Organizing Fiscal Capacity*

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Abstract

This paper investigates how the organization of a tax authority's presence across the territory affects fiscal capacity. I examine a reform of the Brazilian tax authority, which closed one-fourth of local offices and reallocated agents to some of the remaining ones. I show that tax revenues decrease by 3.9% in municipalities previously served by a closed office. The effects are stronger in areas with less third-party reporting, where local information is more valuable. Distance acts as a friction, making it harder to gather local information. After the reform, the increased distance from tax offices results in lower tax revenues and fewer visits by tax agents.

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1 — Introduction

Governments in low- and middle-income countries struggle to increase their tax revenues (Besley and Persson 2013, 2014). One way to overcome this challenge is to enhance the effectiveness of tax authorities, the agencies in charge of tax administration. Recent work has emphasized how their effectiveness is impacted by the quality of tax agents and by the incentives they face (Bergeron et al. 2022; Khan, Khwaja, and Olken 2016). However, even though these agencies are often large and complex bureaucracies, little is known about how the organization of their presence across the national territory impacts fiscal capacity.

A key decision for tax authorities is how many local offices to maintain. This aspect of organizational design is particularly important for national tax authorities that manage tax administration over large areas. More local offices can help tax agents monitor the economy more effectively (Scott 1998; Lee and Zhang 2017) and better detect avasion. However, they also raise administrative costs and can lead to organizational frictions (Kiser 1994; Garicano 2000). Given these potential downsides, it is important to evaluate whether increasing the number of local offices actually improves tax enforcement.

To shed light on this issue, this paper studies a reform of the Federal Revenue of Brazil, the Brazilian tax authority. The reform closed one-fourth of the existing local offices and reallocated tax agents to some of the remaining offices. I leverage variation in how different areas are affected by this reform to investigate how having fewer local offices impacts fiscal capacity. First, I show that ax collection declines in areas previously served by a local office that was closed by the reform. Second, I document that having fewer offices is important because it makes it harder to collect local information on the economy. The physical distance between tax agents and taxpayers acts as a friction in aquiring this information.

The Brazilian tax authority is an ideal context to study the effects of having fewer local offices. This tax authority employs more than 20,000 officials and is tasked with tax administration over a territory of over 8,510,000 km^2. It is responsible for the collection of around 55% of the government tax revenues, including income tax, social security contributions, and taxes on firms' inputs and profits. In 2020, a large reorganization closed one-fourth of the local offices tasked with tax enforcement. Municipalities served by the closing offices were assigned to the jurisdiction of some

of the remaining offices. These offices also received most of the tax officials previously working in the closing offices.

To empirically investigate the impact of this reform, I combine various data sources to track the evolution of the Federal Revenue of Brazil presence and effectiveness across the national territory over the period 2016 to 2022. I reconstruct changes of tax offices' locations and tax jurisdictions boundaries from the internal regulations of the tax authority. I measure tax authority effectiveness using information on the amount of revenues it collects in each of the 5,565 municipalities - the lowest administrative level in Brazil. Moreover, I leverage novel administrative data tracing tax authority employees across offices, their trips for work, and whether they are inflicted disciplinary sanctions. The resulting dataset allows me to measure the effects of the reform at a geographically disaggregated level.

In the first part of the paper, I provide evidence on the effects of the reform on tax enforcement. My difference-in-differences design compares the evolution of tax revenues between municipalities whose jurisdiction tax office closed - *closing* municipalities - and municipalities whose jurisdiction tax office did not close - *not closing* municipalities. I find that *closing* municipalities experience a 3.9% reduction in federal tax collection. The effect is stark and persists up to the end of the sample period in 2022. There is no evidence of differential pre-trends in tax collection; this lends credibility to the identification assumption of parallel trends. Moreover, I employ various measures to document that there is no evidence of differential decline in economic activity - either before or after the closings. This confirms that the decline in tax collection is due to lower tax enforcement and not to a decline in the tax base. I also show that there is no decline in local taxes collected by the municipalities themselves - a useful placebo as one would not expect these taxes to be influenced by federal tax enforcement.

While the results so far have shown that the reform worsens tax enforcement in areas previously served by a closing office, there may also be spillovers in areas served by offices to which the tax agents are reallocated. The direction of the spillovers is not clear ex ante. On one hand, these areas are now served by an office with more tax agents, and this may lead to positive spillovers on tax collection. On the other hand, the tax agents in these offices are now responsible for monitoring a larger jurisdiction and the increased difficulty of their task may lead to negative spillovers on tax collection.

To investigate this possibility, I could compare the evolution of tax collection in *closing* municipalities, in municipalities whose jurisdiction office become larger after the reform - *receiving* municipalities - and in municipalities whose jurisdiction office does not close or receive tax agents from closed offices - the *unaffected* municipalities. However, visual inspection of the dynamic coefficients suggest that these municipalities are on differential trends in tax collection before the reform. Specifically, *closing* and *receiving* municipalities are on an increasing trend in tax collection before the reform when compared to the *unaffected* municipalities. As these trends seem linear, I follow the approach in Dobkin et al. 2018 and I control directly for a linear time trend specific to each group. This approach rests on the assumption that the trend is linear and, absent the reform, would have not changed over time. While this assumption is very strong, it allows me to say something about the nature of the spillovers and assess the sensitivity of the effects of closings on tax revenues. The point estimate for *receiving* municipalities suggests that there are negative spillovers; however, the coefficient is not statistically significant at any of the conventional levels. Reassuringly, the coefficient for *closing* municipalities is precisely estimated and essentially unaltered if compared to the baseline specification. Summing up, having fewer local offices worsens tax enforcement in areas whose jurisdiction tax office is closed and there is no evidence that the reallocation of tax agents leads to countervailing effects on areas that are now served by a larger office.

Having shown that the reform reduces tax enforcement in *closing* municipalities, I now explore what drives this effect. While tax agents' ability to detect evasion using technology and data is unaffected by their location, their ability to gather local information may be. Therefore, we might expect stronger effects of office closings on enforcement in areas where local information is more valuable. Local information is more critical in settings with less third-party reporting (Jensen 2022). In this context, inspections by the Ministry of Labor ensure that firms' employees are registered, creating a trail of information on employee income and firm activity. This trail helps tax officials detect evasion even without local data. I use microdata on labor inspections from the years before the reform to classify municipalities based on the intensity of third-party reporting. Heterogeneity analysis shows that office closings have a stronger effect in municipalities where third-party reporting is less enforced and local information is more valuable.

Why do the closings make it harder to collect local information? One possibility is that tax

officials are now farther from the municipalities that they have to monitor, and distance makes it harder to visit these places and collect local information. To investigate the role of distance, I start by documenting that the reform increases the travel time between a *closing* municipality and the jurisdiction tax office on average by 50 minutes. I then employ an instrumental variable approach to isolate the effect of distance on tax enforcement. My empirical strategy leverages the variation, at baseline, in distance between a municipality and the closest *other* office in the same fiscal region. This distance does not matter for tax enforcement before the reform, as what matters is the actual distance from the jurisdiction tax office. However, for *closing* municipalities this distance becomes the lower bound on the actual distance from the jurisdiction tax office after the reform. Using this approach, I find that municipalities which end up one hour farther away from a tax office experience a 1.7% reduction in tax revenues.

I then turn to investigate how distance worsens tax officials ability to collect *local information*. One possibility is that it acts as a friction preventing tax officials to physically visit areas farther apart. I investigate whether this is the case using the number of trips carried out by tax officials from a local office to a municipality. I find that distance reduces the number of tax official trips to a municipality. Moreover, these trips tend to last longer and cost more. Distance makes visiting municipalities costlier both in terms of tax officials time and monetary terms.

Taken together, these findings provide some of the first evidence on how the organization of a tax authority across the territory impacts a government fiscal capacity. In this context, having fewer local offices is associated with a decline in tax revenues. In the last part of the paper, I perform a back of the envelope calculation in which I compare the foregone tax revenues to estimates of the administrative savings generated by the reform. On average, I find that the reform led to negative net effects. More generally, by shedding light on the forces behind this result, this paper suggests that policymakers interested in a similar reform should consider the relevance of collecting local information and the severity of geographical frictions in their context.

Related Literature This paper relates and contributes to various strands of the literature. First, it speaks to the literature on tax administration in developing countries (Pomeranz and Vila-Belda 2019; Okunogbe and Tourek 2024). Past work has studied the role of incentives for tax collectors (Khan, Khwaja, and Olken 2016, 2019), the type of agent hired (Balán et al. 2022), specialization

(Basri et al. 2021), and the potentialities of technology (Okunogbe and Santoro 2023; Dzansi et al. 2022; Knebelmann, Pouliquen, and Sarr 2023). This paper shows that the decision on how to organize the presence of tax authorities across a territory can affect their effectiveness.

Second, this paper is related to the literatures on the importance of third-party reporting (Pomeranz 2015; Naritomi 2019; Jensen 2022) and on the value of local information (Balán et al. 2022; Knebelmann, Pouliquen, and Sarr 2023) for taxation. It shows how a tax authority's ability to collect local information is constrained by the distance from taxpayers, and highlights that this ability is especially important in areas with limited third-party reporting.

Third, this paper contributes to the literature public sector personnel (Finan, Olken, and Pande 2017). While previous literature has investigated the role of selection (Dal Bó, Finan, and Rossi 2013; Ashraf et al. 2020), incentives (Bertrand et al. 2020), and managers (Fenizia 2022), this paper follows Besley et al. 2022 call to use microdata for examining the effects of system-wide reforms and the importance of different organizational designs. Since the reform examined here alters the size of tax jurisdictions, this paper also connects to the literature on decentralization which explores the effects of different polity sizes (Bardhan 2002; Narasimhan and Weaver 2023; Dahis and Szerman 2024).

Finally, this paper speaks to the literature on the relationship between distance from capitals and state capacity (Mann 1984; Herbst 2014; Müller-Crepon 2023). By focusing on a specific dimension of state capacity and leveraging detailed information on the locations of bureaucrats performing this function, I am able to provide causal evidence on how state capacity deteriorates with distance.

The rest of the paper is organized as follows: Section 2 describes the context and the tax authority reform, while section 3 introduces the data. Section 4 presents evidence on the effects of the reform on tax revenues, whereas Section 5 investigate the mechanisms behinds these effects. In Section 6 I perform a back of the envelope calculation of the net effects of the before, before concluding in Section 7.

2 — Context

This section provides institutional background on Brazil's economic, administrative, and fiscal structure. It also explains how the Federal Revenue of Brazil, the country's tax authority, operates and the impact of the 2020 reform.

2.1. Economic, Administrative, and Fiscal Structure in Brazil

Brazil is the fifth largest country by area and the sixth by population. In 2022, its GDP per capita in PPP was $19,132 USD. The country exhibits significant spatial disparities, with northern states having about half the GDP per capita of southern states (Amann, Azzoni, and Baer 2018). The economy is characterized by high informality: around 65% of firms, 40% of GDP, and 35% of employees operate in the informal sector. Notably, 40% of informal employees work in formal firms (Ulyssea 2018), and many formal workers receive part of their wages off the books (Feinmann, Rocha, and Lauletta 2022). Following strong growth in the early 2000s, Brazil experienced a recession from 2014 to 2016, driven in part by falling commodity prices and macroeconomic policy issues. It wasn't until 2021, after the COVID crisis, that GDP returned to 2014 levels (Spilimbergo and Srinivasan 2019). Brazil has three levels of government: federal, state, and municipal. As of 2024, there are 27 states[1] and 5,570 municipalities.[2] The provision of public services is highly decentralized, with municipalities responsible for essential services like education, health, and transportation. Funding for these services comes mainly from intergovernmental transfers from the state and federal governments. Most tax collection responsibilities lie with the federal government, which, as of 2021, manages 56% of total tax collection. This includes personal and corporate income taxes, social security contributions, VAT on gross revenues and manufactured products, financial transaction contributions, and taxes on net revenues. Smaller components include taxes on fuel, insurance, and rural land. States handle VAT on sales and services, vehicle taxes, and inheritance taxes, accounting for about 39% of total tax revenue. Municipalities collect urban property and service taxes, representing approximately 5% of total taxation.

1. 26 states and one federal district, which includes the capital, Brasilia

2. Five municipalities were created in 2013. I use the 2010 municipalities as a consistent unit of observation. For brevity, I refer to them simply as municipalities.

2.2. The Federal Revenue of Brazil

Federal tax collection is overseen by the Federal Revenue of Brazil (*Receita Federal do Brasil - RFB*), an agency under the *Ministério da Economia*. Established in 1968, the RFB manages tax administration, customs, and the fight against illicit trafficking (Ezequiel 2014, 2018). The RFB's primary roles are tax auditors and tax analysts. Tax auditors are the most prestigious and highest-paid. Salaries for tax officials account for around 65% of the RFB's operating expenditure (OECD 2023). These salaries are not performance-based, and officials have job security unless subject to severe disciplinary action. The RFB is composed of central units in Brasília and regional units across the country.

The regional structure has three tiers (see Figures **??** and **??**). First, the country is divided into 10 fiscal regions[3]. Each region is managed by a regional headquarters (*Superintendência Regional*). Second, each fiscal region is divided into tax jurisdictions covering several municipalities, managed by local offices (*Delegacia*). Third, within each jurisdiction, there are lower-tier offices (*Alfândegas, Inspetorias, Agências, Postos de Atendimento*). There is a clear hierarchy: regional headquarters oversee local offices and lower-tier offices in their fiscal region, while local offices, along with headquarters, oversee the lower-tier offices within their jurisdiction.

Tax enforcement responsibilities are shared between central units in Brasília, regional headquarters, and local offices (Receita Federal do Brasil 2020). Customs, illicit trafficking, and taxpayer services are shared between central units in Brasília, regional headquarters, local offices, and lower-tier offices. It's important to stress that lower-tier offices are not tasked with tax enforcement. Thus, since my analysis focuses on tax enforcement, I do not consider lower-tier offices here.

Large taxpayers are managed by specialized teams at regional headquarters or by two dedicated offices in São Paulo and Rio de Janeiro. Other taxpayers are handled by local tax offices, often in collaboration with regional headquarters. Tax enforcement for small taxpayers involves cross-checking information with tax returns, while enforcement for medium taxpayers follows two steps: initial selection of potential evasion cases and subsequent auditing.[4] Before 2017, selection

3. Fiscal regions typically span multiple states. While the borders of fiscal regions align with state borders, not all state borders define a fiscal region.

4. The definitions of small, medium, and large taxpayers are undisclosed.

occurred at local offices. Since 2017, selection teams specialized by tax type, rather than geographic area, operate across entire fiscal regions. Auditing is conducted by local offices, using hard data and external visits to gather additional information.

2.3. The reform

Background Between 2013 and 2019, the personnel of the RFB decreased by approximately 30% (see figure 3a). This decline can be attributed to two main factors (see figure 3b). (i) A cessation in hiring post-2016, likely in response to budget cuts mandated by the central government amid adverse macroeconomic conditions. (ii) A large increase in the number of officials departing, with a spike in 2019. This spike may be attributed to the aging workforce of tax officials and apprehensions regarding the implications of the 2019 pension reform, which introduced a minimum retirement age. Consequently, the size of local tax offices experienced a significant reduction against this backdrop (see figure 3d).

