Organizing Fiscal Capacity*

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Abstract

This paper evaluates a reform of the Brazilian tax authority that closed one-fourth of existing local offices, resulting in a more centralized structure with fewer but larger offices. Leveraging regional variation induced by the reform, I employ a matched difference-in-differences design to document how the reform impacts tax revenues. Tax revenues decline in areas previously served by a closed office but increase in areas served by an expanded office; the net effect is an increase in tax revenues. These effects arise because the reform affects both the ability to collect taxes in certain areas and how the tax authority chooses to allocate staff effort. First, increased distance between tax offices and the areas they monitor reduces tax revenues, particularly where local information on taxable activity is more valuable. Second, areas served by an expanded office see larger revenue increases when they have higher revenue potential compared to other areas newly assigned to the same office, supporting the hypothesis that centralization allows for improved targeting of enforcement resources. These findings underscore the importance of how bureaucracies are organized across the territory for their effectiveness.

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1 — Introduction

Fiscal capacity is key to economic and political development (Besley and Persson 2011; Weigel 2020). However, governments in low- and middle-income countries often struggle to increase tax revenues (Besley and Persson 2013). To overcome this challenge, there is growing interest in understanding which administrative reforms can enhance the effectiveness of tax authorities (Okunogbe and Tourek 2024; Jensen and Weigel 2024). A fundamental decision faced by tax authorities is how to organize their presence across the territory. One option is to staff many small local offices. This *decentralized* structure brings agents closer to the areas they need to monitor, potentially improving access to local information (Scott 1998). Accessing this information is especially important for tax enforcement in contexts with limited third-party reporting (Kleven et al. 2011; Pomeranz 2015). Alternatively, tax authorities can decide to staff fewer but larger offices. This more *centralized* structure may lead to economies of scale and to a better allocation of limited enforcement resources (Bergeron et al. 2022; Kapon, Del Carpio, and Chassang 2024). While many countries have recently shifted toward a more centralized structure (OECD 2015), there is limited empirical evidence on how this impacts tax revenues.

This paper investigates how the organization of a tax authority's territorial presence affects fiscal capacity. I examine a major reform of the Brazilian tax authority that closed some local offices while expanding others, leading to a more centralized structure in certain regions. First, I document how the reform impacts tax revenues. Revenues increase in areas served by expanded offices but decline in areas previously served by closed offices. On net, centralized regions see an increase in tax revenues. Second, I find evidence that the reform makes it harder to collect revenues in certain areas. Third, I show that the reform alters how the tax authority allocates enforcement resources and enhances its internal efficiency.

The Brazilian tax authority represents a suitable setting to study the effects of different territorial organizational structure. It operates over a huge territory, and it is responsible for the collection of most of the government tax revenues, including income tax, social security contributions, and taxes on firms. The entire territory is partitioned in tax jurisdictions. Each tax jurisdiction spans several municipalities - the lowest tier administrative unit - and is overseen by a local office. Among other things, local offices are tasked with detecting tax evasion. They do so by

combining available data with additional information, which can be collected either remotely or with physical inspections. This setting enables me to overcome two main empirical challenges. The first challenge is the need for granular data on the tax authority presence and effectiveness. The second challenge is establishing causal identification. For example, cross-country differences in how tax authorities are organized may be correlated with other differences in economic structure, institutional features, and macroeconomic conditions which also affect tax revenues.

To measure the tax authority presence and effectiveness, I combine various data sources. I reconstruct changes of tax offices' locations and tax jurisdictions boundaries from internal regulations and obtain data on the amount of federal tax revenues collected in each municipality. Moreover, I assemble a novel dataset on tax agents' career, with information on their deployment across offices and on their managerial positions. I complement these data with information on tax agents' trips for work. The resulting dataset allows me to leverage within-country variation in how centralized the tax authority is, while keeping other institutional features constant and controlling for macroeconomic shocks.

To causally identify the effect of moving to a more centralized structure, I study a reform that altered the number and size of tax jurisdictions in 2020. The reform was triggered by mandated budget cuts, which led to a hiring freeze, a decline in personnel size, and a reduction in the number of maintainable managerial positions. One-fourth of the existing local offices were closed. One-fourth of the existing local offices were instead expanded. Expanded offices absorbed most of the tax agents from the closed offices. Moreover, after the reform these offices oversee a larger jurisdiction - including the municipalities they were already serving (*receiving* municipalities) and the ones previously served by closed offices (*closing* municipalities). Because of the reform, some regions have a more centralized structure. In other regions, the organization is unchanged by the reform and there is the same number of local offices serving the same municipalities (*unaffected* municipalities).

My empirical strategy compares the evolution of tax revenues between municipalities differentially affected by the reform. Because economic structure in Brazil is highly heterogeneous and the choice of which offices to close was not done at random, the three groups of municipalities (*closing*, *receiving*, *unaffected*) display differential trends in economic activity and tax revenues before the reform. To overcome this issue, I employ a matched difference-in-differences design. I

match each unit in the treated group - either the *closing* or the *receiving* municipalities - to a unit from the *unaffected* municipalities based on the evolution of local GDP in the years before the reform. This allows me to compare the evolution of tax revenues among municipalities with a similar trajectory of economic activity. The motivation for this approach is that municipalities on similar trends in economic activity should also be on similar trends in tax revenues.

I start by documenting how the reform impacted tax revenues. First, I compare the evolution of tax revenues between municipalities whose jurisdiction tax office closed - *closing* municipalities - and *unaffected* municipalities that were on similar economic trends before the reform. *Closing* municipalities experience a 3.6% reduction in federal tax collection. Second, I compare the evolution of tax revenues between municipalities whose jurisdiction tax office expanded - *receiving* municipalities - and *unaffected* municipalities which were on similar economic trends before the reform. I find that *receiving* municipalities experience a 3.4% increase in federal tax collection. Several pieces of evidence support the validity of the empirical strategy. In both comparisons, there is no evidence of differential trends in tax revenues before the reform. Similarly, there is no evidence of differential trends in local economic activity neither before the reform - this just confirms that the matching procedure is successful - nor after the reform - this offers reassurance that the changes in tax revenues are not due to changes in the taxable activity. I also show that employing a trajectory balancing approach (Hazlett and Xu 2018) delivers similar average effects for both comparisons. Moreover, local taxes (as opposed to federal taxes) collected by the municipalities themselves do not follow a similar pattern- a useful placebo as one would not expect these taxes to be influenced by federal tax enforcement.

Having shown that the reform impacts tax revenues differentially in *closing* and *receiving* municipalities, the second part of the paper investigates what explains these differences. One reason could be that the reform makes it harder to collect taxes in certain areas. This would explain the revenue drop in *closing* municipalities but not the increase in *receiving* ones. Another possibility is that the reform changes how the tax authority chooses to allocate enforcement resources. This could account for both effects. I present evidence supporting both explanations. First, closing offices limits access to local information due to the increased distance between tax offices and the areas they serve. Second, offices overseeing a larger jurisdiction can better direct enforcement resources to high-revenue areas. I also find suggestive evidence that centralization

can help the tax authority benefit more from the existing pool of high-quality managers.

To investigate the role of distance, I start by documenting that the reform increases the travel time between a *closing* municipality and its new jurisdiction's tax office by an average of 1.5 hours (66% of the pre-reform mean). I then use an instrumental variable approach to isolate the effect of distance on tax revenues. Specifically, the instrument leverages the baseline variation in distance between a municipality and the closest *other* office. This distance does not affect tax enforcement before the reform, as what matters is the distance to the actual jurisdiction's tax office. However, for *closing* municipalities, this distance becomes the lower bound on the distance to the actual jurisdiction's tax office after the reform. This strategy addresses concerns about the post-reform distance being endogenous; for instance, one such concern would be that municipalities with a declining tax potential are assigned to offices farther away. Using this approach, I find that municipalities that end up one hour farther from a tax office experience a 1% larger reduction in tax revenues.

While tax agents' ability to detect evasion using technology and hard data should be unaffected by their location, their ability to gather local information may be. Therefore, one might expect stronger effects of distance on tax revenues in areas where local information is more valuable. Local information is particularly relevant in settings with less third-party reporting (Jensen 2022). To test this hypothesis, I leverage regional variation in the intensity of third-party reporting enforcement from the Ministry of Labor. Labor inspections ensure that firms register their employees, creating an information trail on employees' income and firms' activities. This helps tax agents detecting evasion even without access to local information. I use microdata on labor inspections from the years before the reform to classify municipalities based on the intensity of third-party reporting during that period. Heterogeneity with respect to this measure shows that the negative effects of distance on tax revenues are larger where third-party reporting enforcement is weaker and local information is thus more valuable. This suggests that distance acts as a friction preventing tax agents from gathering local information. Consistently with this mechanism, I leverage data on the trips for work carried out by tax agents to show that municipalities farther away receive fewer visits.

I then turn to examine whether the reform affects how the tax authority allocates enforcement resources. After the reform, centralized offices have more tax agents. Although they cover a

larger area, they may find more efficient to focus staff effort on municipalities with higher revenue potential. To test this, I rank municipalities in post-reform jurisdictions by a proxy of their revenue potential: the average tax collected in the years before the reform. I identify jurisdictions where *receiving* municipalities have higher revenue potential than the *closing* municipalities merged into the same office. In these jurisdictions, there is more scope for reallocating resources to *receiving* municipalities. Indeed, I find that the increase in tax revenue for *receiving* municipalities is stronger in these jurisdictions. This suggests that centralizing tax agents and decision-making over a larger jurisdiction can improve the allocation of limited enforcement resources (Kapon, Del Carpio, and Chassang 2024; Bergeron et al. 2022).

I further explore the possibility that a centralized structure leads to economies of scale that increase the efficiency of the tax authority. By reducing the number of office managers, a centralized structure can be an opportunity to retain only the most talented managers and give them a larger span of control. To explore this, I test whether higher manager quality leads to larger tax collection gains from the reform (Fenizia 2022). Using detailed data on tax agents' careers, I identify high-quality managers overseeing local offices during the reform period. Specifically, I build on the approach in Minni 2024 and define high-quality managers as those promoted to manager at a relatively younger age (below the 25th percentile of the age distribution). In line with the economies of scale hypothesis, I find that tax revenue increases are greater in *receiving* municipalities with high-quality managers in charge.

In the final part of the paper, I quantify the net effects of the reform. To capture both the impacts on *closing* and *receiving* municipalities, I conduct the analysis at the post-reform jurisdiction level. I compare aggregate tax revenues in jurisdictions that centralized with those that did not. The estimates are noisy but suggest that, on average, centralized jurisdictions saw a 2.7% increase in tax revenues. Additionally, centralized jurisdictions employed fewer managers, resulting in savings on managerial wages. Overall, the reform produced positive net effects for the government budget. As a comparison, the additional revenue generated by a centralized jurisdiction are equivalent to funding the main government social assistance program, *Bolsa Família*, for approximately 50,000 households.

Taken together, these findings provide some of the first evidence on how the organization of a bureaucracy presence across the national territory impacts its effectiveness. While in this context

the net effects are positive, this may not generalize to other contexts. However, by uncovering some forces underlying the trade-off between a centralized or decentralized structure, this paper can offer some guidance to policymakers interested in implementing a similar reform in other contexts. Specifically, they should consider the severity of geographical frictions, the importance of local information, the distribution of revenue potential, and the pool of managers.

