4.7612)	(5.3916)
Population density	0.0006***	0.0015***	0.0028***	-0.0040***	-0.0043***
	(0.0001)	(0.0004)	(0.0008)	(0.0013)	(0.0015)
Share of illiteracy	-0.0220**	-0.0496**	-0.0902***	0.1186**	0.1191**
	(0.0092)	(0.0198)	(0.0337)	(0.0513)	(0.0599)
Average years of schooling	-0.8197***	-1.9223***	-3.2204***	3.9467***	3.6869***
	(0.1508)	(0.3686)	(0.6861)	(1.0968)	(1.2741)
Income per capita (in minimum wages)	-0.2753	-0.9571	-2.6795	3.6392	3.4118
	(1.0091)	(2.2804)	(4.0082)	(6.2840)	(7.1242)
Log(number of households)	2.2352***	4.1479**	6.5570**	-8.0269*	-8.4462
	(0.8256)	(1.9095)	(3.2447)	(4.7380)	(5.3655)
Life expectancy	-0.0205	-0.0492	-0.0886	0.1182	0.0818
	(0.0186)	(0.0413)	(0.0698)	(0.1050)	(0.1223)
Share of pop. occupied	1.2378	3.7409	6.7186	-12.7181	-16.2033
	(1.4382)	(3.3880)	(5.9603)	(9.0014)	(10.1398)
Share of households with sanitation	0.0063	0.0143	0.0209	-0.0235	-0.0281
	(0.0064)	(0.0148)	(0.0262)	(0.0413)	(0.0492)
Share of households with piped water	0.0133***	0.0192	0.0237	-0.0146	-0.0027
	(0.0050)	(0.0126)	(0.0233)	(0.0371)	(0.0436)
Share of households with electricity	0.0007	0.0177	0.0417*	-0.0631*	-0.0604
	(0.0056)	(0.0130)	(0.0231)	(0.0368)	(0.0438)
Share of pop. in poverty	-0.0041	-0.0129	-0.0324	0.0453	0.0465
	(0.0136)	(0.0316)	(0.0553)	(0.0851)	(0.0956)
Observations	328	328	328	328	328
R-squared	0.21	0.22	0.21	0.17	0.13

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusions

This paper contributes to the political economy literature by studying the existence of capture at local levels of government, a question still underexplored in the body of empirical research, in the context of the Brazilian dictatorship. This is done by comparing measures of inequality between municipalities that had appointed mayors during the dictatorship with a set of municipalities where mayors were elected directly. The Brazilian dictatorship is an interesting context within which to study such a phenomenon for two reasons. First, it provides this unusual variation in political institutions at the local level. Second, this period was characterized by a large number of ambitious central government projects, implying a large amount of resources spent, which allows to investigate the presence of practices related to capture.

To overcome the clear issue of the selection of disenfranchised municipalities, this paper combines a GRD design with matching techniques, relying on the hypothesis that the main source of selection is related to the geographic characteristics of the municipalities. Evidence is provided that the strategy employed results in a control group that is similar to the group of treated municipalities in a number of relevant predetermined (observable) characteristics.

The main findings are consistent with the hypothesis of elite capture at the local level, since they indicate an increase not only in income inequality in municipalities that had mayors appointed by the regime but also in the share of income earned by the richest. Although data that would provide a better understanding of the channels through which this wealth concentration took place are unavailable, the results are nevertheless consistent with the hypothesis of capture.

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Appendix

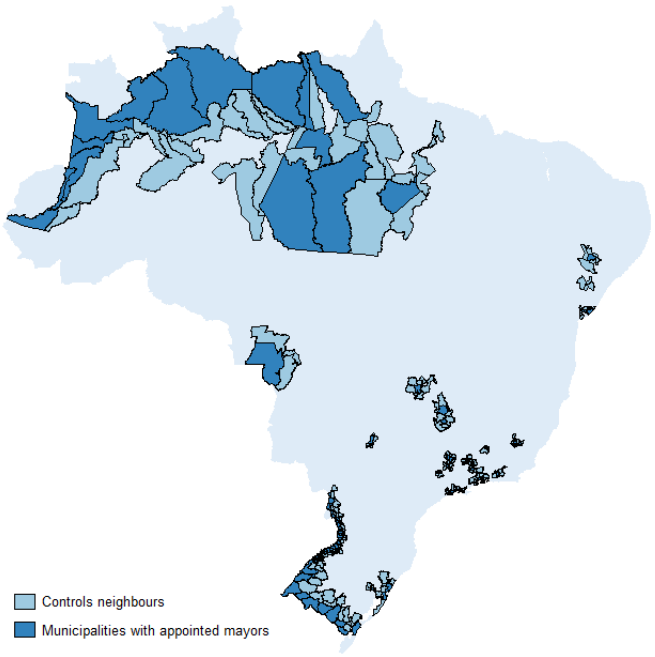

Figure 6: Municipalities with appointed mayors in the sample and neighbors used as the control group (without matching)