Reform The 2020 reform reshaped the regional component of the RFB by closing 24 local offices[5] (see figure 3c). Discussions about cost rationalization and organizational restructuring had been ongoing since at least 2015. However, the decline in budget and personnel allocated to each office likely played significant roles in triggering the 2020 reform[6]. Apart from closing local offices , the reform also led to the closure of other lower-tier tax offices and altered some jurisdiction boundaries even in cases where a tax office remained open.

Figure 4 illustrates schematically the reform . Based on which office they are assigned to at baseline, there are three groups of municipalities. First, municipalities whose jurisdiction tax office closes - the *closing* municipalities. Second, municipalities whose jurisdiction will incorporate the *closing* municipalities after the refrom - the *receiving* municipalities. Third, the municipalities whose jurisdiction will not incorporate the *closing* municipalities after the reform - the *unaffected* municipalities. Figure 4 describes the reform with data. The reform redraws jurisdiction boundaries and reallocates tax officials. *Closing* municipalities are assigned to a new jurisdiction tax office (figure 5a). Second, most tax officials are allocated to the office of the new jurisdiction (figure 5a).

5. See the law of 27 July 2020 at https://www.in.gov.br/en/web/dou/-/portaria-n-284-de-27-de-julho-de-2020-268758810

6. A smaller reform happened in 2017, closing three local offices and affecting 55 municipalities.

Indeed, 83% of tax officials from a closing offices move to the same tax office to which the majority of the jurisdiction municipalities are assigned after the closings [7] This reallocation of tax officials increase the staff size of the offices serving *receiving* municipalities (figure 5c). The map in figure 5c depicts which municipalities belong to each group. It shows that there is considerable regional variation in the effects of the reform. Overall, the reform leads to fewer, larger offices serving a wider jurisdiction.

3 — Data

This section introduces the data sources and the construction of the variables underlying my analysis.

Geographical units I use the 5,565 municipalities present in 2010 as the units of observation. The five municipalities created in 2013 are assigned to the 2010 borders based on their centroid. Each municipality belongs to a meso-region, which has no political or administrative significance but is defined by the statistical office to group municipalities with economic and social similarities within a geographic area of a Brazilian state. There are 137 meso-regions in Brazil. All GIS data are obtained from Pereira and Goncalves 2024. I compute travel distances between the main towns of each municipality using information from *Open Street Map*. I also compute the area (in squared km) for each municipality.

Internal organization of RFB I consult *Federal Revenue of Brazil* (RFB) internal regulations to reconstruct its organization and evolution over the period 2016-2022. Regulations on the organizational structure (*Regimento Interno*) report the names and the functions of the various departments (Ministério da Fazenda 2017, 2020). Moreover, they contain the lists of all the regional units. From these lists, I obtain the locations of the regional headquarters (*superintendências regionais da receita federal do brasil*) and the boundaries of the fiscal regions (*regiões fiscais*). These characteristics do not vary in the sample period. Moreover, I obtain the locations of the local tax offices (*delegacias da receita federal do brasil, delegacias especiais da receita federal do brasil*).

7. 8% move to an office in another layer (either central units, regional headquarters or lower-tier local offices), 4% leave the organization and 5% move to another local tax office.

I complement this organizational structure with regulations on the fiscal jurisdiction of each local tax office (Receita Federal do Brasil 2012). Every year there are many regulations altering these jurisdictions; thus, for each year I use the jurisdiction which is in place on August 1st. I choose August 1st because is the first day after firm's tax returns are due. Notice that, outside of the years in which there are office closings, the jurisdiction re-drawing are minimal and often these regulations only alter the jurisdictions of the lower-tier offices. As mentioned above, these lower-tier offices are not tasked with tax enforcement and thus are outside of the focus of this paper.

Tax collection I use data on federal tax collection at the municipality-year level for the period 2016-2022 from Receita Federal do Brasil 2023. For all municipalities, I have information on the total amount collected and the amount from social security contributions. For a sub-sample of larger municipalities, I have data on amounts collected from income tax (both on workers' income and firms' profits) and from IPI (a tax on inputs paid by firms). I deflate nominal values to 2018 prices. Since tax collection is highly skewed (figure 6), in the analysis I use its log as the main outcome. Additionally, I use data on the collection of municipal taxes (urban property tax - IPTU and tax on services - ISS) from the Brazilian Public Sector Accounting and Tax Information System (Siconfi) accessed through Base Dos Dados 2022.

Receita Federal Personnel data I use data on the universe of federal civil servants from the *Sistema Integrado de Administração de Pessoal* (SIAPE), a central system that unifies all platforms for managing the payroll of federal public servants. I access data for each January from 2013 to 2023. I retain all the civil servants whose reported organizational unit matches one of the units listed in the RFB organizational charts. In this way I obtain a dataset of all tax authority employees, consisting of 232,303 worker-year observations and 29,445 unique workers. As each worker has a unique identifier, I can track them over time and across organizational units. Additionally, I code information on the wage, managerial position, gross wage, and years of experience in the RFB.

I merge these data with the Cadastro de Expulsões da Administração Federal (Portal da Transparência 2023a), which contains information about civilian employees of the Federal Executive Branch who have been punished with dismissal, or revocation of retirement. This database

provides information only on the date the sanctioned was published, not on the description or date of the infraction. I match the sanctions to the SIAPE dataset using the name and the (partially disclosed) social security number. Using this procedure, I find 256 RFB workers who have been punished. 76 instances are revocations of retirement, whereas the others are about tax officials who are still working at the time of the sanction publication. I consider only tax officials who are still working at the time of the sanction.

Trips I use data on business trips undertaken by the civilian employees of the Federal Executive Branch from 2016 to 2022. These data come from the Transparency Portal of the government (Portal da Transparência 2023b). I merge these data with the RFB personnel data from SIAPE using the name and partially anonymized social security number of the bureaucrat who did the trip. This allows me to retain only the trips performed by RFB employees. I use the date to assign each trip to a specific year[8]. Additionally, I geo-reference the destinations and code the duration and cost of each trip. As the data do not report the origin of the trip, I use the office in which the bureaucrat is allocated as the origin.

Socio-economic characteristics I build measure of local economic structure using the 2010 demographic census (IBGE 2010; Base Dos Dados 2022). I compute employment shares by sector (agriculture, secondary, services), the share of formal employment (following the literature, I code a worker as formal if they have a labor card, pay social security contributions, or are employed in the public sector), and the average income. Data on the municipality population for 2010 are from the statistical office (IBGE 2023; Base Dos Dados 2022). I employ three time-varying measures of economic activity. I use estimates of municipality-level GDP (available up to 2021) from the statistical office (IBGE 2023; Base Dos Dados 2022). I also use data on the value of total agricultural production at the municipality level from IPEA 2023. I deflate nominal variables to 2018 prices. As an additional proxy of economic activity, I use VIIRS Nighttime Lights data from the Earth Observation Group (EOG 2023), aggregated at the municipality level.

8. To be consistent with the calendar of the jurisdictions and of the personnel data, I assign trips undertaken before August 1st to the previous year

Sample selection I exclude from the sample state capitals and large municipalities with more than 1 million inhabitants. Tax enforcement in these cities differs from that in other municipalities because they are larger, richer, and contain the bulk of large firms (which are not served by local offices). Additionally, I exclude municipalities that change jurisdiction during the period 2016-2022, even if their tax office is not closing. This is to avoid contamination of the analysis by these jurisdiction changes. I also exclude municipalities for which it is not possible to compute the travel distance along a road from their jurisdiction tax office or their closest other office, as these variables are used in the analysis as explanatory variables or controls. This leaves me with a panel of 5,395 municipalities.[9] When I discuss the results, I will show that they are not affected by the sample selection.

4 — The Effects of Closings: Findings

This section documents that the reform negatively impacts tax enforcement. I show this by comparing the evolution of tax collection in municipalities whose jurisdiction tax office closed after the reform (*closing* municipalities) with municipalities whose jurisdiction tax office did not close (*not closing* municipalities). I start by discussing the difference-in-differences design and the underlying identifying assumptions. I then present the main results on tax collection and provide supporting evidence for the validity of the empirical strategy.

4.1. Empirical Strategy: difference in differences

The hypothesis is that having fewer offices make it harder to monitor the local economy. To test this hypothesis, I compare the evolution of tax collection in municipalities where the jurisdiction tax office closed after the reform (*closing* municipalities) with municipalities where the jurisdiction tax office did not close (*receiving* and *unaffected* municipalities).

I employ a standard difference-in-differences design. The estimating equation is the following:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \beta \text{Closing}_{i(m)} \times \text{Post}_t + X_{i(m)t}\psi + \epsilon_{i(m)t} \tag{1}$$

9. 27 cities are state capitals, 2 additional municipalities are dropped because of the population, 8 because of missing travel time; the remaining are excluded because they change jurisdiction in the sample period.

where y_{it} is one of the outcomes of interest measured in municipality i, part of mesoregion m and in year t. I include a set of municipality fixed effects $\alpha_{i(m)}$, which capture time-invariant municipality-specific characteristics affecting tax enforcement, and of mesoregion-by-year[10] fixed effects α_{mt}, which account for regional time-varying shocks (e.g. economic shocks affecting a region). The matrix $X_{i,t}$ includes a set of municipality-specific controls that I will discuss when I introduce them. $\text{Closing}^{r}_{i(m)}$ is an indicator which takes value 1 if the jurisdiction tax office serving municipality i at baseline closes and 0 otherwise; Post_t is an indicator equal to 1 from 2020 onward. The main coefficient of interest is β, which captures the impact of closings on tax collection.
I also report results from the dynamic specification:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=-4}^{2} \delta_t(\text{Closing}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \tag{2}$$

Where D_t is an indicator for each year between 2016 ($t = -4$) and 2022 ($t = 2$). Here the coefficients of interest are the δ_t. They represent the difference between treated and untreated municipalities in outcome y in year t. I normalize to 0 the coefficient for the year before the offices close, δ_{-1}.

4.2. Results: the effects of closings on tax collection

Table 1 reports results from equation 1. Column 1 reports the specification without any control. It suggests that *closing* municipalities experience a 4% r reduction in tax collection after the reform. In order to assuage concerns about the effects being driven by characteristics of the municipalities, Column 2 includes a set of municipality characteristics interacted with year fixed effects. Specifically, I include controls for the size of a municipality (area and population in 2010), economic structure (employment shares in primary, secondary, and tertiary sectors in 2010), and level of economic development (average income in 2010). I also control for the distance (at baseline and interacted with year fixed effects) from the closest local tax office in a different tax jurisdiction. As local tax offices are usually located in cities, this distance allows controlling for time-varying effects of market access. In Column 3, I additionally control for the share of workers who are

10. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. There are 137 mesoregions in Brazil

formal[11] (from the 2010 census). In Column 4, I control for time-varying proxies of economic activity: nightlights and the value of agricultural production. Throughout these specifications, the estimate is statistically significant at the 5% level and it implies a 3.9% reduction in tax collection. In the rest of the analysis I employ the most restrictive specification (column 4) as my baseline one.

In figure 7 I report the dynamic specification from equation 2. We can appreciate that the effect of closings on tax enforcement is sudden and persists until the end of the sample period.

In table 2 I investigate whether the effect is driven by a specific type of tax. I focus on the types of taxes for which I have disaggregated data: social security contributions, income tax, and tax on firms' inputs (IPI). Note that the breakdown by type of taxes is not available for all municipalities, which leads to changes in sample size and composition. The estimates suggest similar effects on social security contributions and income tax. The point estimates are very similar to the coefficient obtained when looking at total tax collection. Within income tax, the effect seems to be driven more by taxes on firms' profits than by taxes on workers' income. The results are substantially larger for the tax on firms' inputs; however, for this type of tax, the sample size is considerably reduced and this estimate should thus be considered with caution.

4.3. Discussion and Sensitivity Analyisis

In order to interpret β or δ_t as the causal impact of closings on tax collection, I rely on two key identifying assumptions: no anticipation of the treatment and parallel trends. In other words, there was no change in tax enforcement in *closing* municipalities before the reform took place (no anticipation of the treatment), and - absent the offices closings - tax collection in *closing* would have evolved similarly to the untreated municipalities (parallel trends). While these assumptions are fundamentally untestables, visual inspection of the dynamic coefficients in figure 7 suggests that there is no anticipation nor differential pre-trends in tax collection.

The main concern about this assumption arises from the possibility that the reform closed offices in areas with a declining economy and tax base. However, note that the inclusion of mesoregion-by-year fixed effects absorbs regional economic shocks. Moreover, the inclusion of

11. Building on the existing literature, I code as formal the workers who report having a *work card*, paying social security contributions, or being employed in the public sector.

municipalities' economic structure characteristics interacted with year fixed effects controls for country-wide sectoral shocks (e.g., a negative shock to agricultural commodities exports), as well as for possible changes to the tax structure that are sector-specific (e.g., changes in income tax rates that only affect workers in agriculture). Additionally, the lack of pre-trends in tax collection suggests that *not closing* municipalities were not experiencing a decline in tax potential.

One may still worry about shocks occurring at the same time as the reform (e.g., the Covid pandemic having a differential impact on the economy of *closing* municipalities). To assuage this concern, I can estimate the dynamic specification from equation 2 using as dependent variable three different proxies of economic activity[12]. The first proxy is local GDP as estimated by the statistical office. Since this variable is not available for 2022 and there may be concerns that GDP estimates also depend on data provided by the tax authority, I employ two additional proxies: nightlights and the value of agricultural production. Nightlights have been extensively used by economists as measures of economic growth when sub-national data are unavailable or there are concerns about the reliability of official statistics (Henderson, Storeygard, and Weil 2012; Martinez 2022). One limitation of nightlights is that they are less sensitive to agricultural economic activity (Gibson et al. 2021). Therefore, I also use the value of agricultural production from the statistical office. Results are reported in Figure 8. We can see that, across the three proxies, there is no evidence of a differential decline in economic activity in treated municipalities. Additionally, as mentioned above, the baseline specification directly controls for time-varying nightlights and the value of agricultural production[13].

To further strengthen the credibility of the analysis, I perform a placebo exercise in which I show that closings do not negatively impact the collection of local taxes. These taxes are levied by municipal governments and primarily consist of taxes on urban property and services. Since they are not influenced by the federal tax authority, we would not expect to observe a decline if the empirical strategy is correctly identifying the causal impact of closings on tax enforcement. On the contrary, we might expect to see a decline if these municipalities were experiencing a decline in economic activity and their tax potential. Reassuringly, Figure 8 shows that local tax collection did not decrease in *closing* municipalities.

12. I include the full set of controls except, of course, the proxies of economic activity themselves.
13. I do not control for local GDP because it is not available for the last year of my sample period.

Finally, in table 3 I also check whether the results are sensitive to the sample selection. I find that both the point estimates and the precision of the coefficients remain unaltered.