Related Literature This paper relates and contributes to several strands of the literature. First, it contributes to the literature on taxation in developing countries. Recent papers in public finance have focused on the importance of third-party reporting for tax enforcement (Gordon and Li 2009; Kleven et al. 2011; Pomeranz 2015; Slemrod 2019; Naritomi 2019; Pomeranz and Vila-Belda 2019). Because third-party coverage evolves slowly over the process of economic development and structural transformation (Kleven, Kreiner, and Saez 2016; Jensen 2022), there is growing interest in how tax administration reforms can enable governments to raise more revenues (Jensen and Weigel 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), the allocation of tax agents and enforcement resources (Basri et al. 2021; Bergeron et al. 2022; Kapon, Del Carpio, and Chassang 2024), and the potentialities of technology (Fan et al. 2018; Okunogbe and Santoro 2023; Dzansi et al. 2022; Knebelmann, Pouliquen, and Sarr 2023). This paper shows that different ways of organizing a tax authority presence across the national territory – and specifically the choice between a decentralized and a centralized structure – can have relevant effects on fiscal capacity. Moreover, I provide suggestive evidence that even in contexts with limited third-party reporting, tax authorities can leverage local information (Balán et al. 2022) for successful tax enforcement[1]. However, tax agents ability in gathering local information is constrained by how far they are from the areas they need to monitor[2].

Second, this paper speaks to the literature on the effectiveness of public sector bureaucracies

1. In doing so, I speak to the literature on the value of local information in governance in various contexts outside of tax administration (Rogger and Somani 2023), like environmental regulation, agricultural extensions and anti-poverty programs (Duflo et al. 2018; Dal Bó et al. 2021; Basurto, Dupas, and Robinson 2020). Relatedly, a large literature has studied the incentives, constraints, and consequences of creating and updating administrative cadasters of taxpayers (Casaburi and Troiano 2016; Gadenne 2017; Christensen and Garfias 2021; Bowles 2024; Knebelmann, Pouliquen, and Sarr 2023; Martínez 2023; Ferraz, Foremny, and Santini 2024).

2. These findings relate to a broad literature across the social sciences discussing how physical distance limits state capacity (Mann 1984; Herbst 2014; Michalopoulos and Papaioannou 2014; Müller-Crepon 2023).

(Finan, Olken, and Pande 2017; Besley et al. 2022). Previous literature has investigated the role of selection (Dal Bó, Finan, and Rossi 2013; Deserranno 2019; Ashraf et al. 2020; Moreira and Pérez 2024), incentives (Khan, Khwaja, and Olken 2016, 2019; Bertrand et al. 2020; Deserranno and León-Ciliotta 2021; Deserranno et al. 2022), and individual effectiveness (Fenizia 2022; Best, Hjort, and Szakonyi 2023; Dahis, Schiavon, and Scot 2023; Bergeron et al. 2022). This paper leverages microdata on the entire Brazilian tax authority to provide some of the first evidence on how a system-wide reform affects a bureaucracy effectiveness. Moreover, I provide evidence on the importance of public sector managers within tax authorities (Rasul and Rogger 2018; Fenizia 2022; Limodio 2021; Otero and Munoz 2022).

Third, I contribute to the literature on state capacity (Besley and Persson 2011), by showing the role of organizational choices in affecting fiscal capacity in modern times (Cantoni, Mohr, and Weigand 2024). One implication of my results is that differences in the observability of economic activity may determine what is the best way of organizing a tax authority, in line with the literature studying how observability affects the creation and evolution of states (Mayshar, Moav, and Neeman 2017; Sánchez De La Sierra 2020; Garfias and Sellars 2021; Mayshar, Moav, and Pascali 2022; Mastrorocco and Teso 2023). This work is particularly related to papers that investigate the impact of administrative reforms expanding the reach of the state by introducing new administrative layers or altering the size of administrative units, in both historical contexts and contemporary developing countries (Chambru, Henry, and Marx 2021; Chiovelli et al. 2024; Narasimhan and Weaver 2023; Dahis and Szerman 2024). Relative to these papers, I show that administrative remoteness continues to affect the performance of bureaucracies in modern times. Moreover, the mechanisms driving the effects of units' size can differ between political units and bureaucratic jurisdictions. For example, while recent studies on polity size and development emphasize the role of increased political accountability, this channel is absent in bureaucracies, as bureaucrats are not elected. Instead, my findings suggest the benefits of smaller jurisdictions arise because they allow a better access to local information. At the same time, I find evidence that larger jurisdictions can improve the allocation of resources.

Outline The rest of the paper is organized as follows: Section 2 describes the context and the tax authority reform, section 3 introduces the data. Section 4 describes the empirical strategy.

Section 5 presents evidence on the effects of the reform on tax revenues, Section 6 investigates the mechanisms behinds these effects. Finally, Section 7 discusses the net effects of the reform, while Section 8 concludes.

2 — Context

This section provides institutional background on Brazil's economic, administrative, and fiscal structure. It also explains how the country's tax authority operates and it describes 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 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[3] and 5,570 municipalities.[4] 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.

3. 26 states and one federal district, which includes the capital, Brasilia

4. 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.

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.

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 primary roles within RFB 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 Figure 1). First, the country is divided into 10 fiscal regions[5]. Each region is managed by a regional headquarters. Second, each fiscal region is divided into tax jurisdictions covering several municipalities, managed by local offices[6].

Tax enforcement responsibilities are shared between central units in Brasília, regional headquarters, and local offices[7] (Receita Federal do Brasil 2020). 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, in collaboration with regional headquarters. Tax enforcement happens in two steps: initial selection of potential evasion cases and subsequent auditing. The selection is done by teams which operate across the entire fiscal regions. These teams are specialized by tax type, rather than by geographic area. Auditing is conducted by tax agents deployed in local offices, using hard data and external visits to gather additional

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

6. Within each jurisdiction, there are also lower-tier offices (*Alfândegas, Inspetorias, Agências, Postos de Atendimento*). There is a clear hierarchy: regional headquarters (*Superintendências Regional*) oversee local offices (*Delegacias*) and lower-tier offices in their fiscal region, while local offices, along with headquarters, oversee the lower-tier offices within their jurisdiction.

7. Lower-tier offices offer taxpayer services and are tasked with customs control and detecting illicit trafficking. These responsibilities are shared with central units in Brasília, regional headquarters, local offices. It is 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 in my analysis.

information.

2.3. The reform

The 2020 reform reshaped the regional component of the tax authority by closing 24 local offices[8] (see figure 3c). Discussions about cost rationalization and organizational restructuring had been ongoing since at least 2015. However, mandated budget cuts which reduced the number of maintainable managerial positions and the reduction in personnel size (figures 3a and 3b) and a shrinking of the average local offices size (see figure 3d) likely played significant roles in triggering the 2020 reform.

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 reform - the *receiving* municipalities. Third, the municipalities whose office is not closed and whose jurisdiction will not incorporate the *closing* municipalities after the reform - the *unaffected* municipalities. Figure 5 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 5b). This reallocation of tax officials increase the staff size of the offices serving *receiving* municipalities (figure 5c). The map in figure 5d depicts which municipalities belong to each group. It shows that there is considerable regional variation in how the reform pans out. Overall, the reform leads to fewer, larger offices serving wider jurisdictions.

3 — Data

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

8. A smaller reform happened in 2017, closing three local offices which were serving 55 municipalities. 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. In my analysis, I exclude municipalities which switch jurisdictions even if their office is not closed.

Geographical units I use the 5,565 municipalities present in 2010 as geographic units[9]. 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. 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 the tax authority I consult the tax authority (*Federal Revenue of Brazil*) 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. I obtain the locations of the local tax offices (*delegacias da receita federal do brasil, delegacias especiais da receita federal do brasil*). 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. Notice that, outside 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[10]. As mentioned above, these lower-tier offices are not tasked with tax enforcement and thus are outside the focus of this paper. 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.

Tax collection I use data on federal tax collection at the municipality-year level for the period 2013-2022 from Receita Federal do Brasil 2023. For all municipalities, I have information on the total amount collected. 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

9. The five municipalities created in 2013 are assigned to the 2010 borders based on their centroid. All GIS data are obtained from Pereira and Goncalves 2024.

10. Notice that I drop from my main sample analysis the municipalities which change jurisdictions for reasons other than a closing of the office.

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.

Tax authority personnel data I use data on the payroll of federal civil servants from the Transparency Portal of the Brazilian government (Brazilian Federal government 2023). I access data for each January from 2013 to 2022. I retain all the civil servants whose reported organizational unit matches one of the units listed in the tax authority organizational charts. In this way, I obtain a dataset of all tax authority employees, consisting of 29,445 tax agents. As each tax agent has a unique identifier, I can track them over time and across offices. Additionally, I code information on the wage, managerial position, and years of experience in the tax authority.

Tax agents trips I use data on trips for work undertaken by federal civil servants from 2016 to 2022. These data come from the Transparency Portal of the government (Brazilian Federal government 2023). I merge these data with the tax authority personnel data using the name and partially anonymized social security number of the bureaucrat who did the trip. I use the date to assign each trip to a specific year[11]. 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 tax agent is deployed 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[12], 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,

11. 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

12. 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.

I use VIIRS Nighttime Lights data from the Earth Observation Group (EOG 2023), aggregated at the municipality level.

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 taxpayers (which are not served by local offices but by dedicated units in the regional headquarters). 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[13]. When I discuss the results, I will show that they are not affected by sample selection. As some outcomes for my analysis are available only from 2016 on, I focus on the period 2016-2022 and I define 2016 tax revenues as the average tax revenues in the period 2013-2016.

4 — Research Design

In this section, I discuss the matched difference-in-differences design I use to examine the effects of the reform.

4.1. Overview

Municipalities can be grouped into three categories based on their response to the reform. The first group consists of municipalities previously served by a local office that was closed — the *closing* municipalities. The second group includes municipalities whose office was not closed but absorbed staff and jurisdiction from a closed office — the *receiving* municipalities. The third group comprises municipalities whose local offices neither closed nor expanded — the *unaffected* municipalities. If having fewer offices worsens tax enforcement, tax revenues in *closing* municipalities should be negatively impacted. Conversely, if larger offices enhance the effectiveness of the tax authority,

13. 27 cities are state capitals, 2 additional municipalities are dropped because of population size, 8 because of missing travel time; the remaining 133 are excluded because they change jurisdiction in the sample period.

tax revenues in both *closing* and *receiving* municipalities should increase. To identify the costs of centralization, I can compare the evolution of tax revenues between *closing* and *unaffected* municipalities before and after the reform. Similarly, I can identify the benefits of centralization by comparing tax revenue trends between *receiving* and *unaffected* municipalities.

However, the reform did not affect offices at random, and it is plausible that the three groups of municipalities would be on differential tax revenue trends anyway. This would undermine the validity of the difference-in-differences design. Indeed, the evolution of tax revenues displayed in figures 7a and 7b suggest that the reform occurred in regions where tax collection was growing. However, if there is a subset of *unaffected* municipalities were on a similar trend to *closing* or *receiving*, they could serve as a suitable control group. Thus, I apply a matching algorithm to select a subset of *unaffected* municipalities that can serve as a suitable control group.

4.2. Matching algorithm

There are two comparisons of interest: *closing* versus *unaffected* municipalities, and *receiving* versus *unaffected* municipalities. Building on the approach of some recent papers (Fenizia and Saggio 2024; Jäger and Heining 2022), I employ nearest-neighbor propensity score matching to pair each *treated* municipality — either *closing* or *receiving* — with an *unaffected* municipality. The matching algorithm is run separately for each comparison. For each one, the algorithm estimates a probit model on a cross-sectional sample of municipalities, consisting of treated and *unaffected* municipalities. The probit regression relates the *treatment* to the log of local GDP over the seven years prior to the reform, after partialling out unit and region-by-year fixed effects. I use only local GDP from the pre-reform period to avoid concerns that the reform itself may affect GDP. Incorporating as many years as possible allows for maximizing the available information on each unit's dynamics[14]. Using the estimated predicted values as treatment propensities, the algorithm matches each treated municipality to the *unaffected* municipality with the closest propensity score (Ho et al. 2007, 2011). This approach matches on the trajectory of local GDP before the reform. The underlying assumption is that municipalities on similar economic trends will also exhibit similar trends in tax revenues. Since the matching is not performed on the outcome, I can inspect whether the matched groups indeed follow similar tax revenue trends before the reform, providing

14. I use seven pre-reform years as this is the longest span without missing data.

evidence in support of this assumption.