Table 10: Balance check of the baseline characteristics between municipalities with appointed mayors and the control group (without matching)

	(1) Mun. with appointed mayors	(2) Control municipalities	(3) p-value
Inequality (Theil index)	38.04	36.54	0.28
Share of pop. living in urban areas	32.69	31.34	0.62
log(population)	9.98	9.55	0.00
Population density (inhabitants/km2̂)	65.24	117.33	0.49
Share of illiteracy	29.65	33.81	0.03
Average years of schooling	2.15	1.84	0.01
Income per capita (in minimum wages)	0.49	0.45	0.32
Log(number of households)	8.27	7.86	0.00
Life expectancy	54.07	53.04	0.05
Share of pop. occupied	32.70	31.39	0.06
Share of households with sanitation	8.91	7.87	0.59
Share of households with piped water	22.98	21.37	0.58
Share of households with electricity	31.48	31.38	0.98
Share of pop. living in poverty	73.70	76.16	0.29
Number of municipalities	81	197	

Table 11: Effect on the Theil index (without matching)

Dependent variable: Theil index in year t; covariates measured in t=1970

	t=1980		t=1991	
	(1)	(2)	(3)	(4)
Appointed mayor	3.5216**	2.2897	5.6973***	3.7464**
	(1.5104)	(1.4551)	(1.7448)	(1.6491)
Inequality (Theil index)	0.5810***	0.5965***	0.2439***	0.3033***
	(0.0695)	(0.0959)	(0.0809)	(0.1079)
Share of pop. living in urban areas		-0.0042		-0.0361
		(0.0606)		(0.0780)
Log(population)		-10.9123		23.4167**
		(11.0938)		(11.2136)
Population density		-0.0016		-0.0009
		(0.0010)		(0.0007)
Share of illiteracy		-0.1758		0.1183
		(0.1198)		(0.1278)
Average years of schooling		-1.9276		4.5600*
		(2.4069)		(2.3955)
Income per capita (in minimum wages)		1.1827		9.1653
		(6.5794)		(5.9550)
Log(number of households)		12.2541		-22.2124**
		(11.1329)		(11.1984)
Life expectancy		0.7802***		0.8159***
		(0.2714)		(0.2785)
Share of pop. occupied		-21.4805		-35.8878**
		(14.9620)		(17.4987)
Share of households with sanitation		0.0879		0.0425
		(0.0691)		(0.0785)
Share of households with piped water		0.0049		-0.0330
		(0.0707)		(0.0831)
Share of households with electricity		-0.1956**		-0.1042
		(0.0984)		(0.0870)
Share of pop. in poverty		0.0166		0.2216*
		(0.1663)		(0.1254)
Observations	278	278	278	278
R-squared	0.22	0.35	0.07	0.26

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effect on income distribution in 1991 (without matching)