4.4. Spillovers

One may be worried that the closings lead to spillovers in *receiving* municipalities, the ones whose tax jurisdictions incorporate the areas previously served by a closing office. This bias the estimated effect of closings. Notice that ex-ante it is not obvious in which direction the bias would be. On one hand, havignfewer offices may ease organizational frictions within the tax authority; moreover, *receiving* municipalities are now served by an office with a larger personnel size. These factors may improve tax enforcement and thus bias the closing effect by making it larger (more negative). On the other hand, after the reform *receiving* municipalities are served by an office which is responsible for monitoring a larger jurisdiction. This factor may worsen tax enforcement and thus bias the closing effect by making it smaller (less negative). In order to shed light on this, I augment equation 2 by estimating the dynamic impact on tax collection of being a receiving municipality. However, it is evident from figure 9 that when opening up the control group, *closing* and receiving municipalities are on a differential positive trend before the reform. I thus follow the approach in Dobkin et al. 2018; Freyaldenhoven et al. 2024. I control directly for a linear time trend specific to *closing* and *receiving* municipalities. Under the assumption that, absent the reform, the trend would have not changed, this allows me to identify the spillovers on receiving municipalities. Specifically, I estimate the following specification:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=0}^{2} \delta_t(\text{Closing}_{i(m)} \cdot D_t) + \sum_{t=0}^{2} \gamma_t(\text{Receiving}_{i(m)} \cdot D_t) + \beta_t \text{Closing}_{i(m)} \times t + \eta_t \text{Receiving}_{i(m)} \times t + + X_{i(m)t}\psi + \epsilon_{i(m)t} \quad (3)$$

where y_{it} is log of tax collection in municipality i and year t. $\text{Closing}^r_{i(m)}$ is an indicator which takes value 1 if the jurisdiction tax office serving municipality i at baseline closes and 0 otherwise; $\text{Receiving}^r_{i(m)}$ is an indicator which takes value 1 for municipalities whose jurisdiction receives municipalities from closing offices jurisdictions. T_t is an indicator for each period after the reform. $\text{Closing}_{i(m)} \times t$ and $\text{Receiving}_{i(m)} \times t$ capture groups-specific linear trends. Notice that the linear trend is fitted only on the pre-period. I include municipality and mesoregion by year fixed effects,

as well as all the controls of my baseline specification. Standard errors are clustered at the (pre-reform) jurisdiction level.

Table 4 reports the estimates for the average across the post-reform periods coefficients. Even if this exercise relies on strong assumptions, it suggests that if there are spillovers on *receiving* municipalities they are not positive. Thus they would bias the effect of closings towards zero. Moreover, notice that the coefficient on closing is precisely estimated and very similar to the baseline specification.

Overall, the results in this section show that having fewer offices make it harder to monitor the local economy. Municipalities previously served by a *closing* office experience a reduction in tax collection after the reform. In the next section, I investigate the role of local information in explaining this result.

5 — The Effects of Closings: Mechanisms

The previous section has shown that the closings of local offices affected negatively tax enforcement. What explains the decline in tax enforcement? In this section I provide evidence supporting the hypothesis that the decline is explained by a reduction in the ability to collect local information. First, I show that the effects of closings on tax collection are stronger in municipalities where local information is more valuable. Second, I show that the increased distance from local offices causes a decline in tax enforcement and reduces the number of visits from tax officials. Distance acts as a friction preventing tax officials to aquire local information.

5.1. The relevance of local information

If having fewer offices makes it harder to collect local information, one would expect the effects of closings on tax enforcement to be stronger in areas where local information is more valuable. There is ample evidence on how third-party reporting, by generating an information trail, makes tax enforcement easier (Pomeranz 2015; Jensen 2022). For instance, when firms report their employees' wages to the government, it is easier for tax officials to detect workers who are underreporting their income. This suggests that in areas with more third-party reporting, the value of local information is smaller. In the following, I leverage geographic variation in heterogeneity in third-

party reporting enforcement from the Labor Inspection Agency to investigate whether the effects of closings on tax enforcement are stronger in areas where local information is more valuable.

5.1.1. The Labor Inspection Agency

The Labor Inspection Agency, which is part of the Ministry of Labor, conducts inspections to enforce labor regulations. These inspections usually target formal firms because it is difficult to visit unregistered firms due to the lack of records of their activity. Compliance with all aspects of labor regulations is assessed. One such aspect is ensuring that workers are formal[14] (Almeida and Carneiro 2012). The presence of informal employees within formal firms is widespread (Ulyssea 2018). By enforcing worker formality, these inspections increase third-party reporting, generating an information trail on workers' income as well as on the firms' size and volume of activity.

5.1.2. Empirical Strategy and Results

I build a measure of third-party reporting enforcement at the municipality level by leveraging microdata on the universe of labor inspections carried out by the Labor Inspection Agency. I count the number of inspections conducted in a municipality in the years before the RFB reform (2016–2019). I then define municipalities as *high labor enforcement* if they received more inspections than the median (or the 75th percentile) municipality in their mesoregion. With this measure at hand, I test for heterogeneity in my baseline difference-in-differences specification:

$$y_{i(m)t} = \alpha_{i(m)s} + \alpha_{mt} + \beta_1 \text{Closing}_{i(m)} \times \text{Post}_t + \\ \beta_2 \text{Closing}_{i(m)} \times \text{Post}_t \times \text{High Labor Inspection Intensity}_t + X_{i(m)} \cdot \alpha_t + \epsilon_{i(m)t} \quad (4)$$

The hypothesis is that the effects of closings are stronger where local information is more valuable and attenuated ($\beta_2 > 0$) where there is more third-party reporting enforcement.

I report results in table 12. The effect of RFB office closings on tax collection is negative, but it is attenuated in municipalities with high labor inspection intensity. This is true irrespective of whether *high labor inspection intensity* is defined using the median (column 1) or the 75th

14. That is, they enforce that workers are registered and have a regular *work card*.

percentile (column 2). I also report the dynamic specification in figure 15. This helps to see that there is no evidence of differential pre-trends between the two groups, whereas the divergence in tax collection after the reform is sudden and stark.

Summing up, this section has shown that the effects of closings are attenuated in municipalities with more third-party reporting enforcement. This highlights the relevance of local information in explaining why having fewwer offices worsens tax enforcement. Additionally, this exercise underscores the possibility of complementarities between different government agencies and departments (Besley et al. 2022).

5.2. The role of distance

I have provided evidence that an increased difficulty in collecting local information is relevant for explaining the decline in tax enforcement after the closing of local offices. Why does closing offices make it harder to collect local information? This section investigates whether distance from a tax office acts as a friction in tax officials' ability to monitor the local economy. I start by documenting how the reform increased the distance between municipalities and their jurisdiction tax office. I then discuss the empirical challenges associated with estimating the causal effect of distance on tax enforcement. After introducing an instrumental variable to circumvent these challenges, I show how distance impacts tax enforcement. Finally, to better understand how distance matters, I provide evidence of how it affects tax officials' trips to municipalities

The reform increases the distance from local tax offices I report the dynamic coefficients from equation 2 using distance between a municipality and the jurisdiction tax office as an outcome. Figure 10c shows that the distance between *closing* municipalities and their jurisdiction tax office increased on average by 1.5 hours after the reform.

5.2.1. Empirical Strategy

The goal of this section is to study how the distance between a tax office and different municipalities impacts tax enforcement. I am interested in estimating the following regression model:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \gamma Distance_{i(m)t}^{i \to r} + X_{i(m)t}\psi + \varepsilon_{i,t} \tag{5}$$

where $y_{i(m)t}$ is one of the outcomes of interest measured in municipality i, mesoregion m and year t. I include a set of municipality fixed effects $\alpha_{i(m)}$, which capture time-invariant municipality-specific characteristics affecting tax enforcement, and of mesoregion-by-year fixed effects α_{mt}, which account for regional time-varying shocks (e.g., for economic shocks affecting a region). The matrix $X_{i(m)t}$ includes the same set of municipality-specific controls as in the baseline specification of the previous section. $Distance_{it}^{i \to r}$ is the distance (in hours of travel) between a municipality i and the jurisdiction tax office r in year t. The coefficient γ captures whether outcome y_{it} changes differentially in municipalities that become farther apart from the jurisdiction tax office.

Tax office locations and tax jurisdiction boundaries are not random. For instance, tax offices may be located close to municipalities with higher economic activity; similarly, municipalities with higher economic activity may be assigned to the jurisdiction of an office that is closer to them. As $Distance_{it}^{i \to r}$ varies over time in the sample because of the tax offices closings in 2020, I can include municipality fixed effects to assuage the time-invariant component of these concerns. Yet, one may be worried that the decision of closing specific offices and the assignment of municipalities to a jurisdiction after the closings is correlated with the error term.

Research design In order to assuage the concerns outlined above, I build an instrument for $Distance_{it}^{i \to r}$. The first stage equation is:

$$Distance_{it}^{i \to r} = \iota_1 Distance_{i(-1)}^{i \to \bar{r}} + \iota_2 Distance_{i(-1)}^{i \to \bar{r}} \times \text{Closing}_{i(m)} \times \text{Post}_t + \varepsilon_{i,t} \tag{6}$$

where $Distance_{i(-1)}^{i \to \bar{r}}$ is the distance, at baseline, between a municipality and the closest tax office in another tax jurisdiction but in the same fiscal region[15]. $\text{Closing}_{i(m)}^{r}$ is an indicator which takes value 1 if the jurisdiction tax office serving municipality i at baseline closes and 0 otherwise; Post_t is an indicator equal to 1 from 2020 onward.

The instrument leverages the natural variation, at baseline, in distance between a municipality and other tax offices; this distance becomes relevant only if and after the tax office serving a municipality closes. 11 provides an illustration of how the instrument works. Each municipality (stylized as a town in the figure) is part, at baseline, of a tax jurisdiction (denoted by different colors)

15. Notice that a municipality will always be assigned to a tax office in the same fiscal region and that fiscal regions borders do not vary other time

and is served by the corresponding office (dots in the figure). For each municipality, I compute the distance from the closest tax office in another tax jurisdiction but within the same fiscal region.$Distance^{i\to\bar{r}}_{i(-1)}$ (dashed lines in the figure). After the reform, this distance becomes more relevant for municipalities served by closing offices (the ones with a cross on them). Moreover, notice that for municipality A, the closest other office would be located in a different fiscal region. Since municipalities cannot be assigned to an office in a different fiscal region, the instrument will not consider that distance. However, cases like municipality A allow me to control for time-varying effects of distance from the closest other office in *any* fiscal region. This constitutes a proxy for market access: fiscal region boundaries do not matter for goods trade, but they do matter for tax enforcement. This allows me to isolate the effect of distance between a municipality and the local tax office from the effects of geographic remoteness.

This instrument addresses concerns about endogenous jurisdiction assignments after the closings. For instance, municipalities with declining tax potential could be assigned to an office farther away. Because it uses baseline distances, it also addresses the concern that municipalities tend to experience a larger increase in distance if many closings happen in a region (e.g., because a region has a declining tax potential).

First Stage The relevance of the instrument can be visualized in figure 12. The distance, at baseline, between a municipality and the closest tax office in another tax jurisdiction but in the same fiscal region, $Distance^{i\to\bar{r}}_{i(-1)}$, is positively associated with the distance between a municipality and the jurisdiction tax office, $Distance^{i\to r}_{it}$, for all the municipalities, but this relationship becomes steeper after the reform for municipalities whose jurisdiction tax office close. Column 1 of table 5 presents results from estimating the first stage equation 6. As expected, both coefficients are large and positive. The Wald test on the joint nullity of the coefficient is very high. In column 2, I include municipality and mesoregion-by-year fixed effects, as in the baseline specification. Of course this absorbs the non-interacted term. The coefficient on the interacted term remains positive and significant. Additionally, column 3 shows that there is some (negative) correlation between the distance component of the instrument and the likelihood that the jurisdiction tax office closes, but this relationship disappears once one controls for region fixed effects (column 4).

Additional threats to identification The reform not only changes the distance between tax offices and municipalities, but it also redistributes personnel across offices. This affects both the number and characteristics of the staff serving a region, potentially confounding the results. To ensure that the research design is isolating the effect of distance, I conduct several additional sensitivity analyses

As a first step, I run the analysis only on the sub-sample of municipalities whose tax office closes (the *closing* municipalities). This rules out the possibility that the results are driven by frictions associated with changing office locations other than distance. All the municipalities in this sub-sample experience these other frictions, but they vary in their distance from the tax office. Moreover, this addresses concerns any spillover on *receiving* municipalities.

Second, I additionally control for the jurisdiction to which a municipality is assigned (after the closings) interacted with year fixed effects. This absorbs jurisdiction-specific shocks (such as changes in staff size, quality, and jurisdiction area). The variation comes from municipalities that experience the closing of the jurisdiction office, end up in the same jurisdiction, but at a different distance from the tax office. Third, I can include baseline jurisdiction by post-closing jurisdiction by year fixed effects. The variation comes from municipalities that change office due to the closings, are initially in the same jurisdiction, and end up in the same jurisdiction but at a different distance from the tax office. This addresses concerns related to frictions in switching offices that are specific to the origin-destination pair. One such example would be that the number of tax officials from the previous office differs across the new offices.

5.2.2. Results: Effects of distance on tax enforcement

Having shown above the relevance of the instrument, I employ it to estimate equation 5. Results are displayed in table 6. I report the panel specification, the reduced form, and the 2SLS. Reported standard errors are clustered at the level of the baseline tax jurisdiction. Notice that the F-statistic on the first stage is 29.6, above the standard threshold. The coefficients suggest a negative effect of distance on tax collection. The baseline specification with all the controls (column 7) implies that one additional hour of distance from a tax office causes a 1.7% decline in tax collection. I also display the dynamic coefficients of the reduced form in figure 13. One can appreciate the lack of pre-trends; this strengthens the validity of the exclusion restriction assumption.

I then explore the possibility that the effects of distance may be non-linear. I compute different quantiles for the instrument. That is I consider various quantiles of the distance between a municipality and the closest tax office in another tax jurisdiction but in the same fiscal region $Distance^{i\to\bar{r}}_{i(-1)}$; I then interact the indicator for these quantiels with *Closing Post*. I consider various number of quantiles, from 2 to 5. In table 7 I report the reduced form estimates. One can appreciate how the effect is negative and sizable for all the quantiles. Also municipalities that are closer to the other tax office (*Q1* in the table) experience a decline in tax collection[16]. While this effect is not statistically significant at the conventional level in the most disaggregated specification, the direction of the coefficients suggests that distance does not explain the whole effect: there is a decline in tax collection also in those municipalities that are very close to the tax office. This decline could be due to frictions associated with switching office. That said, distance *does* matter. The effect is larger for municipalities farther away. In the most disaggregated specification, the decline in tax collection after the closings for municipalities in the "farthest" quantile (*Q5*) is almost three times the decline for municipalities in the "nearest" quantile (*Q1*).

In order to assuage concerns about the empirical strategy picking up effects other than distance, I report the reduced forms and 2SLS estimates for the additional specifications described above in table 8. For these specifications, I cluster standard errors at the municipality level. This is for two reasons. First, the the treatment (distance) varies at the municipality level. Second, because there are only 24 tax jurisdictions in the *closing* sample, there would be too few groups to cluster at the baseline tax jurisdiction level. Notice that these specifications are very demanding because of the combination of numerous fixed effects and smaller sample. Yet, coefficients are always large, precisely estimated and negative.