4.3. Econometric Specification

To estimate the impact of the reform on tax revenues, I estimate the following equation on the full sample

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=-4}^{2} \delta_t(\text{Treatment}_{i(m)}^{C,R} \cdot D_t) + \sum_{t=-4}^{2} \eta_t(\text{Not-Matched}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \tag{1}$$

where y_{it} is tax revenues 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[15] fixed effects α_{mt}, which account for regional time-varying shocks (e.g. economic shocks affecting a region). D_t is an indicator for each year between 2016[16] ($t = -4$) and 2022 ($t = 2$). I exclude the indicator for 2019, the year before the reform. $\text{Treatment}_{i(m)}^{C,R}$ is an indicator which takes value 1 if the municipality i is treated and 0 otherwise. Depending on the comparison, the treatment is being a *closing* or a *receiving* municipality. The matrix $X_{i,t}$ includes a set of municipality-specific controls that I will describe when introducing the results. $\text{Not-Matched}_{i(m)}$ is an indicator for municipalities which either receives a different treatment or are *unaffected* but not selected by the matching algorithm.

The coefficients of interest are the δ_t. They represent the difference between treated and untreated municipalities in tax revenues in year t. I normalize to 0 the coefficient for the year before the reform, δ_{-1}. Standard errors are clustered at the level of treatment, that in this case is the pre-reform tax jurisdiction level.

Notice that inclusion of the interaction between Not-Matched and year fixed effects allows the use of information from the full sample to estimate region-by-year fixed effects and the municipality-specific controls, while ensuring that the coefficients of interest are estimated only using the treated and untreated groups selected by the matching algorithm.

15. 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

16. I *bin* tax revenues for the previous periods, that is for the year 2016 the value of tax collection is the average over the period 2013-2016

4.4. Validity of the research design

I assess whether the matching algorithm successfully selects municipalities with similar economic activity trajectories. Figure 8 examines the economic trends in the comparison between *closing* and *unaffected* municipalities. It reports the dynamic coefficients from the equivalent of equation 1 but using three proxies of economic activity as an outcome. The top-left panel uses the log of local GDP[17]. The top-right panel uses the log of nightlights as the outcome. Nightlights have been widely used by economists as proxies for economic growth when sub-national data is unavailable or when concerns arise 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, in the bottom-left panel, I use the log of the value of agricultural production from the statistical office.

The figure shows no evidence of differential trends in local GDP between *closing* and *unaffected* municipalities. It is reassuring that, while the algorithm balances the pre-reform trajectory, there is no significant difference in the post-reform periods either. Additionally, there is no evidence of differential trends when using the other proxies for economic activity, though it is worth noting that the estimates for agricultural production are quite noisy.

Similarly, figure 9 examines economic trends in the comparison between *receiving* and *unaffected* municipalities. The coefficient on local GDP for the year 2016 is large and negative - despite being not statistically significant at the conventional levels. However, the coefficients for the subsequent years are close to zero and do not display any clear trend. As with the previous comparison, there is no evidence of differential trends when considering the other proxies for economic activity.

Overall, the algorithm successfully selects control groups that follow similar economic trends in both comparisons. With this established, to identify the causal effect of the reform on tax revenues, the key assumption is that, in the absence of the reform, tax revenues in treated and control municipalities would have followed parallel trends. While I cannot directly test this assumption, I will evaluate potential violations by analyzing the dynamic coefficients in the years leading up to the reform when discussing the results.

17. Since local GDP data is not available for 2022, only the coefficients for two post-reform periods are reported.

5 — The effects of the reform: Main Findings

This section presents the main results on the effects of the reform on tax enforcement. The reform has two components: it closes some local offices and expands others. I separately identify the effects of these two components of the reform.

5.1. The effects of closing local offices

I identify the effects of closing local offices by comparing *closing* and *unaffected* municipalities. Table 1 reports the estimated coefficients for equation 1. Column 1 reports the specification without any control. It suggests that *closing* municipalities experience a 3.5% 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 controls for time-varying effects of market access. I additionally control for the share of workers who are formal[18] (from the 2010 census). The estimate is statistically significant at the 5% level and it implies a 3.6% reduction in tax collection. In the rest of the analysis I employ the specification in column 2 as my baseline specification.

Figure 10a reports the coefficients and the 95% confidence interval for the baseline specification. The effect on tax revenues is evident already in the first period, but the point estimates are larger in the last two periods. Moreover, visual inspections of the dynamic coefficients in the years leading up to the reform suggests the absence of differential pre-trends. This lends credibility to identifying assumption of parallel trends.

18. 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.

5.2. The effects of expanding local offices

I identify the effects of expanding local offices by comparing *receiving* and *unaffected* municipalities. Table 1 reports the estimated coefficients for equation 1. Column 3 reports the specification without any control. It suggests that *receiving* municipalities experience a 2.7% increase in tax collection after the reform. Column 4 includes the same set of municipality characteristics described above. The estimate is statistically significant at the 5% level and it implies a 3.4% increase in tax collection.

Figure 10b reports the coefficients and the 95% confidence interval for the baseline specification. The effect on tax revenues is evident already in the first period, but the point estimates are larger in the last two periods. Also in this case, visual inspections of the dynamic coefficients in the years leading up to the reform suggests the absence of differential pre-trends.

5.3. Discussion and sensitivity analysis

In order to interpret the estimates presented so far as the causal impact of the reform on tax collection, I rely on the parallel trends assumption.

As discussed above, the visual inspection of the dynamic coefficients in figures 10a and 10b suggests that there is no differential pre-trends in tax collection. 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 different groups of municipalities). However, note that the inclusion of mesoregion-by-year fixed effects absorbs regional idiosyncratic shocks. Moreover, the inclusion of 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, as discussed above, there is no evidence of differential trends in economic activity after the reform. Lastly, I can control directly for two time-varying proxies of economic activity: nightlights and the value of agricultural production. Columns 1 and 3 of table 2 show that the results are unaffected. Another concern is that the results might be driven by the sample selection. Columns 2 and 4 of table 2 show that when using the whole sample the results are overall very similar.

Following the guidance of the recent econometrics literature (Freyaldenhoven, Hansen, and Shapiro 2019; Roth et al. 2023), I also assess the sensitivity of the results to violations of the parallel

trends assumption[19]. I employ the approach developed in Rambachan and Roth 2023 to construct robust confidence interval under the restriction that the magnitude of the post-treatment violation of parallel trends can be no larger than a constant M times the largest deviation observed in the pre-periods. I assess the sensitivity of my results focusing on the estimates for the second year after the reform; as one can see from the dynamic coefficients in figures 10a and 10b, the second period is when the effects are more evident. Figure 11a summarizes the sensitivity of the estimates for *closing* municipalities, by reporting the 95% robust confidence intervals for different values of the constant M. Similarly, Figure 11b summarizes the sensitivity of the estimates for *receiving* municipalities. In both cases, the results are robust up to the value $M = 0.4$. This indicates that to invalidate the conclusion, we would need to allow for a post-treatment violation of parallel trends larger than 40% of the maximum pre-treatment violation.

As one may be concerned about the results being driven by the specific choice of the matching difference-in-differences approach, I also investigate the effects of the reform using the trajectory balancing method from Hazlett and Xu 2018. This approach reweighs control units such that the averages of the pre-treatment tax revenues are approximately equal between the treatment and (reweighed) control groups. This model only allows to account for unit and year fixed effects. I report the effects of the reform on *closing* versus *unaffected* municipalities in figure 12a and the effects of the reform on *receiving* versus *unaffected* municipalities in figure 12b. The effects are qualitatively similar to the matched difference-in-differences approach. The estimate treatment effect for *closing* municipalities is a 2% reduction in tax revenues, statistically significant at the 95% level. This estimate is smaller in magnitude than the one estimated with the matched difference-in-differences. The estimate treatment effect for *receiving* municipalities is a 1.6% increase in tax revenues, statistically significant at the 95% level. Again, this estimate is smaller in magnitude than the one estimated with the matched difference-in-differences. More importantly, this approach suggests a dip in tax revenues in the year 2020; after that, there is a clear increase in tax revenues (comparable in magnitude with the DID estimates).

Overall, the results in this section show that a more centralized structure leads to heterogeneous

19. Notice that the recently-highlighted potential pitfalls of two-way fixed effects estimators (Roth et al. 2023) do not apply to my setting as there is no staggered adoption.

effects. *Closing* municipalities experience a reduction in tax collection after the reform. *Receiving* municipalities, whose office expands after the reform, experience an increase in tax revenues. In the next section I turn to ask which mechanisms explain these differential effects.

6 — The effects of the reform: Mechanisms

The previous section has shown that the effects of a more centralized structure on tax enforcement. What explains the different impact between *closing* and *receiving* municipalities? One reason could be that the reform makes it harder to collect taxes in certain areas. This would explain the revenue drop in *closing* municipalities but not the increase in *receiving*ones. Another possibility is that the reform changes how the tax authority allocates enforcement resources. This could account for both effects. In this section, I present evidence supporting both explanations and I conduct several exercises to understand the underlying mechanisms.

I first show the role of the increased distance between tax offices and municipalities in explaining the decline in tax revenues for *closing* municipalities; I provide suggestive evidence that this effect is stronger in areas where local information is more valuable. I then show that *receiving* municipalities experience a larger increase in tax revenues when they have relatively higher-revenue potential than the other municipalities merged into the same office; this is consistent with a centralized structure enabling a more efficient allocation of enforcement resources. Finally, I show that higher managerial quality leads to larger increases in revenues in *receiving* municipalities; this suggests that a centralized structure may enhance a tax authority efficiency by retaining the most talented managers and giving them a larger span of control.

6.1. Distance from the tax office

6.1.1. Overview

This section investigates the possibility that one reason why a centralized structure deteriorates tax enforcement in *closing* municipalities is that it increases the distance between these municipalities and their jurisdiction office.

I start by examining how the reform affects this distance. I report the dynamic coefficients from equation 1 using distance between a municipality and the jurisdiction tax office as an outcome.

Figure 13 shows that the travel-time by car between *closing* municipalities and their jurisdiction tax office increased on average by 1.5 hours after the reform, a 66% increase with respect to the pre-reform average.

In this context, distance may matter because it makes it harder to physically inspect taxpayers premises and collect local information. Moreover, tax agents may be more knowledgeable about the municipalities nearby their homes and one may think that they tend to live close to the office where they are located. Finally, taxpayers may perceive that the probability of detection if they evade is lower if they are farther from the tax office, and thus may evade more.

While I cannot distinguish between these possibilities, in the following I employ an instrumental variable approach to isolate the effect of distance on tax revenues. I then show that the effects of distance are stronger in areas where gathering local information is more valuable and that distance reduces the number of tax agents' visits.

6.1.2. Empirical strategy

Baseline estimating equation To study how the distance between a tax office and different municipalities impacts tax enforcement, I am interested in estimating the following regression model:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \gamma Distance^{i \to r}_{i(m)t} + \sum_{t=-4}^{2} \eta_t (\text{Not-Matched}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \varepsilon_{i,t} \quad (2)$$

where $Distance^{i \to r}_{it}$ 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. Standard errors are clustered at the level of treatment, that in this case is the municipality - each municipality is at a different distance from the jurisdiction tax office. All the other parameters are like in equation 1.

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 $Distance^{i \to r}_{it}$ 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.