	(1) Share of income earned by the 20% poorest	(2) Share of income earned by the 40% poorest	(3) Share of income earned by the 60% poorest	(4) Share of income earned by the 20% richest	(5) Share of income earned by the 10% richest
Appointed mayor	-0.3058**	-0.7845***	-1.3161***	1.8190***	1.9555**
	(0.1304)	(0.2789)	(0.4677)	(0.6794)	(0.7714)
Inequality (Theil index)	-0.0135*	-0.0465**	-0.0891***	0.1495***	0.1576***
	(0.0079)	(0.0188)	(0.0331)	(0.0475)	(0.0520)
Share of pop. living in urban areas	0.0021	0.0049	0.0011	-0.0032	-0.0018
	(0.0058)	(0.0144)	(0.0254)	(0.0364)	(0.0400)
Log(population)	-2.0149**	-4.5017**	-7.6109**	10.6552**	10.5188**
	(0.9183)	(2.0291)	(3.3288)	(4.7070)	(5.2533)
Population density	0.0001	0.0002*	0.0004*	-0.0006*	-0.0007*
	(0.0001)	(0.0001)	(0.0002)	(0.0004)	(0.0004)
Share of illiteracy	-0.0224*	-0.0446*	-0.0744**	0.0921*	0.0778
	(0.0116)	(0.0228)	(0.0370)	(0.0537)	(0.0612)
Average years of schooling	-0.5359***	-1.0948***	-1.6934**	1.7382*	1.1854
	(0.1774)	(0.3921)	(0.6846)	(1.0389)	(1.2054)
Income per capita (in minimum wages)	-0.4102	-1.4877	-2.6028	3.2687	2.8243
	(0.4690)	(1.0566)	(1.7942)	(2.5776)	(2.7736)
Log(number of households)	1.8295**	4.1080**	6.9994**	-10.0123**	-9.9541*
	(0.9195)	(2.0253)	(3.3191)	(4.7084)	(5.2645)
Life expectancy	-0.0576***	-0.1336***	-0.2311***	0.3326***	0.3254**
	(0.0218)	(0.0485)	(0.0816)	(0.1162)	(0.1269)
Share of pop. occupied	2.3384*	5.2997*	8.6168*	-14.1938**	-17.8625**
	(1.3620)	(2.9758)	(5.0028)	(7.2054)	(8.2101)
Share of households with sanitation	0.0004	-0.0021	-0.0132	0.0276	0.0363
	(0.0056)	(0.0129)	(0.0228)	(0.0352)	(0.0407)
Share of households with piped water	0.0054	0.0087	0.0155	-0.0164	-0.0202
	(0.0055)	(0.0141)	(0.0255)	(0.0384)	(0.0437)
Share of households with electricity	0.0053	0.0173	0.0324	-0.0342	-0.0200
	(0.0064)	(0.0150)	(0.0262)	(0.0392)	(0.0448)
Share of pop. in poverty	-0.0102	-0.0343	-0.0639*	0.0929*	0.1053*
	(0.0112)	(0.0236)	(0.0383)	(0.0544)	(0.0587)
Observations	278	278	278	278	278
R-squared	0.24	0.28	0.28	0.26	0.22

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Effect on the socioeconomic variables in 1980

	(1) Inequality (Theil index)	(2) Share of pop. living in urban areas	(3) Log(population)	(4) Population density	(5) Share of illiteracy	(6) Average years of schooling	(7) Income per capita (in minimum wages)
Appointed mayor	1.3327	2.3212	0.0318	-5.1895	-0.4195	0.0574	0.0223
	(1.8442)	(1.7675)	(0.0421)	(4.0928)	(0.9396)	(0.0501)	(0.0303)
Observations	147	147	147	147	147	147	147
R-squared	0.41	0.85	0.94	0.99	0.87	0.91	0.84
	(1) Log(number of households)	(2) Life expectancy	(3) Share of pop. occupied	(4) Share of households with sanitation	(5) Share of households with piped water	(6) Share of households with electricity	(7) Share of pop. in poverty
Appointed mayor	0.0334	0.5833	-0.0025	-2.8621**	0.1184	1.1113	-0.3537
	(0.0406)	(0.3732)	(0.0077)	(1.0951)	(1.2255)	(1.7781)	(1.4100)
Observations	147	147	147	147	147	147	147
R-squared	0.95	0.71	0.48	0.91	0.89	0.83	0.78

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Effect on the socioeconomic variables in 1991

	(1) Inequality (Theil index)	(2) Share of pop. living in urban areas	(3) Log(population)	(4) Population density	(5) Share of illiteracy	(6) Average years of schooling	(7) Income per capita (in minimum wages)
Appointed mayor	4.3194**	2.5252	0.1214**	-11.8986	0.2192	0.0496	-0.0043
	(2.0024)	(2.0813)	(0.0613)	(8.0273)	(1.0994)	(0.0788)	(0.0296)
Observations	147	147	147	147	147	147	147
R-squared	0.28	0.77	0.89	0.97	0.81	0.87	0.83
	(1) Log(number of households)	(2) Life expectancy	(3) Share of pop. occupied	(4) Share of households with sanitation	(5) Share of households with piped water	(6) Share of households with electricity	(7) Share of pop. in poverty
Appointed mayor	0.1135*	0.0579	-0.3621	-0.0199	-0.0088	-0.0058	1.1380
	(0.0600)	(0.4621)	(0.8803)	(0.0224)	(0.0171)	(0.0213)	(1.3874)
Observations	147	147	147	147	147	147	147
R-squared	0.90	0.64	0.68	0.77	0.83	0.61	0.81

Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$