This part of the analysis suggests that distance from the jurisdiction tax office impacts negatively tax enforcement. In the next section, I try to understand how distance matters.

16. Notice that the reference category in this regression are the municipalities whose jurisdiction tax office does not close.

5.2.3. How does distance matter? Effects of distance on tax officials trips

The previous section has shown that distance causes a decline in tax enforcement. Given that in this context taxes are filed electronically, collected through the bank network and technology is vastly used for tax auditing, this result is not obvious ex ante. It suggests that distance acts as a friction preventing tax officials to collect additional *local information*.

One way in which tax officials can collect this *local information* is by physically inspecting firms premises. While I do not have data on these inspections, I use information on the trips realized for work by tax officials as a proxy. I restrict the sample to the trips undertaken by tax officials allocated to local tax offices[17] and count the number of trips received at the municipality-year level.

The first three columns of table 9 show that distance does not affect (or if anything slightly increases) the likelihood that a municipality receives a trip from a local tax office. However, conditional on receiving a trip, municipalities farther away receive less trips (column 4 to 6). Columns 7 and 8 combine the extensive and intensive margin by estimating a Poisson regression (notice that here I do not report te 2SLS as the first stage is linear but the second stage is non-linear). Higher distance from a tax office decreases the number of tax officials trips. In figure 14 I report the dynamic specifications of the reduced form.

Finally, in tables 10 and 11 I employ the same empirical strategy to investigate how distance affects other characteristics of tax officials' trips. Specifically, I show that both the average and total cost of these trips are higher when involving municipalities farther from the local tax office. I find qualitatively similar, though noisier, results when examining the duration of trips in days: visits to farther municipalities tend to last longer. Since the data only report the start and end dates of each trip, I cannot determine whether the longer duration is due to more time spent in the municipality or more time spent traveling there. Overall, these results suggest that greater distance increases the cost of trips, both in terms of monetary expenses and the time tax officials inves

17. In the analysis I focus on trips from a local office to a municipality. A trip is defined as a trip from a local office if the tax official undertaking the trip was located in a local tax office that year. That is, I do not consider trips undertaken by tax officials located in headquarters or lower tier offices.

6 — The net effects of the reform

In this section, I investigate the net effects of the reform. First, I estimate the change in tax collection due to the closing of local offices. Second, I estimate the savings in administrative costs due to the reform.

Net effects of reform on tax revenues In section 4 I have shown that the reform led to a decline in tax collection in municipalities which were served by a closed office, as well as to negative spillovers in areas which receive more tax agents and a larger jurisdiction after the reform. To capture both effects, I pool *receiving* and *closing* municipalities together and label their jurisdictions as *centralized*. I compare the evolution of tax collection in these *centralized* municipalities and in those whose tax jurisdiction was unaffected by the reform.

I estimate the equivalent of 2 but with an indicator for being part of a *centralized* jurisdiction. However, figure 18 suggests that *centralized* municipalities were on differential trends in tax collection before the reform. Similarly to what I have done for the spillover analysis, I thus follow the approach in Dobkin et al. 2018; Freyaldenhoven et al. 2024. I control directly for a linear time trend specific to *centralized* municipalities. Under the assumption that the trend is linear, and, absent the reform, it would have not changed, this allows me to identify the causal effects of centralization on tax collection. Specifically, I estimate the following specification.

$$y_{i(m))t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=0}^{2} \beta_c \text{Centralized}_{i(m)} \times \text{T}_t + \beta_t \text{Centralized}_{i(m)} \times t + X_{i(m)} \cdot \alpha_t + \epsilon_{i(m)t} \quad (7)$$

where y_{it} is one of the outcomes of interest measured in municipality i and year t. $\textit{Centralized}_{i(m)}$ is an indicator equal to 1 if a municipality is part, after the reform, of a jurisdiction which underwent a centralization. T_t is an indicator for each period after the reform. $\text{Centralized}_{i(m)} \times t$ captures a linear trend specific to *centralized* municipalities. Notice that the linear trend is fitted only on the pre-period (because of the inclusion of the interaction between $\text{Centralized}_{i(m)}$ and each post-reform period T_t). I will report $\textit{Centralized}_{i(m} \times \textit{Post}_t$, the average across the post-reform

periods coefficients. I include municipality and mesoregion by year fixed effects, as well as all the controls of my baseline specification. Standard errors are clustered at the (post-reform) jurisdiction level.

Column 1 in table 13 reports the results for the specification with no controls, whereas column 2 includes the same controls of the baseline specification. The average of the dynamic coefficients would imply a 3% reduction in tax collection; the coefficient is not statistically significant at the conventional levels. To obtain an estimate of how much tax revenues were foregone by the reform, I multiply the average change in tax revenues[18] by 2,664 - the number of *centralized* municipalities. The estimated foregone revenues are 20,911,368,861 Br$. The 95% confidence interval for the change in tax revenues is between -48,344,398,143 Br$ and 7,695,525,080 Br$.

Savings from the reform In this paragraph I estimate the administrative savings originated from the reform. As before, I compare *centralized* and *not centralized* jurisdictions. I start by investigating how the reform affected staff size, the number of managers, and the wage bill. Figure 19 displays that the number of tax agents decline *before* the reform and remains essentialy constant thereafter (top-left panel). Instead, the number of managers falls *after* the reform. By closing local offices, the tax authority is able to save on the number of managerial positions. This reduction translate in a reduction in the wage bill, both in total (bottom-left panel) and for managers (bottom-right panel). On average, each centralized jurisdiction save $5,740,671$ Br$ in wages. As there are 24 *centralized* jurisdictions, the total savings in wage bill are 137,776,103 Br$, with a 95% confidence interval between 11,738,533 Br$ and 263,813,674 Br$. Additionally, one may think that there are overhead costs that are saved by closing offices. From (OECD 2023), I know that for the entire tax authority wage bill amounts to 65% of the total costs. I use this proportion and the average wage bill for each of the 24 closing offices in the years before the closings to back up an estimate of saved overhead costs (54,544,270 Br$).

Comparing effects on revenues and savings Putting these estimates together we have that on average the reform costed 20,911,368,861 Br$ in foregone revenues and saved 192,320,373 Br$, with a total negative effect of 20,719,048,488 Br$. However, given the uncertainty in the estimates,

18. $e^{-0.0308} * \bar{\text{Tax Collection}}$, where the average tax collection in a municipality year in the sample is 258,802,516 Br$

when considering the upper bound on tax revenue change and the upper bound on wage bill savings, the total effect could also be positive (8,013,883,024 Br$).

7 — Conclusion

This paper investigates how thow the organization of a tax authority across the territory impacts a government fiscal capacity. Leveraging a reform of the Federal Revenue of Brazil as a natural experiment, I document how having less local offices across the territory makes it harder to monitor the local economy and to collect local information.

Policymakers interested in a similar reform should consider the characteristics of their context. This paper suggests that important characteristics to consider are the value of local information and prevalence of third party reporting, as well as the severity of geographical frictions which make collecting local information harder.

While this paper has studied the impact of having fewer local offices in the context of a tax authority, many other public sector agencies address face the problem of how to organize their presence. Examples include agencies tasked with monitoring compliance with environmental regulations and agencies tasked with deliverying social assistance programs. These agencies need to collect local information to operate effectively. Investigating the effect of different organizational choices and how they are shaped by monitoring technologies is an interesting area for future research.

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8 — Figures and tables

Figure 1 — Schematic representation of the regional structure of the RFB

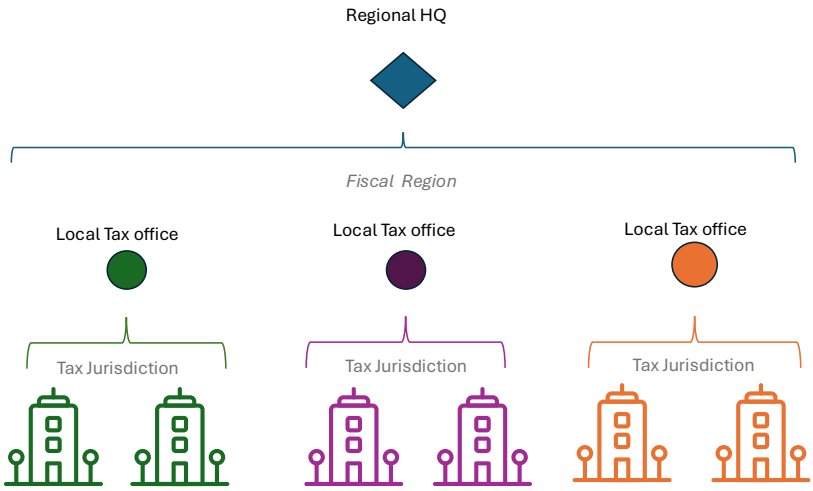
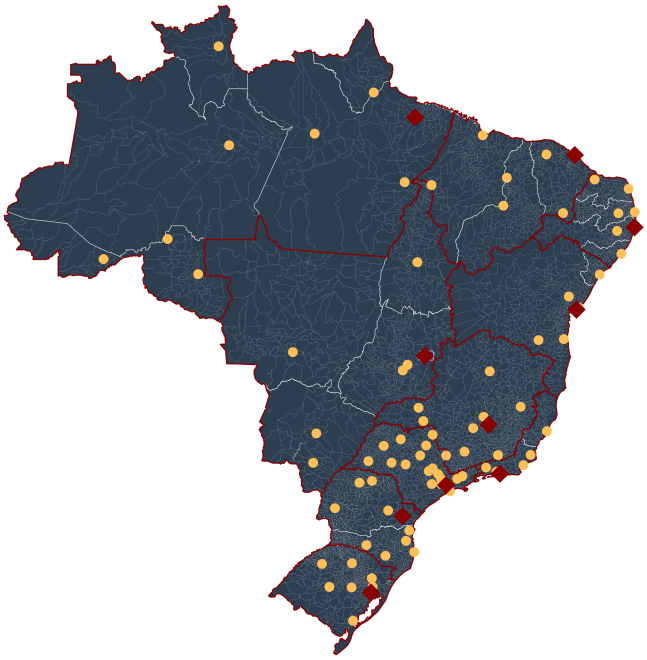
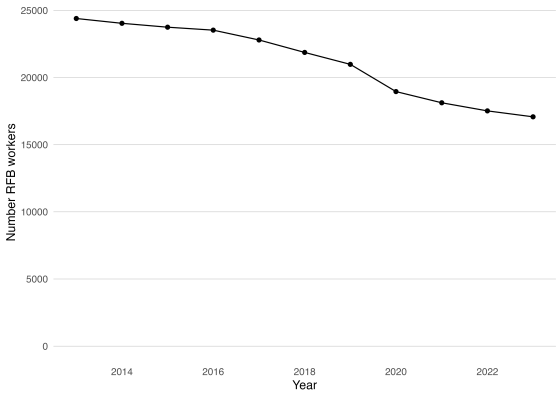


Figure 2 — Map of RFB regional units

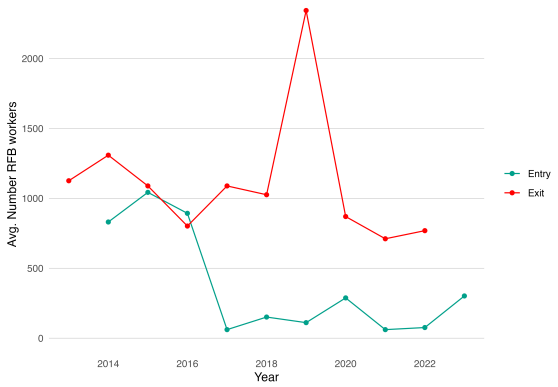


Notes. This map displays the 10 fiscal regions boundaries (red lines), regional headquarters (red diamonds) and local offices (yellow dots). Thick white lines represent states borders. Thin white lines represent municipalities borders.

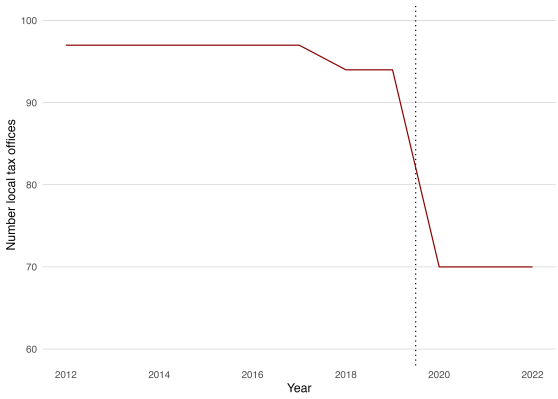
Figure 3 — The evolution of RFB



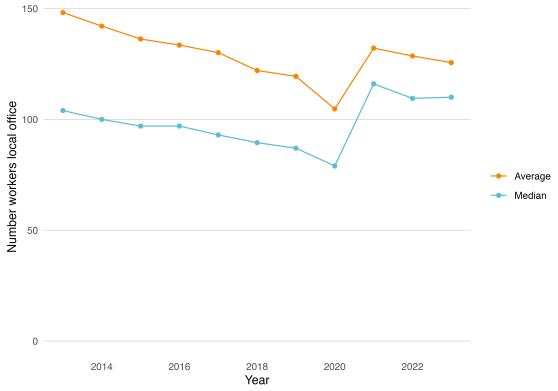
(a) Number of tax officials



(b) Number of tax officials entering and leaving



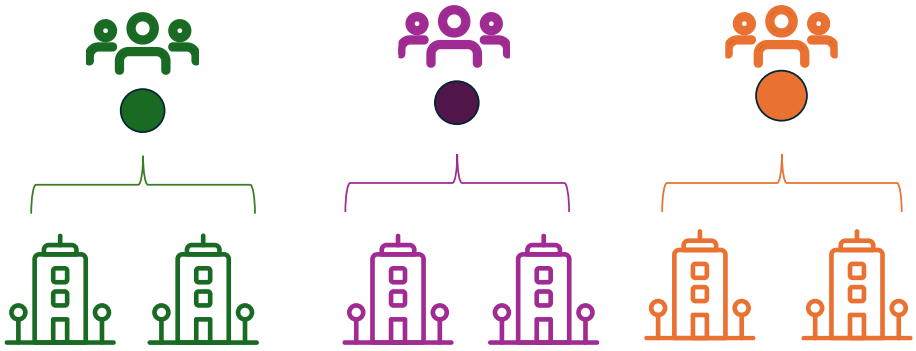
(c) Number of local tax offices



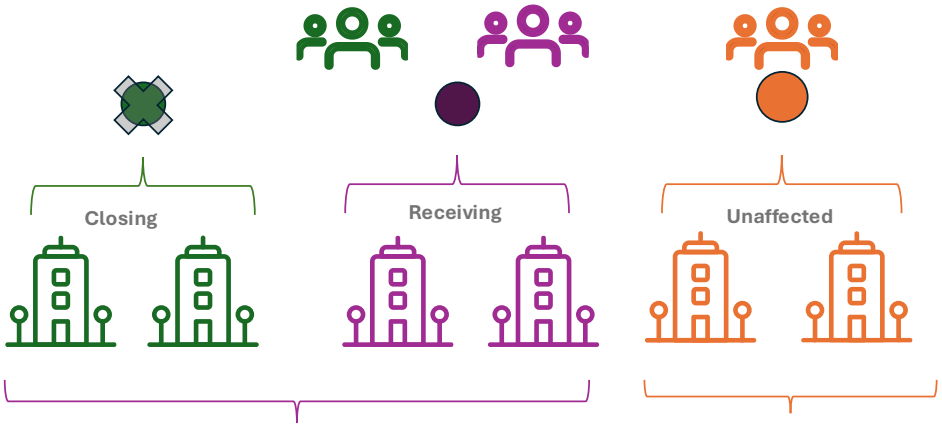
(d) Size of local tax offices

Notes. These graphs describe the evolution of the tax authority over time. The number of tax officials decline over time (top-left), due to retirements and a freezing in new hiring (top-right). The 2020 reform, perhaps triggered by the decline in number of tax officials, closes 24 local offices (bottom-left). Because of these closings, the average size of local offices bounces back up after a few years of decline (bottom-right).