Instrumental variable In order to assuage the concerns outlined above, I build an instrument for $Distance_{it}^{i \to r}$. The instrument leverages the 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. Figure 14 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) 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. $Distance_{i(-1)}^{i \to \bar{r}}$ (solid 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 (see dashed line) 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 allows 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.

The first stage equation is:

$$Distance_{it}^{i \to r} = \iota_1 Distance_{i(-1)}^{i \to \bar{r}} + \iota_2 Distance_{i(-1)}^{i \to \bar{r}} \times \text{Closing}_{i(m)} \times \text{Post}_t + \varepsilon_{i,t} \tag{3}$$

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[20]. $\text{Closing}_{i(m)}^{r}$ 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 addresses concerns about endogenous jurisdiction assignments after the

20. 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

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).

6.1.3. Results

First Stage The relevance of the instrument can be visualized in figure 15. These plots represent the correlation between the distance from the closest office in a different tax jurisdiction but same fiscal region (vertical axis) and the distance from the actual jurisdiction tax office (horizontal axis). The top panel represents the correlation (the equivalent of ι_1 from 3) before the reform. There is a clear positive correlation both for municipalities whose jurisdiction tax office closes in 2020 (blue dots) and for those whose office remains open (red dots). However, the bottom panel displays that this relationship becomes steeper after the reform for *closing* municipalities only (the equivalent of ι_2 from 3). Column 1 of table 3 presents results from estimating the first stage equation 3. As expected, both coefficients are large and positive. The Wald test statistics on the joint nullity of the coefficient is very high.

Effects on tax revenues Having shown above the relevance of the instrument, I employ it to estimate equation 2. Results are displayed in table 3. I report the coefficients from the baseline specification (column 2), the reduced form[21] (column 3), and the 2SLS (column 4). The 2SLS estimate is very similar to the potentially endogenous estimate of column 2. Notice that the F-statistic on the first stage is very high, above the standard threshold. The coefficients suggest a negative effect of distance on tax collection. One additional hour of distance from a tax office causes a 1% decline in tax collection. I also display the dynamic coefficients of the reduced form in figure 16. One can appreciate the lack of pre-trends; this strengthens the validity of the exclusion restriction assumption.

In order to test whether distance is the only factor driving the negative effects of closing offices on tax revenues in closing municipalities, I examine the relationship between distance and tax revenues in a more non-parametric way. I compute different quantiles for the instrument. That is,

21. Notice that the coefficient ι_1 is abosrved by municipality fixed effects.

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 quantiles with *Closing Post*. I consider various number of quantiles, from 2 to 5. In table 4, 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[22]. While this effect is usually not statistically significant at the conventional levels, the magnitude and direction of the coefficients suggests that distance does not explain the whole effect. This decline could be due to frictions associated with switching office.

Robustness The centralization 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 instrumental variable is isolating the effect of distance, I conduct several additional sensitivity analyses. Reduced forms and 2SLS estimates are reported in table 5.

As a first step, I run the analysis only on the sub-sample of *closing* municipalities. This rules out the possibility that the results are driven by frictions associated with changing office other than distance. All the municipalities in this sub-sample experience these other frictions, but they vary in their distance from their new tax office. 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 specification 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

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

the new offices. 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.

6.1.4. Local information

So far, the results in this section have 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. While tax agents' ability to detect evasion using technology and hard data should be unaffected by their location, their ability to gather local information may be. If local information is a mechanism behind the reduction in tax revenues after the closing of local offices, we might expect stronger effects 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 the extent of third-party reporting enforcement from the Labor Inspection Agency to investigate whether the effects of distance from tax offices on tax enforcement are stronger in areas where local information is more valuable.

Empirical strategy 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[23] (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. Specifically, newly registered workers enter the administrative records that are available to the government agencies, including the tax authority.

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

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 having high or low labor inspections based on whether they received more or less inspections than the median (or the 75th percentile) municipality in their mesoregion. With this measure at hand, I test for heterogeneity in the reduced form dynamic specification:

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=-4}^{2} \delta_t^L (Distance_{i(-1)}^{i \to \bar{r}} \times \text{Closing}_{i(m)} \times \text{Low Labor Inspection})_{i(m)} \cdot D_t)$$
$$+ \sum_{t=-4}^{2} \delta_t^H (Distance_{i(-1)}^{i \to \bar{r}} \times \text{Closing}_{i(m)} \times \text{High Labor Inspection}_{i(m)} \cdot D_t)$$
$$+ \sum_{t=-4}^{2} \eta_t (\text{Not-Matched}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \quad (4)$$

The hypothesis is that the effects of distance are stronger where third-party reporting enforcement is weaker (δ^L) and attenuated where there is more third-party reporting enforcement (δ^H).

One may be worried that the number of labor inspections is correlated with other municipalities characteristics that may affect tax enforcement beyond third-party reporting enforcement. Indeed, table 6 displays that municipalities receiving more labor inspections are different: they are larger (in terms of area, population, and local GDP), less agricultural and with a lower formality rate. While my specification controls flexibly for all these characteristics and there is no evidence of differential pre-trends in tax revenues conditional on these characteristics, these results should be interpreted as suggestive.

Results on tax revenues I report the coefficients from the dynamic specification in figure 17. The top panel classifies municipalities based on the median number of inspections, whereas the bottom panel uses the 75th percentile. In both classifications, the effect of closings on tax enforcement are driven by municipalities where there low third-party reporting enforcement.

One can see that there is no evidence of differential pre-trends between the two groups. The divergence between the two groups appears starker in the last two periods.

6.1.5. Tax agents' trips

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 tax agents' trips for work as a proxy. I restrict the sample to the trips undertaken by tax agents in local offices and count the number of trips received at the municipality-year level.

The first three columns of table 7 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 fewer trips (column 4 to 6). Columns 7 and 8 combine the extensive and intensive margin by estimating a Poisson regression[24]. Municipalities farther from a local office receive less tax agents' visits. In figure 18, I report the dynamic specifications of the reduced form. For the extensive margin specification (top-left), visual inspection of the coefficients suggests some differential pre-trends and mixed effects after the reform. Instead, for the intensive margin (top-right) and Poisson (bottom-left) specifications there is no evidence of pre-trends and the coefficients after the reform are negative (but not always statistically significant at the 95% level).

Summing up, this section has shown that increased distance between municipalities and local offices is an important channel explaining the negative effects of closing local offices. The effects are stronger in areas in which gathering local information is more valuable. The results on the number of tax agents' trips provide suggestive evidence that distance acts as a friction making it harder for tax agents to gather local information.

6.2. Allocation of Enforcement Resources

6.2.1. Overview

In this section I test the idea that a centralized structure may lead to a more efficient allocation of enforcement resources (Basri et al. 2021; Bergeron et al. 2022; Kapon, Del Carpio, and Chassang 2024). The reform assigns to *receiving* offices more enforcement resources to cover a larger

24. Notice that here I do not report the 2SLS as the first stage is linear, but the second stage is non-linear.

jurisdiction. Even if the total number of tax agents is unchanged[25], tax revenues in *receiving* municipalities can increase if the local office is reallocating relatively more resources toward these areas. This can lead to a more efficient targeting of enforcement resources if the reallocation is happening in jurisdictions where *receiving* municipalities have higher revenue potential than the other municipalities newly assigned to the same office.

6.2.2. Empirical strategy

If tax offices allocate the additional resources towards the municipalities with higher-revenue potential, one would expect a larger increase in tax revenues for receiving municipalities if they are more likely to be the priority for tax enforcement. To test this hypothesis, for each *receiving* jurisdiction after the reform I rank municipalities by a proxy of their revenue-potential, the amount of taxes collected on average in the years before the reform. I then compute the share of *receiving* municipalities that after the reform are still in the first *n* positions of this ranking, where *n* is the number of municipalities in the same jurisdiction before the reform. Tax jurisdictions for which this share is above the median are the ones for which *receiving* municipalities are more likely to be still the priority for tax enforcement. I then conduct a heterogeneity analysis using the following specification:

$$
\begin{aligned}
y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} &+ \sum_{t=-4}^{2} \delta_t^L(\text{Receiving} \times \text{No priority}_{i(m)} \cdot D_t) \\
&+ \sum_{t=-4}^{2} \delta_t^H(\text{Receiving} \times \text{Still priority}_{i(m)} \cdot D_t)_{i(m)} \cdot D_t) \\
&+ \sum_{t=-4}^{2} \eta_t(\text{Not-Matched}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \quad (5)
\end{aligned}
$$

where everything is like in equation 1, except that now I estimate the effects of being a *receiving* municipality separately for jurisdictions where they are more (*Still priority*) or less (*No priority*) likely to the priority for tax enforcement.

25. Figure 19 shows that there is a small and not statistically significant increase in tax agents per population after the reform in *receiving* offices.

6.2.3. Results

I find that the increase in tax revenues for *receiving* municipalities is stronger in these jurisdictions. Figure 20 reports the point estimates and the 95% confidence interval. The estimates suggest that the increase in tax revenues for *receiving* municipalities is driven by jurisdictions in which these municipalities are more likely to be the priority for tax enforcement. A caveat with these results is that in these jurisdictions there is also a small increase in number of tax agents per population, implying that they may also have more enforcement resources available (see figure 21).

Overall, the evidence from this exercise suggests that centralizing tax agents and decision power over a larger area in fewer offices can lead to an improved allocation of limited enforcement resources (Kapon, Del Carpio, and Chassang 2024; Bergeron et al. 2022).

6.3. Managerial Quality

6.3.1. Overview

Public sector managers can influence the productivity of the offices they oversee (Fenizia 2022) through supervision, mentoring, and the allocation of tax agents (Minni 2024; Sen 2024). As managers vary in quality, a centralized structure can be an opportunity to retain only the most talented managers and give them a larger span of control. As a first step to test this hypothesis, in this section I examine whether higher manager quality leads to larger tax collection gains from the reform. After classifying high-quality managers using data on tax agents' careers, I conduct a heterogeneity analysis for my baseline difference-in-differences model. I find that the increase of tax revenues in *receiving* municipalities is stronger where high-quality managers oversee the office.

6.3.2. Empirical strategy

Building on Minni 2024, I construct a proxy for high-quality managers based on their promotion speed. Promotions reflect how higher-level managers perceive a tax agent's success and are not solely based on seniority. I define high-flyer managers as those who reach work-level 4 at a relatively younger age. I focus on work-level 4 because it is the first level at which a tax agent can oversee a local office.

I compile a list of all managers overseeing local offices during the sample period. These are

the managers I classify. I then consider all the other tax agents deployed in local or lower-tier offices who reach work-level 4 between 2013 and 2022. Figure 22 shows the age distribution at promotion to work-level 4. I classify high-quality managers as those promoted at an age below the 25th percentile of this distribution, meaning before age 41.

The intuition behind this measure is that faster progression up the managerial ladder indicates higher performance, reflecting the tax authority's valuation of the manager's work. I validate this empirically by showing that high-flyer status correlates with future performance and personal success. While wage increases are limited in this context, being deployed to regional or central headquarters serves as a promotion. Table 8 shows that high-flyer status is positively correlated with future deployment to these headquarters.

I apply this measure to all managers overseeing local offices during the sample period. Due to substantial managerial turnover during the reform, I focus on offices with the same manager for three years around the reform (one year before and two years after). Twenty-nine out of 70 jurisdictions meet this criterion. I then conduct the following heterogeneity analysis:

$$\begin{aligned} y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=-4}^{2} \delta_t^L (\text{Receiving} \times \text{Low-flyer Manager}_{i(m)} \cdot D_t) \\ + \sum_{t=-4}^{2} \delta_t^H (\text{Receiving} \times \text{High-flyer Manager}_{i(m)} \cdot D_t)_{i(m)} \cdot D_t) \\ + \sum_{t=-4}^{2} \eta_t (\text{Not-Matched}_{i(m)} \cdot D_t) + \sum_{t=-4}^{2} \iota_t (\text{Not-Same Manager}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \end{aligned} \quad (6)$$

The hypothesis is that the effects of being a *receiving* municipality are stronger if the manager in charge of the local office around the period of the reform is a high-quality manager ($\delta^H > \delta^L$).

6.3.3. Results

Figure 23 presents the dynamic specification. Note that the high-flyer group shows more volatility in the pre-reform period, likely due to the small number of municipalities in this group. After the reform, municipalities with low-flyer managers see a small but precisely estimated increase in tax revenues. In contrast, the effect is much larger in municipalities managed by high-flyer managers.

I then repeat the analysis but further exploring heterogeneity by whether *receiving* municipalities are more or less likely to the priority for enforcement. The results are reported in figure 20. In jurisdictions in which *receiving* municipalities are less likely to be the priority for enforcement but that are overseen by a high-quality manager, *receiving* municipalities experience an increase in tax revenues; however this effect is transitory and lasts only for the first period[26]. In jurisdictions with a low-quality managers, there is no change in tax revenues. On the other hand, in jurisdictions in which *receiving* municipalities are more likely to be the priority for enforcement, there are large and positive effects on tax revenues also when overseen by low-quality managers. However, if the jurisdiction is served by a high-quality manager, the effects are larger.

In summary, this section documents that higher manager quality leads to larger tax collection gains from the reform. This provides further evidence on how a centralized structure leads to economies of scale that increase the efficiency of the tax authority.

7 — The effects of the reform: net effects

Overview After having documented the heterogeneous effects of the reform, in this section I quantify the net effects of centralization at the aggregate level, using jurisdictions as the unit of analysis. Specifically, I compare the aggregate evolution of tax collection between jurisdictions that underwent centralization and those that did not[27]. This exercise at the aggregate level has the advantage of measuring the overall change in tax collection resulting from centralization. Policymakers can use this aggregate measure to compare the changes in tax collection and the savings from reduced administrative costs due to fewer offices. However, there are two main caveats to this analysis. First, the number of observations is limited, which means the analysis is underpowered. Second, the aggregate level of analysis does not allow to control for regional yearly shocks which may bias the results.

Empirical Strategy I employ a standard difference-in-difference design:

26. One can also notice that offices are classified as having a high-quality manager based on whom was in charge until the first period only.

27. A jurisdiction is classified as having undergone centralization if, after the reform, it includes both *closing* and *receiving* municipalities.

$$y_{j(s)t} = \alpha_{j(s)} + \alpha_{st} + \rho \text{Centralized}_{j(s)} \times \text{Post}_t + + X_{j(s)t}\psi + \epsilon_{j(s)t} \tag{7}$$

where the outcome is the log of total tax collection in jurisdiction *j*, part of fiscal region *s*, in year *t*. I include jurisdiction fixed effects $\alpha_{j(s)}$ to control for time-invariant characteristics affecting tax collection. Moreover, year fixed effects α_t capture time-varying aggregate shocks. $Centralized_{j(s)}$ is an indicator equal to 1 if a jurisdiction underwent a centralization because of the reform. $Post_t$ is an indicator equal to 1 from 2020 onward. I also control flexibly for the average municipalities characteristics in a jurisdiction[28].

Results I report the results in figure 26. One can appreciate that while there is no detectable effect in first period after the reform, in the two subsequent years centralized jurisdictions experience an increase in tax revenues. The effect in the last two period is statistically significant at the 90% level. The average net effect is a 2.7% increase in tax revenues. This corresponds roughly to 301 millions Reais (2018 prices).

Moreover, one may expect that by closing local offices the tax authority saved on managerial costs. To quantify these savings, I estimate 7 but using the total managerial wages in a jurisdiction as dependent variable. Results are reported in figure 27 and imply an average reduction of 1.1 millions Reais (2018 prices).

Overall, the reform produced positive net effects for the government budget. To put things in perspective, the additional revenue and savings generated by a centralized jurisdiction are equivalent to funding the main government social assistance program, *Bolsa Família*, for approximately 50,000 households.

8 — Conclusion

This paper investigates how the organization of a tax authority across the territory impacts fiscal capacity. Leveraging a reform of the Brazilian tax authority as a natural experiment, I provide evidence that a more centralized structure increases tax revenues. However, the effects are highly heterogeneous. Tax revenues decline in areas previously served by a closed office, but increase in

28. I include the same controls of the baseline specification

areas served by an office that expanded. Additionally, I shed light on some mechanisms explaining these differences and provide suggestive evidence of a trade-off between acquiring information and internal efficiency. On one hand, after the closings increased distance makes it harder for the tax authority to gather local information on taxable activity. On the other hand, a centralized structure leads to a more efficient allocation of enforcement resources and economies of scale arising from high-quality managers being in charge of larger offices.

By uncovering some forces underlying the trade-off between a centralized or decentralized structure, this paper offers some guidance to policymakers interested in implementing a similar reform in other contexts. Specifically, they should consider the severity of geographical frictions, how important is to access local information, the distribution of revenue potential, and the pool of office managers. Additionally, they should consider whether their objective is only to maximize tax revenues or also to achieve equity in tax enforcement intensity across different regions (Bachas, Jensen, and Gadenne 2024). For instance, stark differences in tax enforcement intensity may incentivize firms to relocate in areas where is easier to evade and lead to distortions in the allocation of economic activity (Fajgelbaum et al. 2019; Dix-Carneiro et al. 2021).

More broadly, this paper has shown that a bureaucracy effectiveness can be impacted by how it is organized across the territory. Beyond tax administration, many other public sector agencies face the problem of how to organize their territorial presence to balance acquiring information and internal efficiency. Examples include agencies tasked with monitoring compliance with environmental regulations and with delivering social assistance programs (Balboni et al. 2023; Assunção et al. 2023; Muralidharan, Niehaus, and Sukhtankar 2023; Banerjee et al. 2024). Investigating the effect of different organizational choices and how they are shaped by the available monitoring and enforcement technologies is an interesting area for future research.

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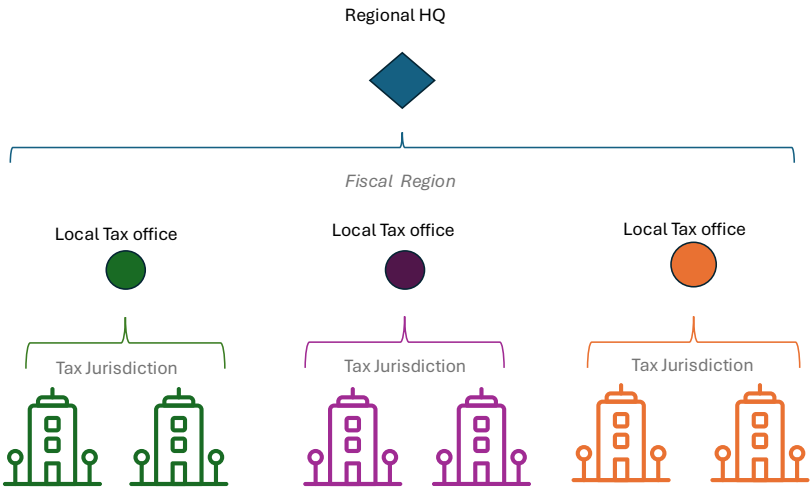
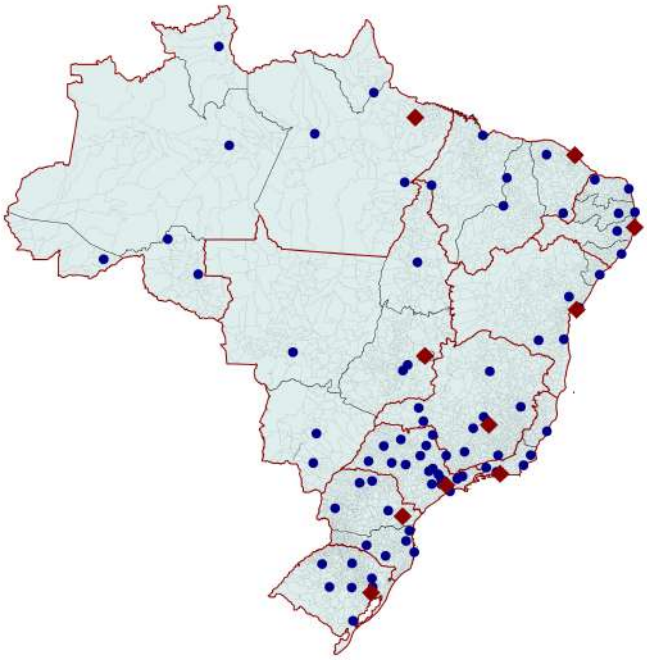
9 — Figures and tables

Figure 1 — Schematic representation of the regional structure of the tax authority



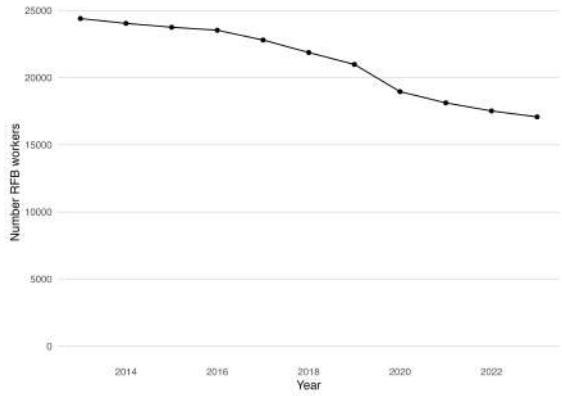
Notes. This scheme illustrates the hierarchy of the regional units of the Brazilian tax authority. There are 10 fiscal regions. Each region is managed by a regional headquarter (top tier). Each fiscal region is further partitioned in tax jurisdiction. There are 94 tax jurisdictions in 2019. Each jurisdiction is managed by a local office (middle tier). Every jurisdiction spans multiple municipalities (stylized towns at bottom)

Figure 2 — Map of the tax authority regional units

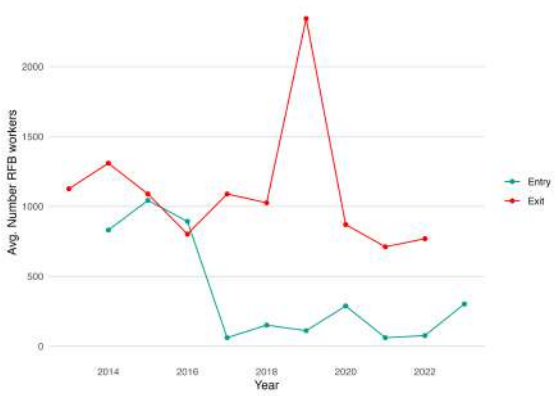


Notes. This map displays the 10 fiscal regions boundaries (red lines), regional headquarters (red diamonds) and local offices (blue dots). Thick black lines represent states borders. Thin black lines represent municipalities borders.