Figure 4 — Illustration of how the reform affects different municipalities



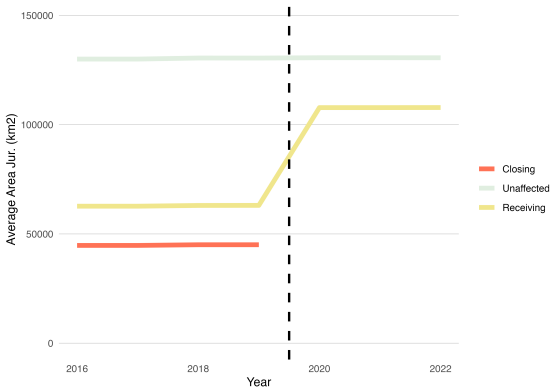
(a) Baseline



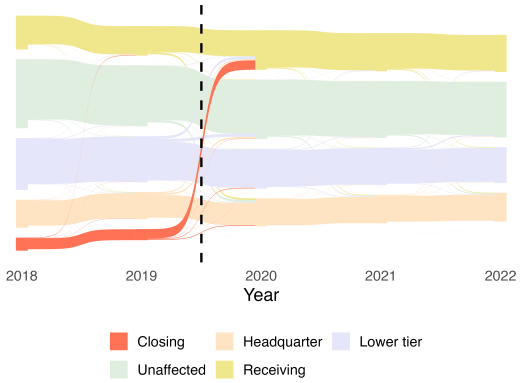
(b) Post reform

Notes. Schematic illustration of how the reform affects different municipalities. *Closing* municipalities are the ones whose jurisdiction tax office closes in 2020. *Centralized* jurisdictions are the ones which receive municipalities and tax officials from the closing offices.

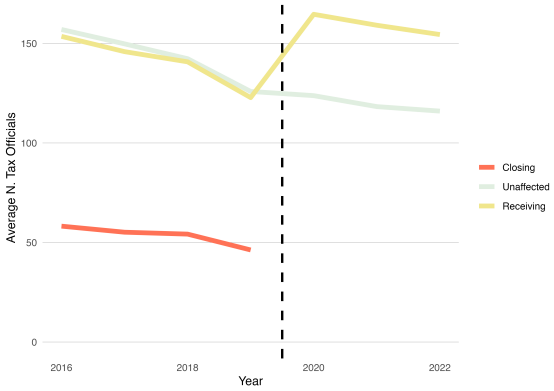
Figure 5 — The reform



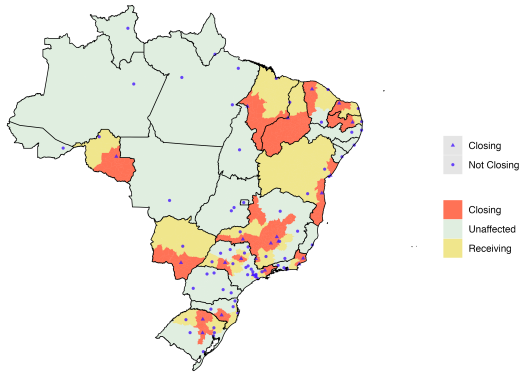
(a) Municipalities served by closing offices are assigned to some of the remaining offices



(b) Most tax officials from closing offices reallocated to a *receiving* office



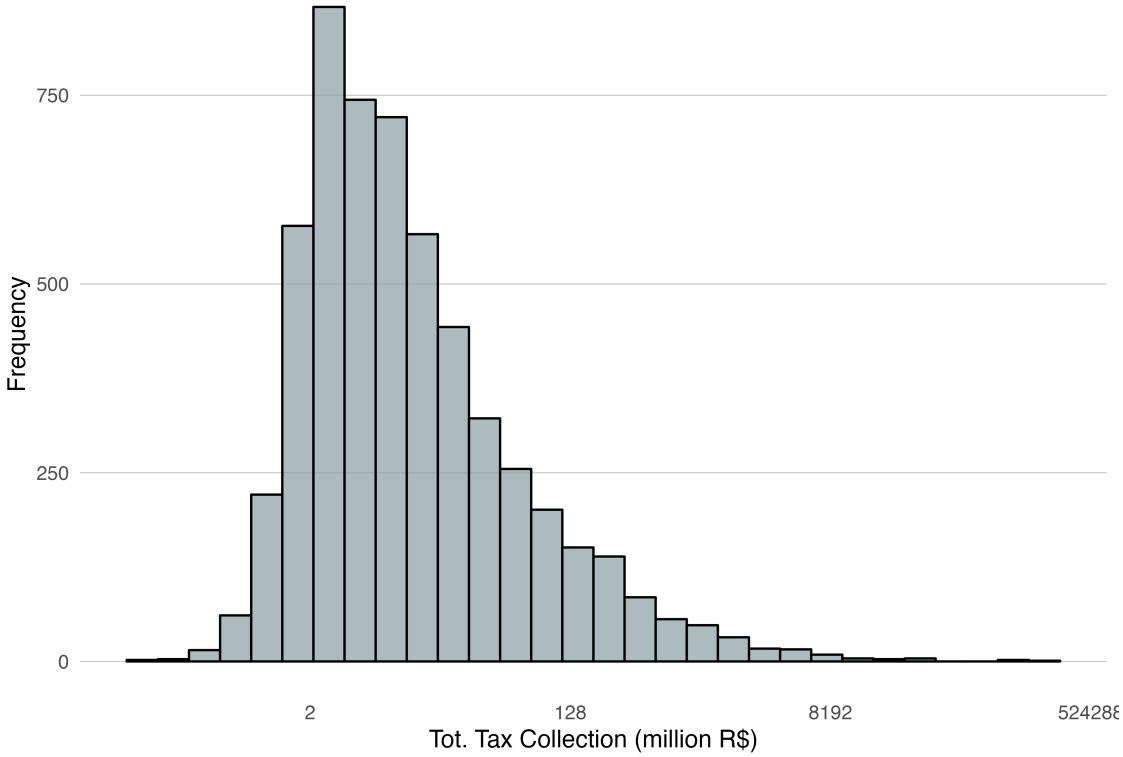
(c) Reallocation of tax officials increases personnel size in *receiving* offices



(d) Map of municipalities by group

Notes. These graphs illustrate the reform. Municipalities are assigned to the jurisdiction of the remaining offices. Tax officials from closing offices are assigned to the remaining offices, increasing their size. The bottom right panel display how each municipality is affected by the reform.

Figure 6 — Federal tax collection in 2016 by municipality



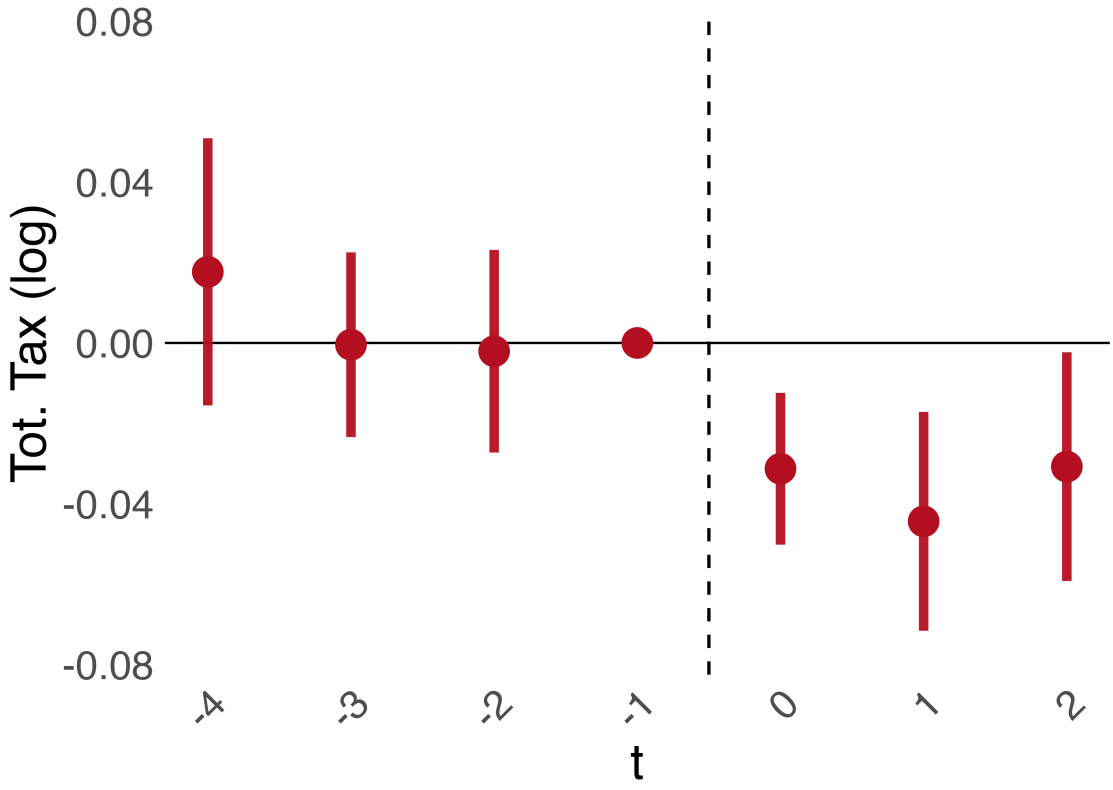
Notes. Federal tax collection (in millions of 2018 R$) in each of the 5,565 municipalities in 2016. As tax collection is highly skewed, the plot reports the log of tax collection (the horizontal axis reports the non-logged values).

Table 1: Closings reduce tax collection in affected municipalities

	Tot. Tax (log)			
	(1)	(2)	(3)	(4)
Closing Post	-0.040**	-0.039**	-0.039**	-0.039**
	(0.016)	(0.015)	(0.015)	(0.015)
Municipality FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls		Socio-econ	+ Formal	+ Econ Act.
Dep. Var. Mean	16.3	16.3	16.3	16.3
R^2	0.99	0.99	0.99	0.99
Clusters	91	91	91	91
Observations	37,751	37,751	37,751	37,751

Notes. Dependent variable is the log of federal tax collection. *Closing Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction tax office closes in 2020. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *Socio-econ controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access). *Formal controls:* formality share from 2010 census interacted with year fixed effects. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 7 — Dynamic effects of closings on tax collection



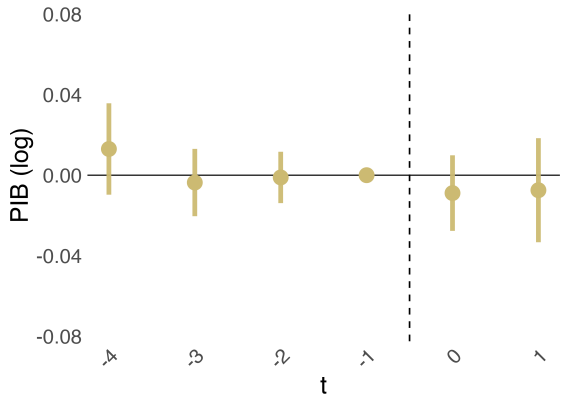
Notes. Dynamic version of column 4 of table 1. *Closing:* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020.

Table 2: Closings reduce tax collection in affected municipalities: type of tax

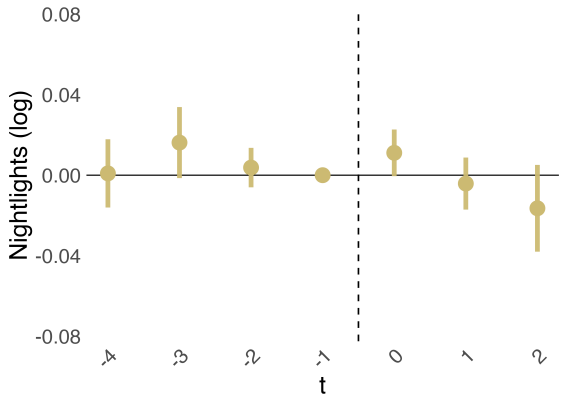
	SS Tax (log)	Inc. Tax (log)	Inc. Tax PF (log)	Inc. Tax PJ (log)	IPI (log)	Other Tax (log)
	(1)	(2)	(3)	(4)	(5)	(6)
Closing Post	-0.040***	-0.031*	-0.005	-0.044**	-0.258**	-0.074
	(0.015)	(0.019)	(0.015)	(0.021)	(0.122)	(0.061)
Municipality FE	✓	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓
Controls	All	All	All	All	All	All
Dep. Var. Mean	15.9	14.1	13.2	13.8	13.1	16.4
R^2	0.98	0.98	0.98	0.98	0.99	0.99
Clusters	91	91	91	91	91	91
Observations	37,751	37,741	34,838	34,834	10,000	10,001

Notes. Dependent variable is the log of collection of different types of federal tax. *Closing Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction tax office closes in 2020. The types of taxes considered from column 1 to 6 are: social security contributions, income tax (on either firms or workers), income tax on workers, income tax on firms, tax on firms inputs (IPI), other taxes (residual category ontained by subtracting social security, income tax and IPI from total collection). Notice that the number of observations changes a lot across columns because of missing values in the dependent variable. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *Socio-econ controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access). *Formal controls:* formality share from 2010 census interacted with year fixed effects. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

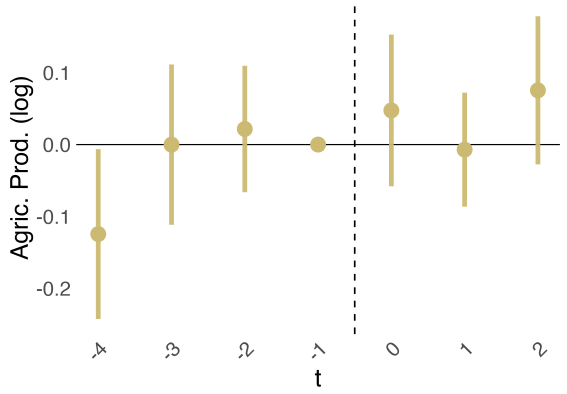
Figure 8 — Trends in economic activity & Placebo with local taxes