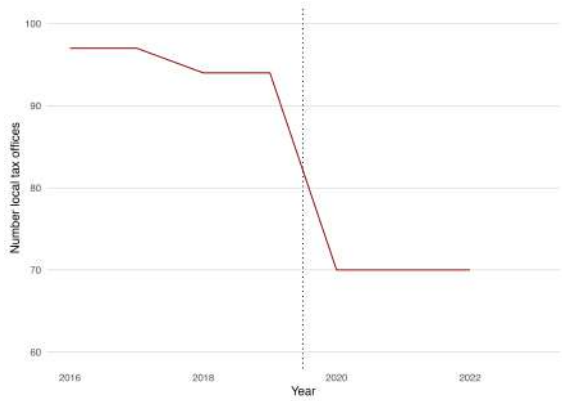
Figure 3 — The evolution of the tax authority



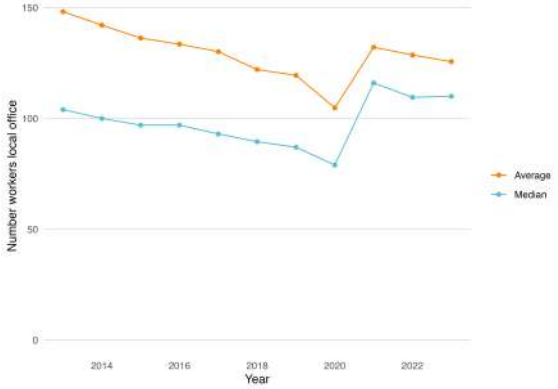
(a) Number of tax agents



(b) Number of tax agents entering and leaving



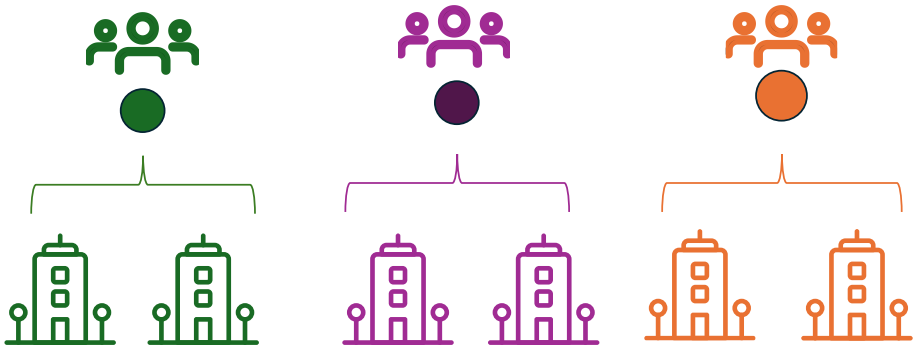
(c) Number of local tax offices



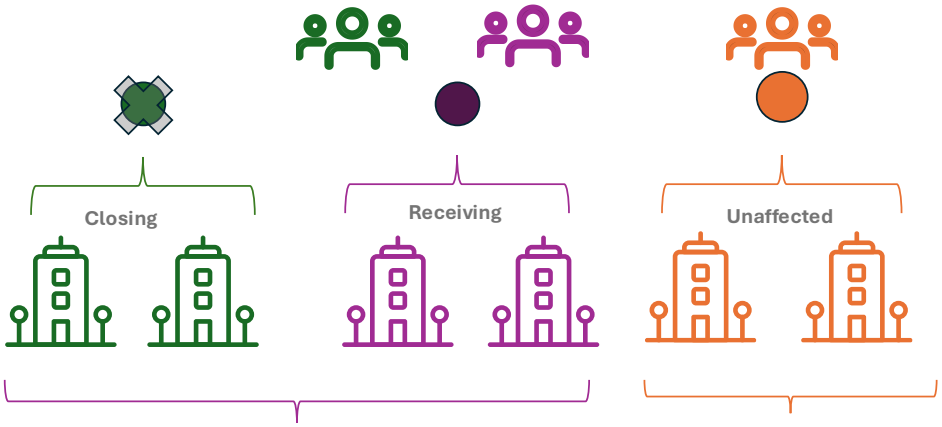
(d) Size of local tax offices

Notes. These graphs describe the evolution of the tax authority over time. The number of tax agents decline over time (top-left), due to retirements and a freezing in new hiring (top-right). The 2020 reform closes 24 local offices (bottom-left). Because of these closings, the 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



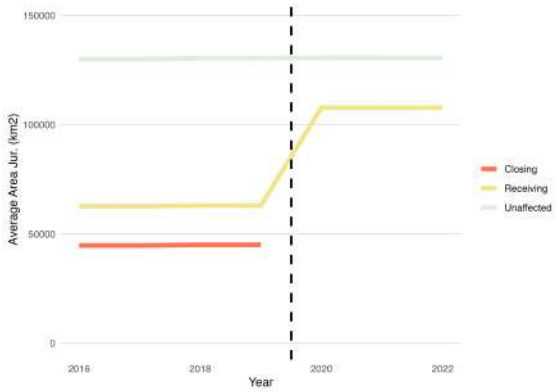
(a) Baseline



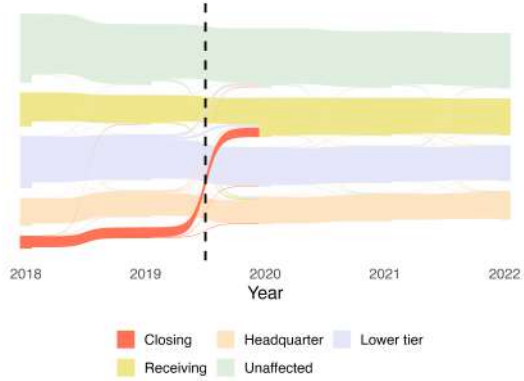
(b) Post reform

Notes. Schematic illustration of how the reform affects different municipalities. *Closing* municipalities are the ones whose jurisdiction tax office closes in 2020. *Receiving* municipalities are the ones whose tax offices absorbs municipalities and tax agents from the closing offices. *Unaffected* municipalities are the ones whose tax offices do not close nor absorb municipalities and tax agents from the closed offices.

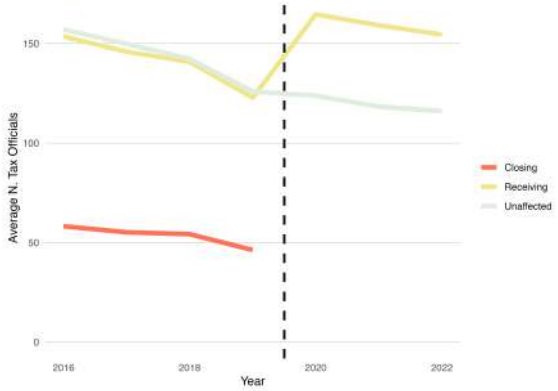
Figure 5 — The reform



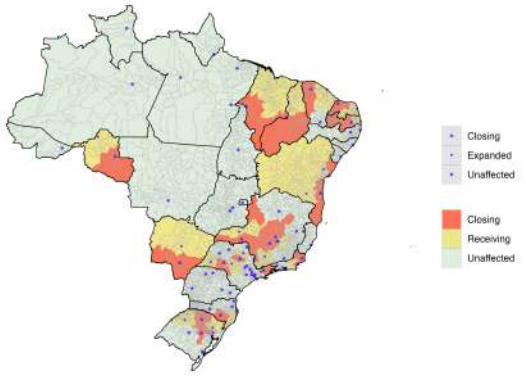
(a) Municipalities served by closing offices are assigned to some of the remaining offices



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



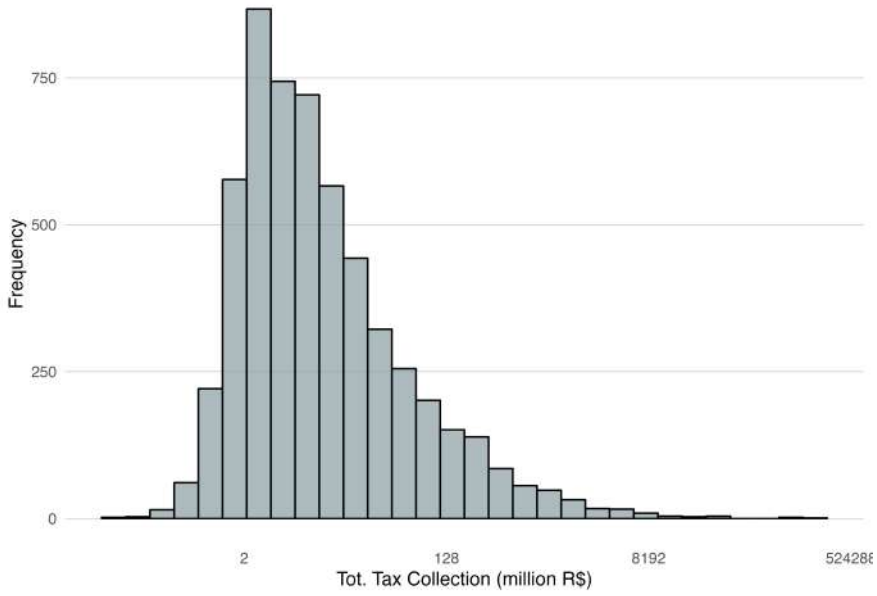
(c) Reallocation of tax agents increases personnel size in *receiving* offices



(d) Map of municipalities by group

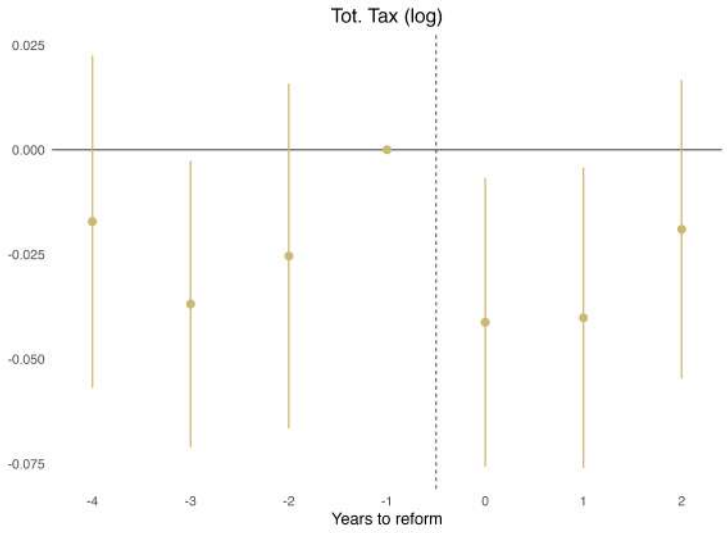
Notes. These graphs illustrate the reform. Municipalities are assigned to the jurisdiction of the remaining offices. Tax agents from closing offices are assigned to the remaining offices, increasing their size. The bottom right panel displays how each municipality is affected by the reform.

Figure 6 — Federal tax collection in 2016 by municipality

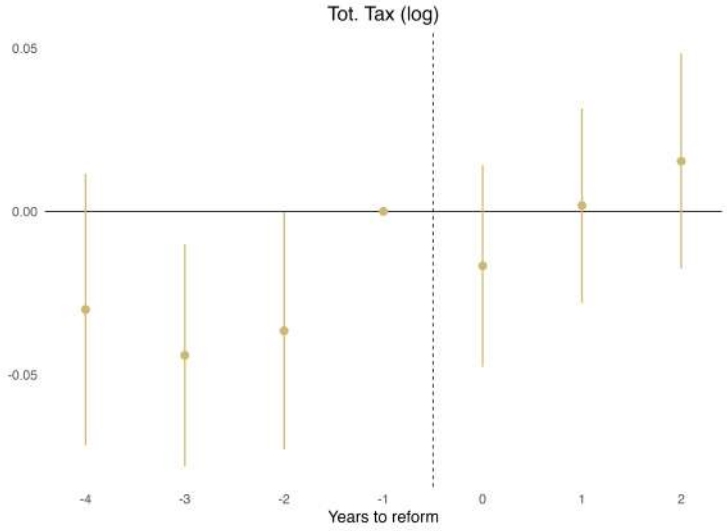


Notes. Federal tax collection (in millions of 2018 R$) in each of the 5,565 municipalities in 2016. As tax collection is highly skewed, the plot reports the log of tax collection (the horizontal axis reports the non-logged values).