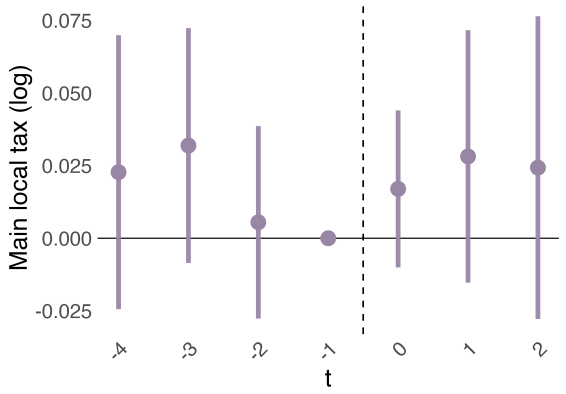
(a) GDP



(b) Nightlights



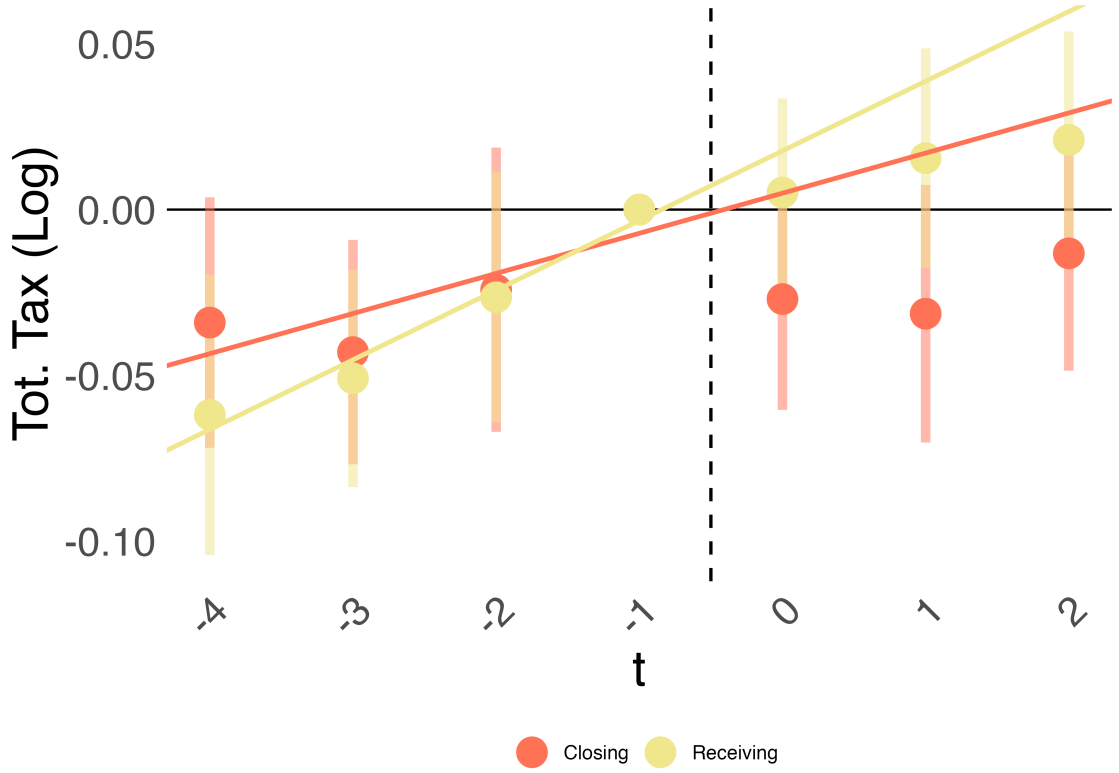
(c) Value Agricultural Production



(d) Local tax collection

Notes. I display dynamic coefficients for the correlation between *Closing*, and local economic activity. *Closing:* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. I use three proxies of economic activity. Local GDP estimates provided by the Brazilian statistical office (available up to 2021); nightlights; local value of agricultural production provided by the Brazilian statistical office. The bottom right panel displays dynamic effect on municipalities local tax collection - which we expect to be unaffected by federal tax enforcement. All the dependent variables are logged. *Fixed effects:* municipality and mesoregion-by-year fixed effects. All specification include *Socio-econ controls* and *Formal controls*. The bottom right panel includes also *Economic activity*. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings.

Figure 9 — Closings and spillovers on tax collection in receiving municipalities



Notes. Dependent variable is the log of federal tax collection. Dynamic coefficients for *closing* and *receiving*. *Closing:* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving:* indicator equal to 1 for municipalities whose jurisdiction receives municipalities from closing offices jurisdictions. The right panel also includes a line fitted on the pre-periods coefficients. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *Socio-econ controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access). *Formal controls:* formality share from 2010 census interacted with year fixed effects. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings.

Table 3: Closings reduce tax collection in affected municipalities: alternative samples

	Tot. Tax (log)			
	(1)	(2)	(3)	(4)
Closing Post	-0.039**	-0.039**	-0.039**	-0.039**
	(0.015)	(0.015)	(0.015)	(0.015)
Municipality	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls	All	All	All	All
Sample	Main	+ Large	+ Capitals	+ Change
Dep. Var. Mean	16.3	16.3	16.4	16.4
R^2	0.99	0.99	0.99	0.99
Clusters	91	94	94	94
Observations	37,751	37,856	37,954	37,968

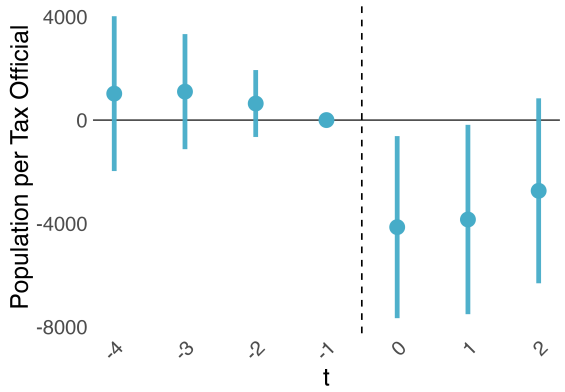
Notes. Dependent variable is the log of federal tax collection. Column 1 reports the baseline sample for reference. Column 2 includes municipalities with a population larger than 1 million. Column 3 includes state capitals and Brasilia. Column 4 includes the municipalities which experienced a change in tax jurisdiction underelated to the closings in the analyisis time-window. Notice that the only municipalities not included in column 4 are the ones for which is not possible to compute travel distance on road from the closest tax office - that is one of the controls. *Closing Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction tax office closes in 2020. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *Socio-econ controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access). *Formal controls:* formality share from 2010 census interacted with year fixed effects. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 4: Closings and spillovers on tax collection in receiving municipalities

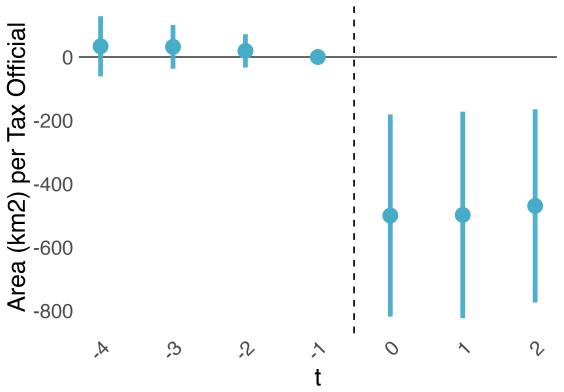
	Tot. Tax (log)
Closing × Post	-0.041*
	(0.021)
Receiving × Post	-0.025
	(0.021)
Municipality	✓
Region-Year FE	✓
Controls	All
Dep. Var. Mean	16.3
R^2	0.99
Clusters	91
Observations	37,751

Notes. Dependent variable is the log of federal tax collection. Because figure 9 suggests that *closing* and *receiving* municipalities are on differential trends, I include a linear trend specific for each of the two groups. In order to fit this trend only on the pre-reform period, I include both the linear trend and the interaction between a group indicator and each post-period year. I then compute the reported coefficients as the average between the three coefficients. Standard errors are obtained from a t-test on this average coefficient being equal to 0. *Closing Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction tax office closes in 2020. *Receiving Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction receives municipalities from closing offices jurisdictions. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *Socio-econ controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access). *Formal controls:* formality share from 2010 census interacted with year fixed effects. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

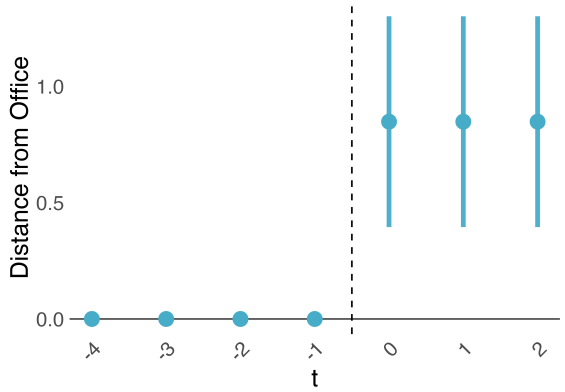
Figure 10 — How did the closings affect municipalities' exposure to the tax authority?



(a) Evolution of population by tax official by group.



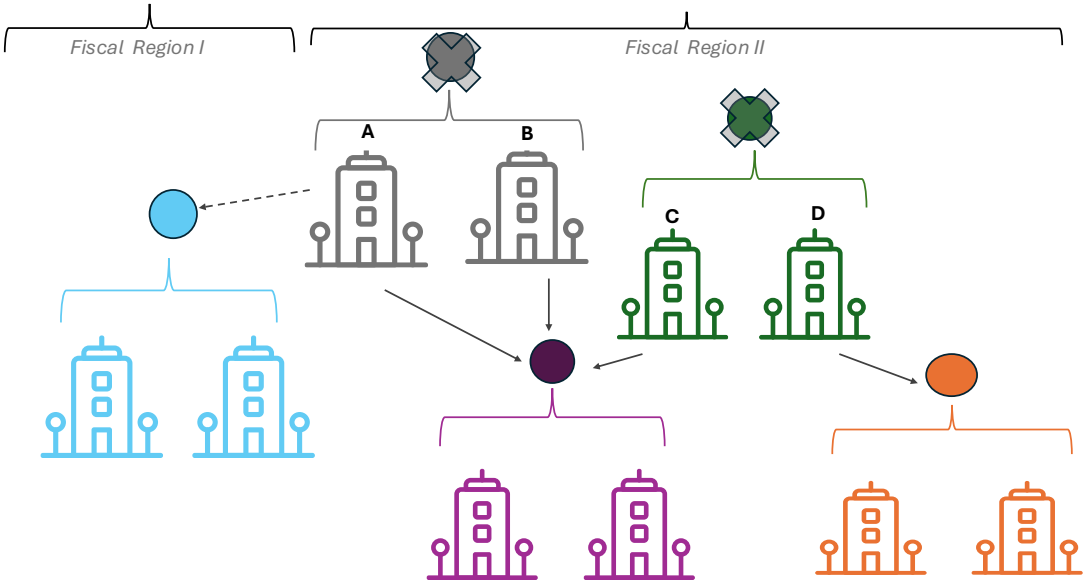
(b) Evolution of area by tax official by group.



(c) Evolution of distance from tax office by group.

Notes. I display dynamic coefficients for *closing*. The outcomes are measure of exposure of a municipality to the tax authority. *Population by tax official:* number of inhabitants (as measured in 2010) in the tax jurisdiction divided by number of tax officials working in the local office. *Area by tax official:* total area (in km2) of the tax jurisdiction divided by number of tax officials working in the local office. *Distance from tax office:* distance (in hours of travel) between a municipality and the jurisdiction tax office. Notice that the first two outcomes vary at the jurisdiction level, the third one at the municipality level. Standard errors are cluster at the baseline tax jurisdiction level.

Figure 11 — Illustration of the IV



Notes. Schematic illustration of the variation underlying the IV approach. The variation comes from differences - at baseline - in distance from other tax office in the same fiscal region. This distance become *relevant* if the jurisdiction tax office closes.

Table 5: First Stage

	Distance office		Closing	
	(1)	(2)	(3)	(4)
Distance other office (pre)	0.951***		-0.002*	0.000
	(0.040)		(0.001)	(0.000)
Distance other office (pre) × Closing Post	0.470***	0.499***		
	(0.078)	(0.093)		
Municipality		✓		
Region-Year FE		✓		
Meso				✓
Dep. Var. Mean	3.6313	3.6313	0.22195	0.22195
Wald (joint nullity)	332.62	28.863	3.7379	0.08753
R^2	0.95233	0.99972	0.00401	0.69184
Clusters	91	91	91	91
Observations	37,751	37,751	5,393	5,393

Notes. Distance office: Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing Post* is an indicator equal to 1 for a municipality whose jurisdiction tax office closed. Notice that fiscal regions border do not change over time and a municipalities would not be assigned to a tax office from another fiscal region. Thus, in the following I will use *Distance other office (pre)* + *Distance other office (pre)* × *Closing Post* as instrument for *Distance office*. Columns 1 and 2 show the relevance of the instrument; column 2 includes the same set of fixed effects as the second stage main specification. Column 3 and 4 is a cross-section in the year before the closings (because the variables are time-unvarying); it shows that *Distance other office (pre)* does not predict whether a jurisdiction tax office closes after the inclusion of mesoregion fixed effect. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. I report at the bottom the Wald statistic from a test on the joint nullity of all the coefficients. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 12 — Visualization of the first stage



(a) Pre - reform



(b) Post- reform

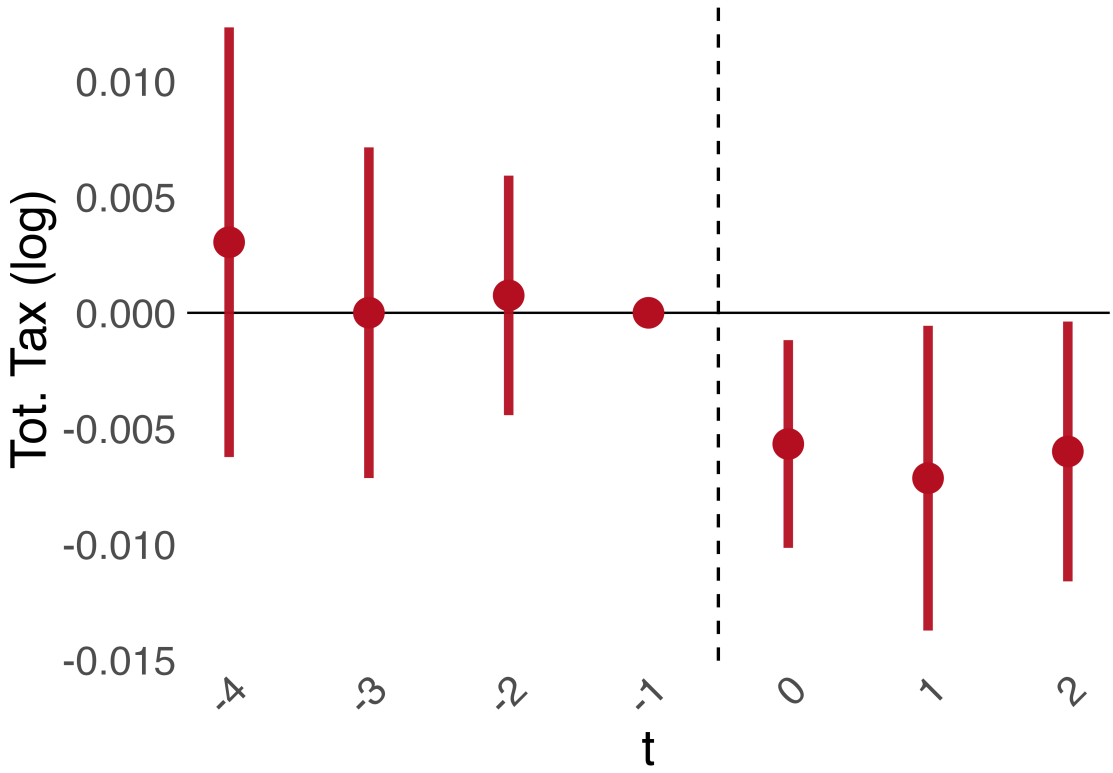
Notes. Scatter plot of *Distance office* (vertical axis) and *Distance other office (pre)* (horizontal axis). *Distance office:* Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing* is an indicator equal to 1 for a municipality whose jurisdiction tax office closes in 2020. While there is a positive correlation between *Distance other office (pre)* and *Distance office* also before the reform (top panel), this relationship becomes steeper after the reform (bottom panel) for municipalities whose jurisdiction tax office closed (green dots).