Figure 7 — Closing, receiving and unaffected municipalities are on differential trends in tax collection



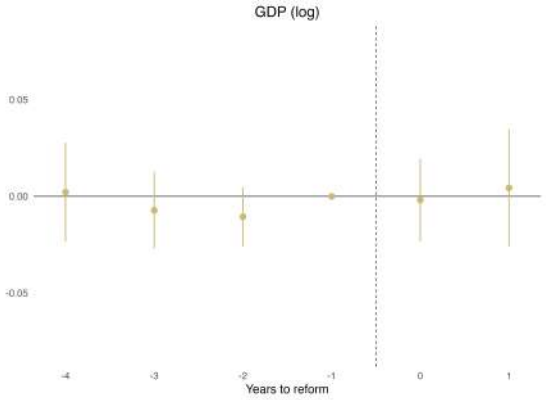
(a) Closing vs Unaffected municipalities



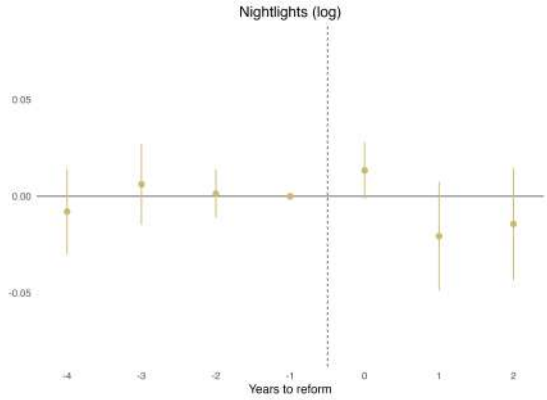
(b) Receiving vs Unaffected municipalities

Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (top panel) or *receiving* (bottom panel). The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. In the top panel an indicator for *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. In the bottom panel an indicator for *closing* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

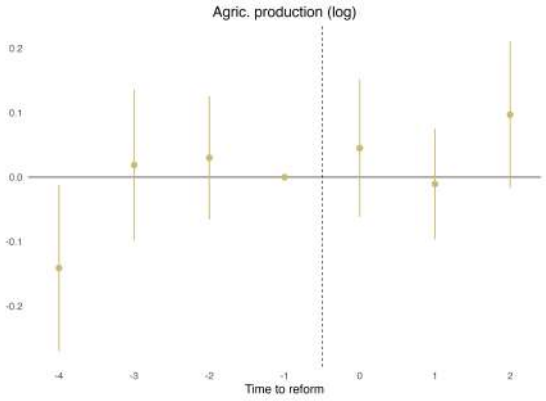
Figure 8 — The matching procedure is effective at selecting a group of *unaffected* municipalities on similar economic trends than *closing* municipalities



(a) Local GDP



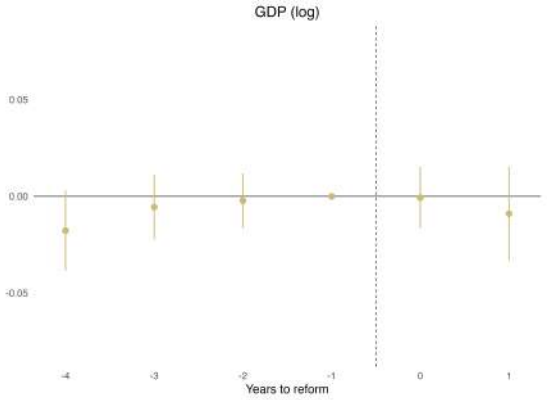
(b) Nightlights



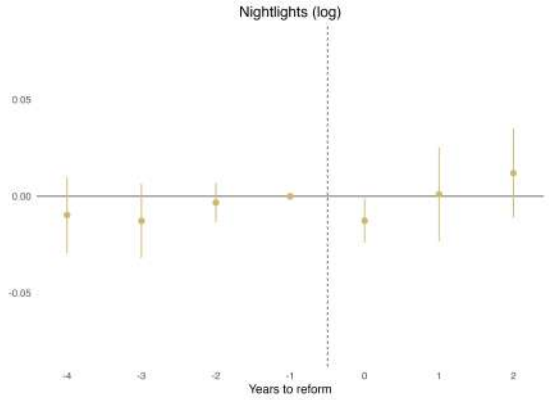
(c) Agricultural production

Notes. Observations are at the municipality-year level. The dependent variable is the local GDP (log) in top-left panel, nightlights (log) in top-right panel, and agricultural production (log) in bottom-left panel. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing*. The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. Not-matched and *receiving* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group.

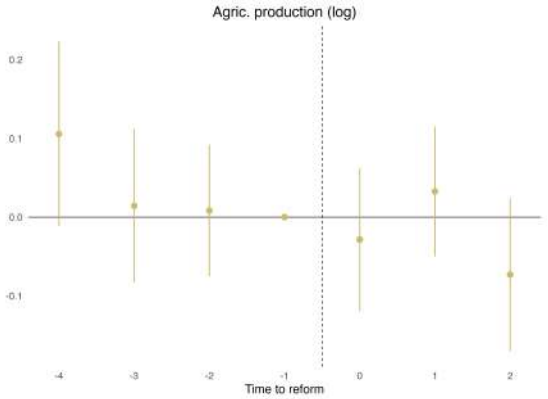
Figure 9 — The matching procedure is effective at selecting a group of *unaffected* municipalities on similar economic trends than *receiving* municipalities



(a) Local GDP



(b) Nightlights



(c) Agricultural production

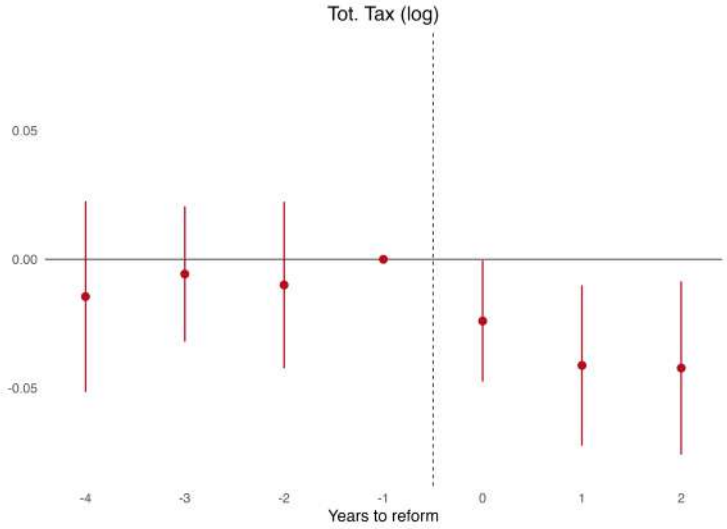
Notes. Observations are at the municipality-year level. The dependent variable is the local GDP (log) in top-left panel, nightlights (log) in top-right panel, and agricultural production (log) in bottom-left panel. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *receiving*. The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Receiving:* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. Not-matched and *closing* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group.

Table 1: Effects of the reform on tax revenues

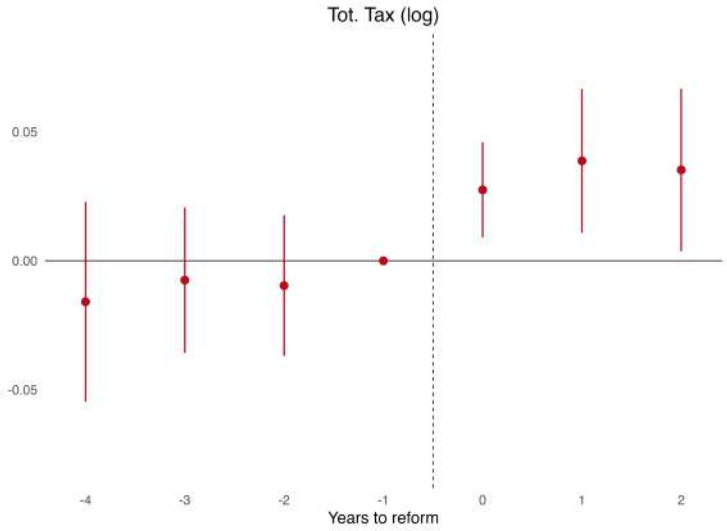
	Tot. Tax (log)			
	Closing		Receiving	
	(1)	(2)	(3)	(4)
Treatment × Period = -4	-0.009	-0.014	-0.010	-0.016
	(0.018)	(0.019)	(0.019)	(0.020)
Treatment × Period = -3	-0.005	-0.006	-0.004	-0.008
	(0.013)	(0.013)	(0.014)	(0.014)
Treatment × Period = -2	-0.004	-0.010	-0.018	-0.010
	(0.017)	(0.016)	(0.014)	(0.014)
Treatment × Period = 0	-0.023*	-0.024**	0.017	0.028***
	(0.012)	(0.012)	(0.011)	(0.009)
Treatment × Period = 1	-0.039**	-0.041**	0.031**	0.039***
	(0.015)	(0.016)	(0.014)	(0.014)
Treatment × Period = 2	-0.043**	-0.042**	0.035**	0.035**
	(0.017)	(0.017)	(0.016)	(0.016)
Municipality	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls		Yes		Yes
Dep. Var. Mean	16.1	16.1	16.1	16.1
R^2	0.99	0.99	0.99	0.99
Clusters	91	91	91	91
Observations	37,765	37,765	37,765	37,765

Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. Treatment is being a closing municipality in columns 1 and 2, and being a receiving municipality in columns 3 and 4. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. In columns 1 and 2 an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. In columns 3 and 4 an indicator for not-matched and *closing* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 10 — The effects of the reform on tax revenues: dynamic coefficients



(a) Closing vs Unaffected municipalities



(b) Receiving vs Unaffected municipalities

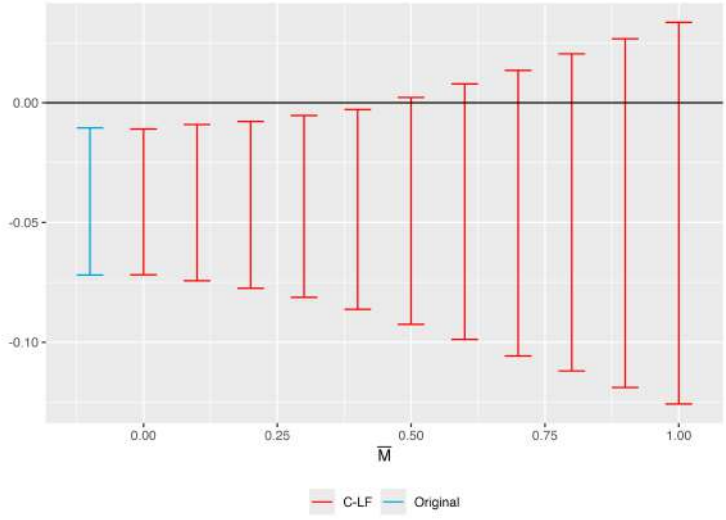
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (top panel) or *receiving* (bottom panel). The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *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.*Fixed effects:* municipality and mesoregion-by-year fixed effects. In the top panel an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. In the bottom panel an indicator for not-matched and *closing* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