Table 6: The effect of distance on tax collection

	Tot. Tax (log)		
	Endo	RF	2SLS
Distance office	-0.015***		-0.017***
	(0.003)		(0.006)
Distance other office (pre) × Closing Post		-0.008**	
		(0.004)	
Municipality	✓	✓	✓
Region-Year FE	✓	✓	✓
Controls	All	All	All
Dep. Var. Mean	16.3	16.3	16.3
R^2	0.99	0.99	0.99
Clusters	91	91	91
Observations	37,751	37,751	37,751
Wald (1st stage), Distance office			29.6

Notes. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Distance office:* Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing post:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). Column 1 reports the OLS estimates of the endogenous variable *Distance office*; column 2 reports the reduced form estimates; column 3 reports the 2SLS estimates. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *All controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access); formality share from 2010 census interacted with year fixed effects; nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 13 — The effect of distance on tax collection



Notes. Dynamic version of column 5 of table 6.

Table 7: The effect of distance on tax collection (reduced form): non linear specification

	Tot. Tax (log)			
	(1)	(2)	(3)	(4)
Distance other office (pre) Q1 × Closing Post	-0.034*	-0.034	-0.036	-0.024
	(0.018)	(0.022)	(0.027)	(0.032)
Distance other office (pre) Q2 × Closing Post	-0.051***	-0.044***	-0.032*	-0.041**
	(0.017)	(0.016)	(0.017)	(0.020)
Distance other office (pre) Q3 × Closing Post		-0.042*	-0.047***	-0.041**
		(0.021)	(0.016)	(0.017)
Distance other office (pre) Q4 × Closing Post			-0.058**	-0.044**
			(0.027)	(0.018)
Distance other office (pre) Q5 × Closing Post				-0.064**
				(0.028)
Municipality	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls	All	All	All	All
Dep. Var. Mean	16.3	16.3	16.3	16.3
R^2	0.99	0.99	0.99	0.99
Clusters	91	91	91	91
Observations	37,751	37,751	37,751	37,751

Notes. I report the reduced form estimates only. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Closing post:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). *Distance other office (pre) Q1/Q2/Q3/Q4/Q5:* Quantiles for distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. Quantiles column 1: Q1: 0.13 - 3.30; Q2: 3.30 - 285.44. Quantiles column 2: Q1: 0.13 - 2.50; Q2: 2.50 - 4.31; Q3: 4.31 -285.44. Quantiles column 3: Q1: 0.13 - 2.09; Q2: 2.09 - 3.30; Q3: 3.30 - 5.03; Q4: 5.03 - 285.44 . Quantiles column 4: Q1: 0.13 - 1.85; Q2: 1.85 - 2.82; Q3: 2.82 - 3.85; Q4: 3.85 - 5.80; Q5: 5.80 -285.44. *Fixed effects:* municipality and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *All controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access); formality share from 2010 census interacted with year fixed effects; nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 8: The effect of distance on tax collection: additional specifications

	Tot. Tax (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Distance other office (pre) × Closing Post	-0.024***		-0.020**		-0.019**	
	(0.008)		(0.008)		(0.008)	
Distance office		-0.059***		-0.052**		-0.048**
		(0.022)		(0.023)		(0.022)
Municipality	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓
Post Tax Jur - Year			✓	✓		
Tax Jur Pair - Year					✓	✓
Controls	All	All	All	All	All	All
Dep. Var. Mean	16.1	16.1	16.1	16.1	16.1	16.1
Wald (1st stage), Distance office		38.6		29.5		31.1
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Clusters	1,197	1,197	1,197	1,197	1,197	1,197
Observations	8,379	8,379	8,379	8,379	8,379	8,379

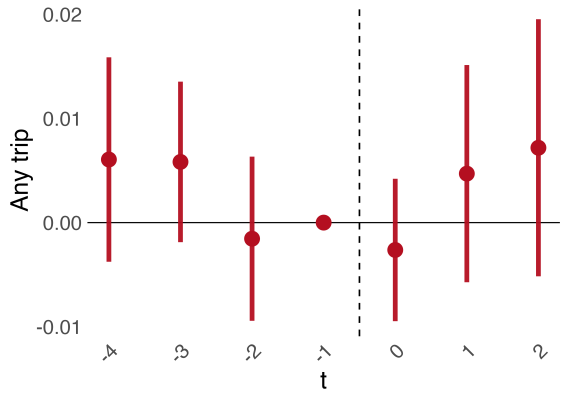
Notes. The sample contains treated municipalities only. Odd columns report reduced form estimates, even columns report 2SLS estimates. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Distance office:* Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing Post:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). *Fixed effects:* municipality and mesoregion-by-year fixed effects. Columns 3 and 4 include also indicators for jurisdictions (after the reform) interacted with year fixed effects. Columns 5 and 6 include also jurisdictions (before the reform) and jurisdictions (after the reform) pairs interacted with year fixed effects. *All controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access); formality share from 2010 census interacted with year fixed effects; nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 9: The effect of distance on tax officials trips to municipalities

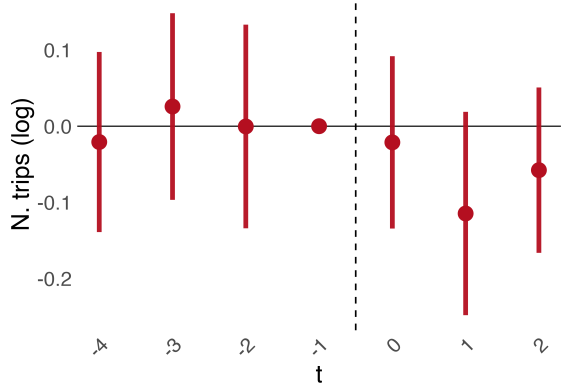
	Any trip			N. trips (log)			N. trips	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	Poisson	Poisson
Distance office	-0.003		0.001	-0.150***		-0.165**	-0.276***	
	(0.004)		(0.007)	(0.048)		(0.076)	(0.073)	
Distance other office (pre) × Closing Post		0.001			-0.074**			-0.136**
		(0.003)			(0.036)			(0.056)
Municipality	✓	✓	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	All	All	All	All	All	All	All	All
Dep. Var. Mean	0.13	0.13	0.13	1.3	1.3	1.3	2.8	2.8
Wald (1st stage), Distance office			29.6			101.4		
R^2	0.56	0.56	0.56	0.84	0.84	0.84		
Clusters	91	91	91	91	91	91	91	91
Observations	37,751	37,751	37,751	4,944	4,944	4,944	37,751	37,751

Notes. *Any Trip:* Indicator equal to 1 if any tax official from a local office visited a municipality in a year. *N. trips:* Number of times a tax official from a local office visited a municipality in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). Columns 1 and 4 report the report the OLS estimates of the potentially endogenous variable *Distance office*. Columns 2 and 5 report the reduced form estimates. Columns 3 and 6 report the 2SLS estimates. Column 7 reports a Poisson estimation of the endogenous variable and column 8 reports the reduced form. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. *All controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access); formality share from 2010 census interacted with year fixed effects; nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

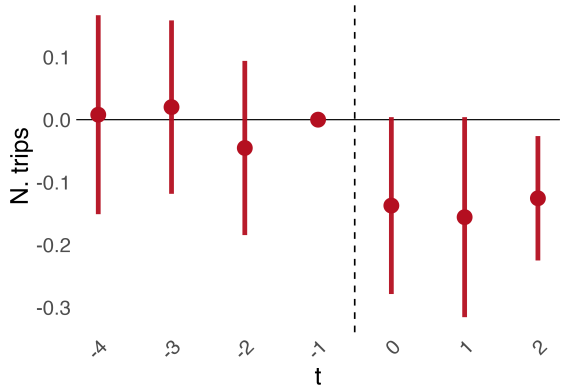
Figure 14 — The effect of distance on tax officials trips to municipalities



Notes. Dynamic version of column 2 of table 9.



Notes. Dynamic version of column 5 of table 9.



Notes. Dynamic version of column 8 of table 9.

Table 10: The effect of distance on characteristics of tax officials trips to municipalities

	Days trips (total)			Cost trips (total)		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance office	-0.587		0.747	-65.741		164.761**
	(0.536)		(0.470)	(110.630)		(73.331)
Distance other office (pre) × Closing Post		0.373*			82.135**	
		(0.212)			(33.365)	
Municipality	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓
Dep. Var. Mean	5.8	5.8	5.8	1,009.5	1,009.5	1,009.5
Wald (1st stage), Distance office			28.9			28.9
R^2	0.78	0.78	0.78	0.76	0.76	0.76
Clusters	91	91	91	91	91	91
Observations	37,751	37,751	37,751	37,751	37,751	37,751

Notes. *Days trips (total):* total duration in number of days of trips performed by tax officials from a local office to a municipality. *Cost trips (total):* total cost in 2018 Brazilian Reais of trips performed by tax officials from a local office to a municipality. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). Columns 1 and 4 report the report the OLS estimates of the potentially endogenous variable *Distance office*. Columns 2 and 5 report the reduced form estimates. Columns 3 and 6 report the 2SLS estimates. *Fixed effects:* municipality and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 11: The effect of distance on characteristics of tax officials trips to municipalities

	Days trips (avg)			Cost trips (avg)		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance office	0.008		0.029	2.075		7.205*
	(0.010)		(0.021)	(1.806)		(3.887)
Distance other office (pre) × Closing Post		0.014			3.592**	
		(0.009)			(1.600)	
Municipality	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓
Dep. Var. Mean	0.40	0.40	0.40	61.4	61.4	61.4
Wald (1st stage), Distance office			28.9			28.9
R^2	0.51	0.51	0.51	0.53	0.53	0.53
Clusters	91	91	91	91	91	91
Observations	37,751	37,751	37,751	37,751	37,751	37,751

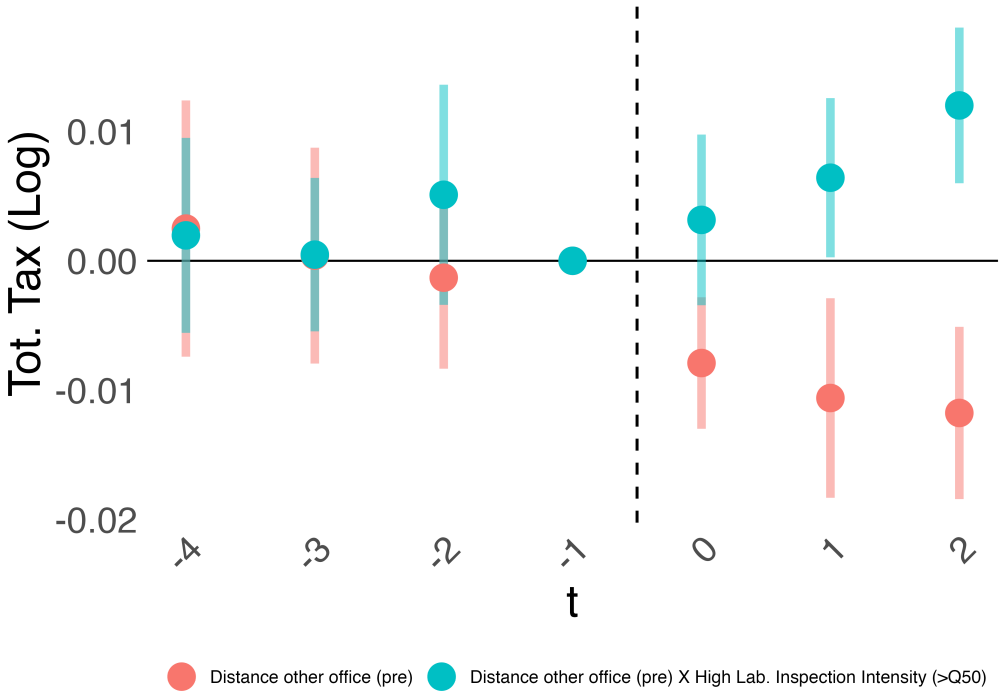
Notes. *Days trips (avg):* average duration in number of days of trips performed by tax officials from a local office to a municipality. *Cost trips (avg):* average cost in 2018 Brazilian Reais of trips performed by tax officials from a local office to a municipality. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). Columns 1 and 4 report the report the OLS estimates of the potentially endogenous variable *Distance office*. Columns 2 and 5 report the reduced form estimates. Columns 3 and 6 report the 2SLS estimates. *Fixed effects:* municipality and mesoregion-by-year fixed effects. Mesoregions do not have any administrative substance but are defined by the statistical office as regions with similar socio-economic characteristics. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 12: Effect stronger where local information is more valuable

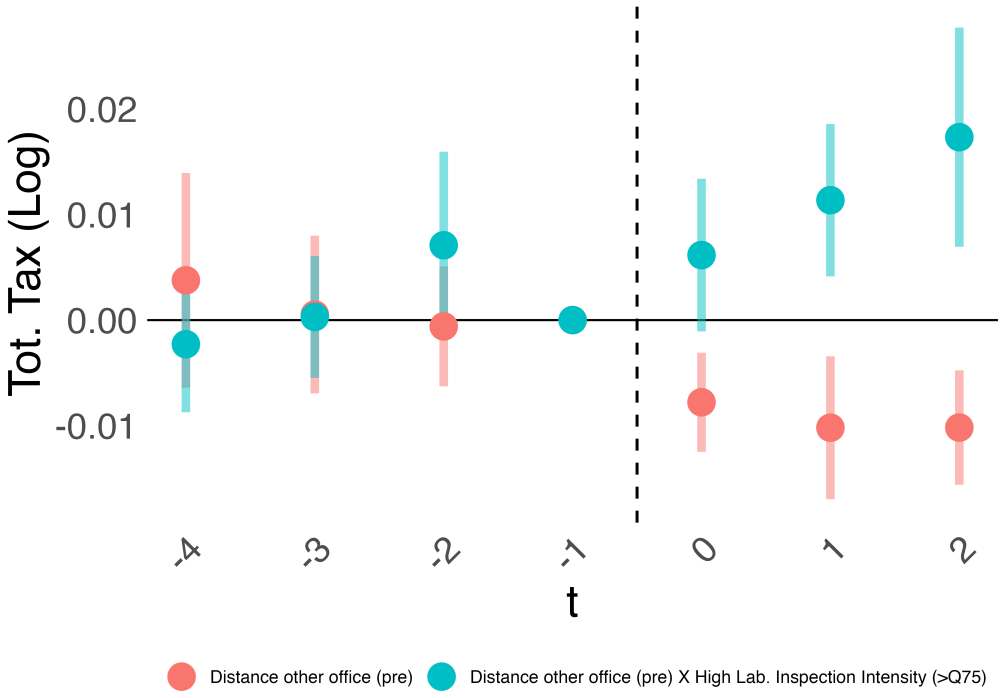
	Tot. Tax (log)	
	(1)	(2)
Closing Post	-0.048***	-0.047***
	(0.016)	(0.016)
Closing Post × High Lab. Inspection	0.020*	0.029**
	(0.011)	(0.012)
Municipality	✓	✓
Region-Year FE	✓	✓
Controls	All	All
Dep. Var. Mean	16.3	16.3
R^2	0.99	0.99
Clusters	91	91
Observations	37,751	37,751

Notes. Tot. Tax (log): total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Closing Post:* indicator equal to 1 from 2020 on for municipalities whose jurisdiction tax office closes in 2020. *High Lab. Inspection Intensity*: Indicator equal to 1 if a municipality is above the 50th (column 1) or the 75th (column 2) percentile in the distribution of the number of labor inspections carried out in a mesoregion in the years 2016-2019. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. Standard errors are clustered at the level of the tax jurisdiction in the year before the closings. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 15 — Effect stronger where local information is more valuable

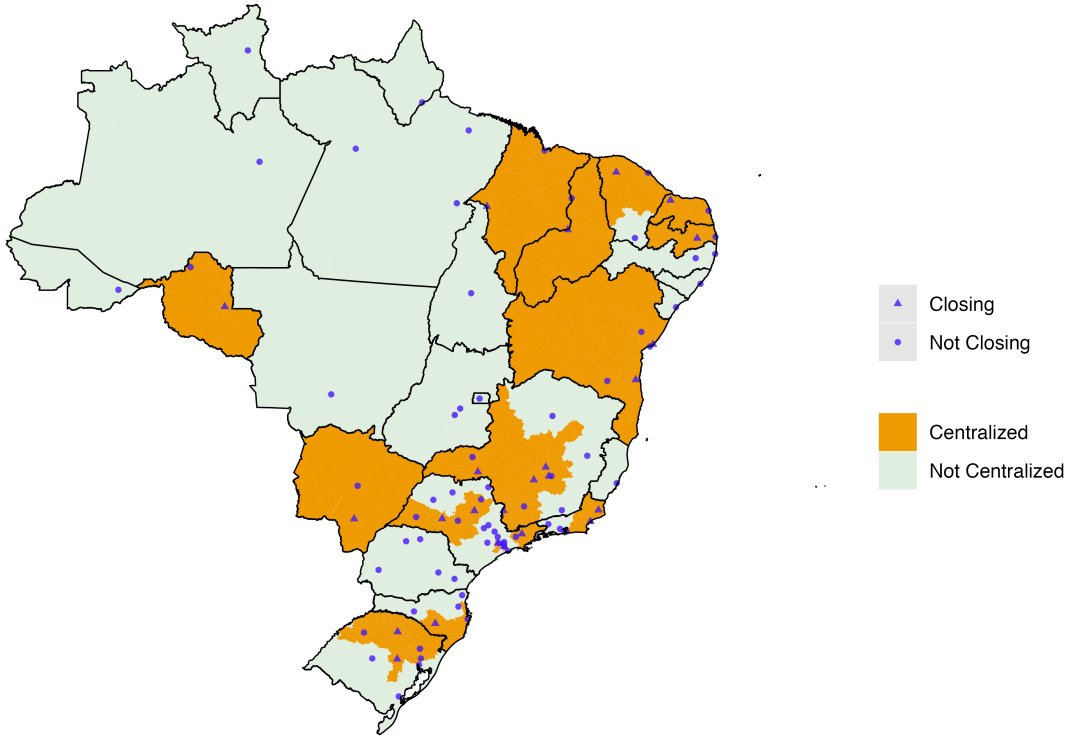


Notes. Dynamic version of column 1 of table 12.



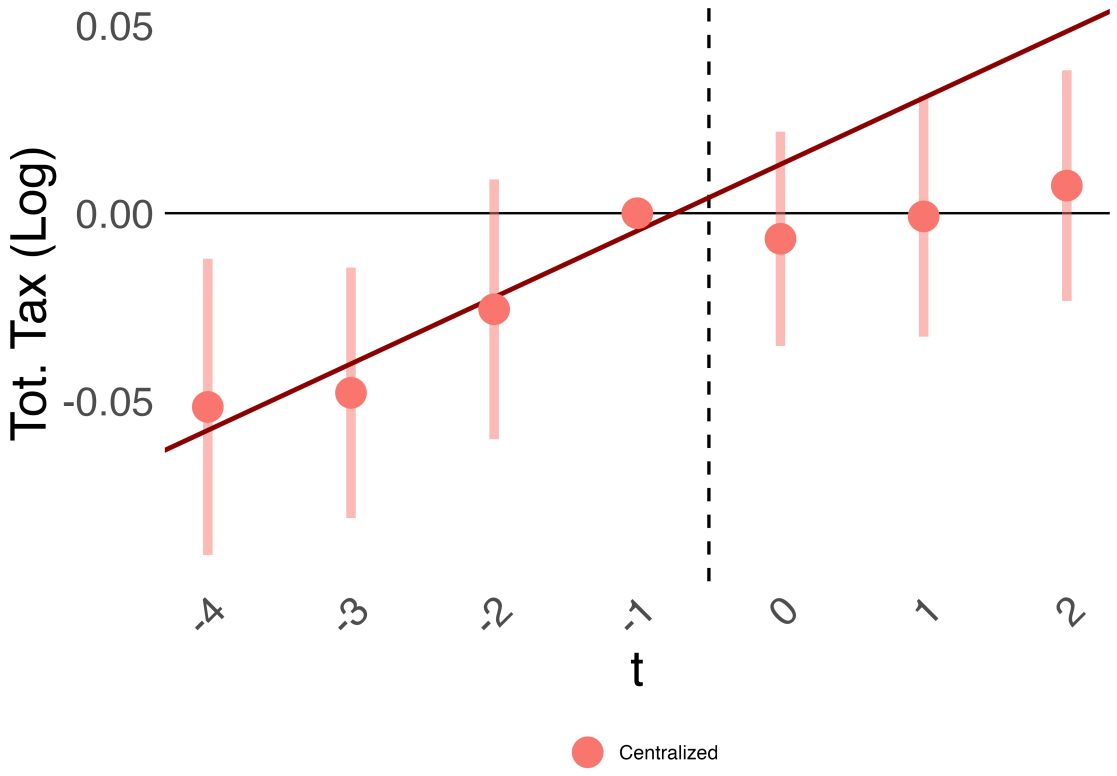
Notes. Dynamic version of column 2 of table 12.

Figure 16 — Map of jurisdictions which underwent a centralization



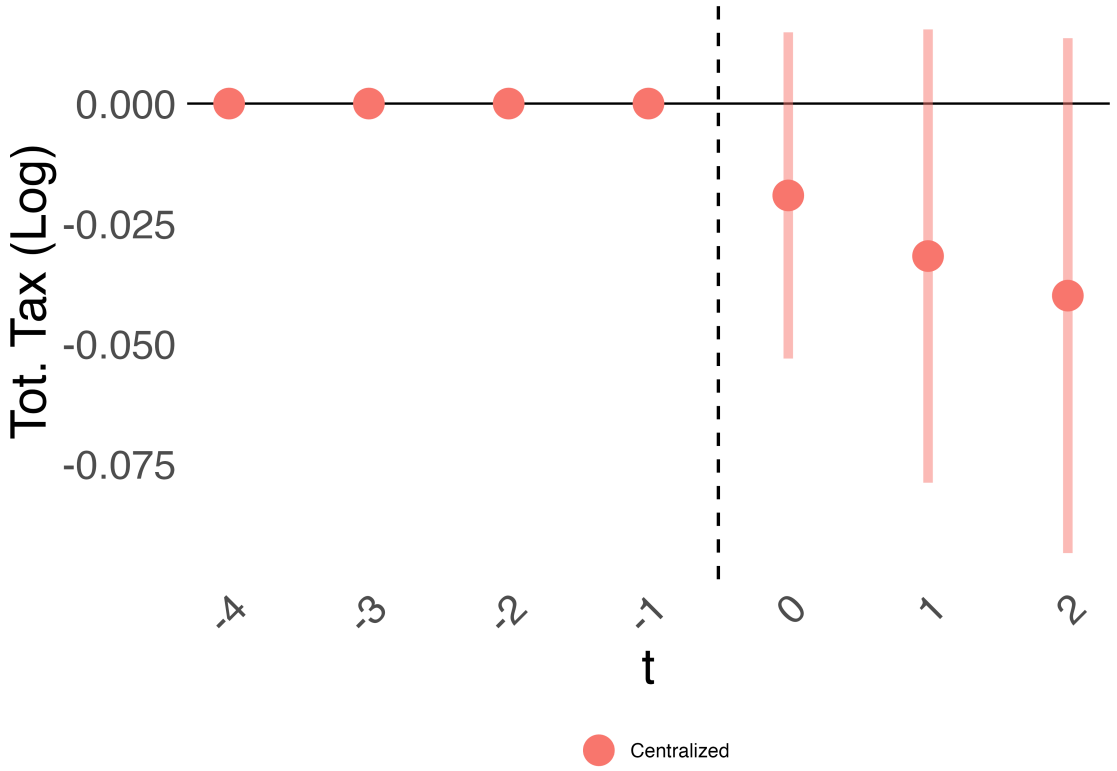
Notes. The map visualizes which jurisdictions underwent a centralization and which did not.

Figure 17 — Centralized municipalities are on differential trends



Notes. Tot. Tax (log): total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Centralized:* Indicator which takes value 1 for municipalities that after the closings are part of a jurisdiction which underwent a centralization in 2020. The dynamic coefficients highlight that *centralized* municipalities are on differential trends.

Figure 18 — The net effect of centralization on tax collection



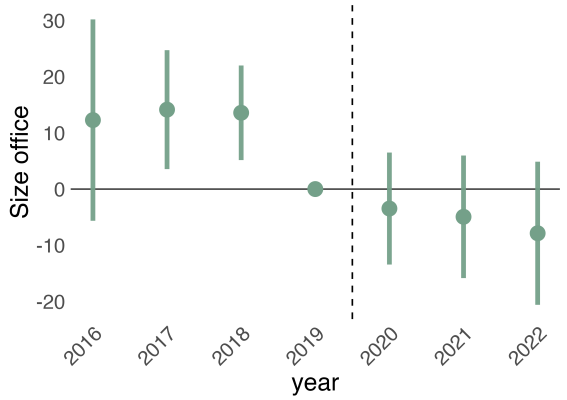
Notes. Dynamic version of column 2 of table 13.

Table 13: The net effect of centralization on tax collection

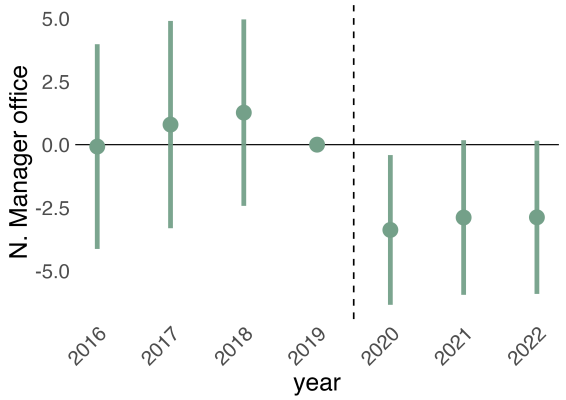
	Tot. Tax (log)	
	(1)	(2)
Centralized × Period = 0	-0.029*	-0.020
	(0.017)	(0.017)
Centralized × Period = 1	-0.029	-0.032
	(0.022)	(0.023)
Centralized × Period = 2	-0.026	-0.041
	(0.027)	(0.027)
Centralized × Year Trend	0.013*	0.018**
	(0.007)	(0.007)
Municipality	✓	✓
Region-Year FE	✓	✓
Controls		All
Dep. Var. Mean	16.4	16.4
R^2	0.99	0.99
Clusters	70	70
Observations	38,024	37,968

Notes. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Centralized post:* Indicator which takes value 1 for jurisdictions which underwent a centralization in 2020. In column 1 the units of analysis are the jurisdictions in 2020 (after the reform). The units of analysis are the municipalities. Because figure 18 suggests that *centralized* municipalities are on differential trends, I include a linear trend specific for *centralized* municipalities. In order to fit this trend only on the pre-reform period, I include both the linear trend and the interaction between *centralized* and each post-period year. ISpecification in column 4 includes all the controls: area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log) - all from 2010 census and interacted with year fixed effects; distance from any tax office in any fiscal region but in a different tax jurisdiction - in the year before the tax offices closings and interacted with year fixed effects (captures flexibly the possibility that distance from tax offices is correlated with market access); formality share from 2010 census interacted with year fixed effects; nightlights (log) and value of agricultural production (log), time-varying. Standard errors are clustered at the level of the tax jurisdiction after the reform. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

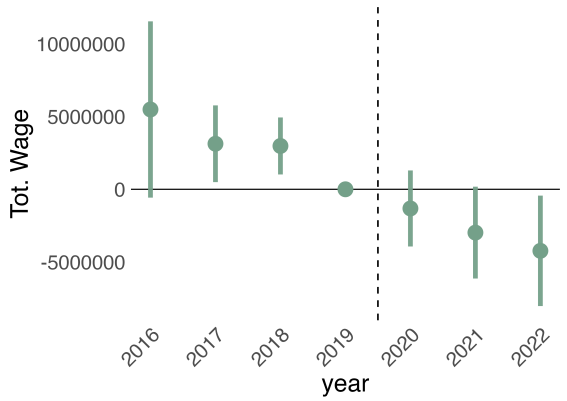
Figure 19 — Reform, number tax agents and managers, and wage bill



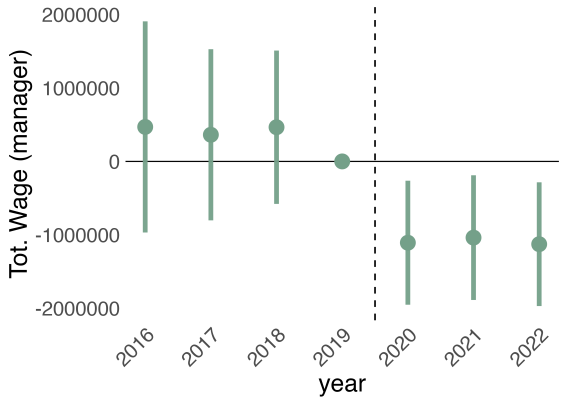
(a) Number tax agents



(b) Number managers



(c) Total wage bill



(d) Manager wage bill

Notes. I display dynamic coefficients of being in a centralized jurisdiction on the number of tax agents and managers, and on the wage bill.