Table 2: Effects of the reform on tax revenues: additional robustness

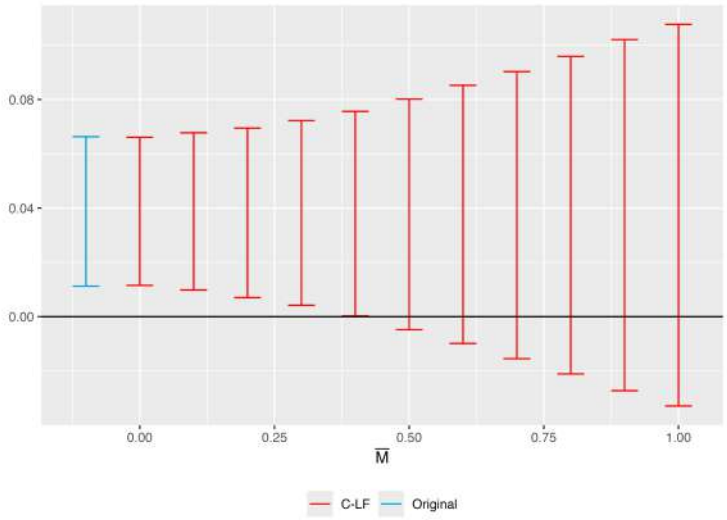
	Tot. Tax (log)			
	Closing		Receiving	
	(1)	(2)	(3)	(4)
Treatment × Period = -4	-0.012	-0.024	-0.016	-0.017
	(0.019)	(0.016)	(0.020)	(0.015)
Treatment × Period = -3	-0.006	-0.009	-0.007	-0.017*
	(0.013)	(0.012)	(0.014)	(0.010)
Treatment × Period = -2	-0.010	-0.015	-0.010	-0.006
	(0.016)	(0.015)	(0.014)	(0.011)
Treatment × Period = 0	-0.025**	-0.025**	0.028***	0.022***
	(0.012)	(0.011)	(0.009)	(0.008)
Treatment × Period = 1	-0.040**	-0.037**	0.038***	0.035***
	(0.016)	(0.015)	(0.014)	(0.011)
Treatment × Period = 2	-0.043**	-0.042**	0.035**	0.029**
	(0.017)	(0.017)	(0.016)	(0.013)
Municipality	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls	Yes	Yes	Yes	Yes
Econ Activity	Yes		Yes	
Dep. Var. Mean	16.1	16.2	16.1	16.2
R^2	0.99	0.99	0.99	0.99
Clusters	91	94	91	94
Observations	37,765	38,899	37,765	38,899

Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. Columns 1 and 3 uses the main analysis sample. Columns 2 and 4 include capitals, large cities, and municipalities which change jurisdiction in the time-frame even if their office does not close. Treatment is being a closing municipality in columns 1 and 2, and being a receiving municipality in columns 3 and 4. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. Columns 1 and 3 also include economic activity controls. *Economic activity:* nightlights (log) and value of agricultural production (log), time-varying. In columns 1 and 2 an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. In columns 3 and 4 an indicator for not-matched and *closing* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 11 — The effects of the reform on tax revenues: robust confidence sets



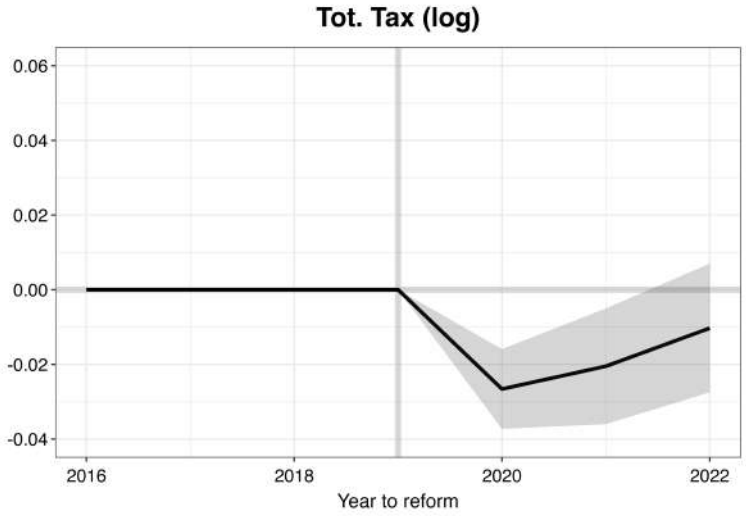
(a) Closing vs Unaffected municipalities



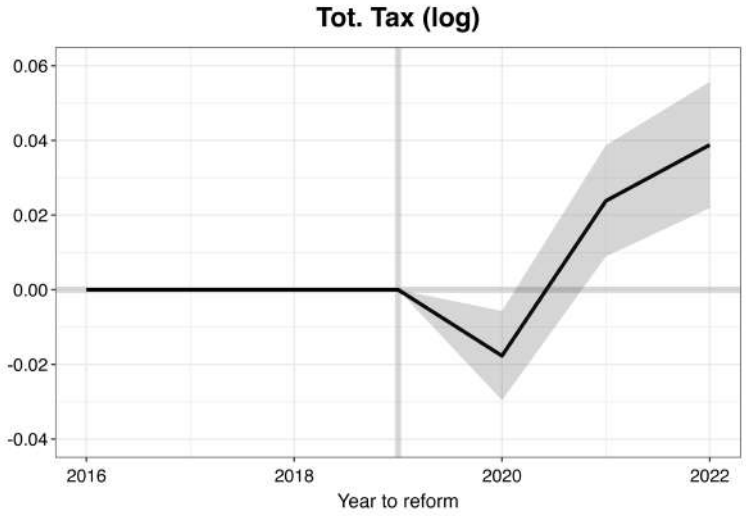
(b) Receiving vs Unaffected municipalities

Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients for the interaction between the period $t = 1$ and *closing* (top panel) or *receiving* (bottom panel). The bars represent the robust confidence sets for relative magnitude bonds, built using the approach in Rambachan and Roth 2023. The specifications are equivalent to figures 10a and 10b.

Figure 12 — The effects of the reform on tax revenues: trajectory balancing approach



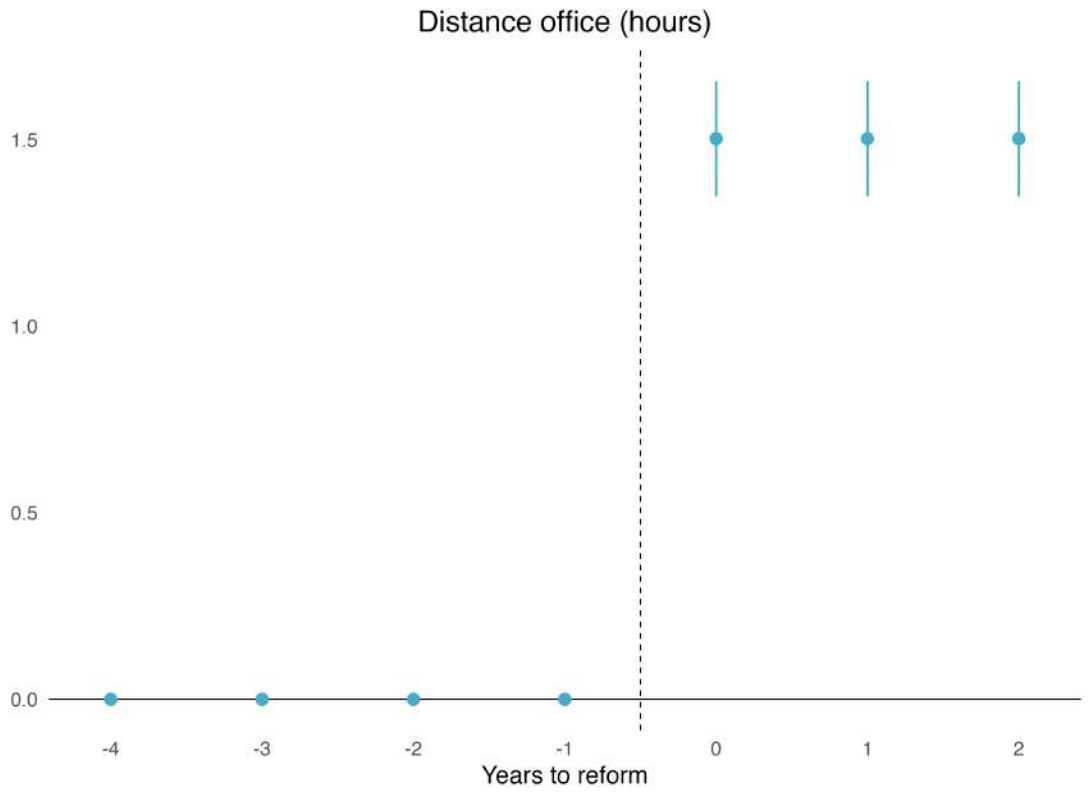
(a) Closing vs Unaffected municipalities



(b) Receiving vs Unaffected municipalities

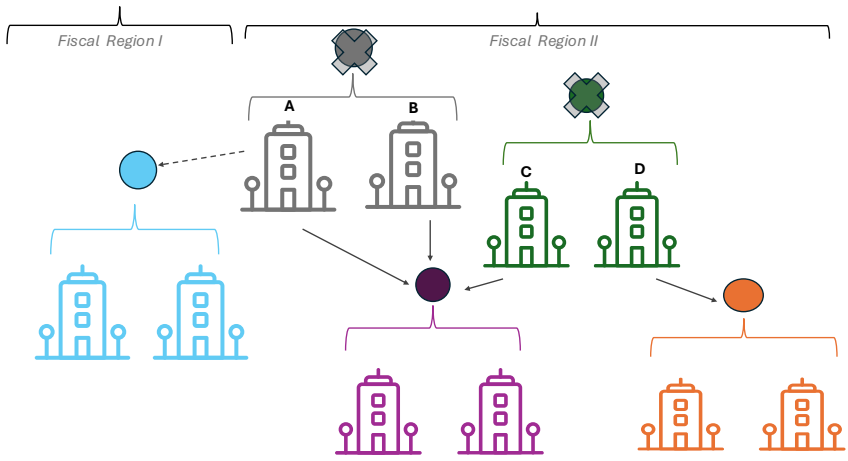
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The coefficients are estimated using the approach in Hazlett and Xu 2018. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (top panel) or *receiving* (bottom panel). *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and year fixed effects. In the top panel *receiving* municipalities are excluded from the sample. In the bottom panel *closing* municipalities are excluded from the sample. Standard errors are clustered at the municipality level.

Figure 13 — Distance between municipalities and tax offices increase after the reform



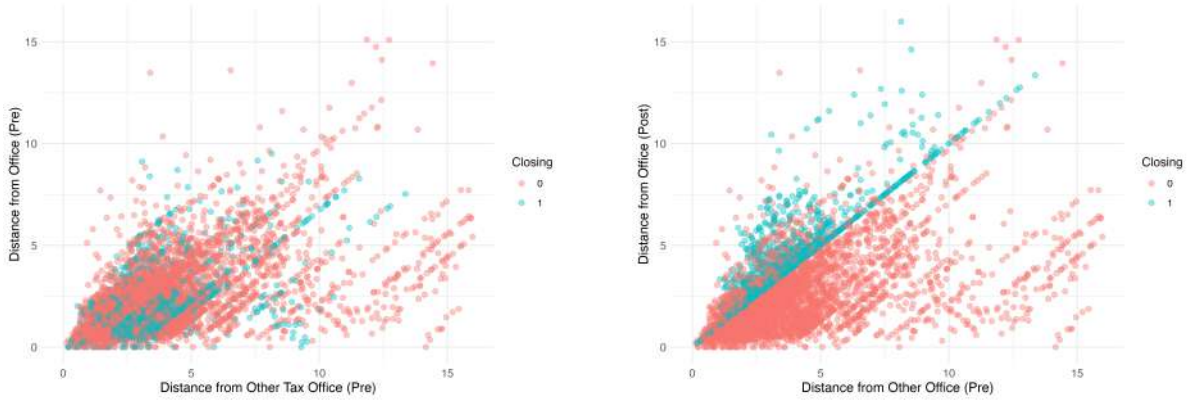
Notes. Observations are at the municipality-year level. The dependent variable is the distance (in hours) between a municipality and the jurisdiction tax office. The plot reports estimated coefficients and 95% confidence interval for the interaction between *Closing* and indicator for years. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. The coefficient for the year 2019 is normalized to 0.

Figure 14 — Illustration of the instrumental variable for distance



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

Figure 15 — Instrumental variable for distance: description



(a) Pre-reform **(b) Post-reform**

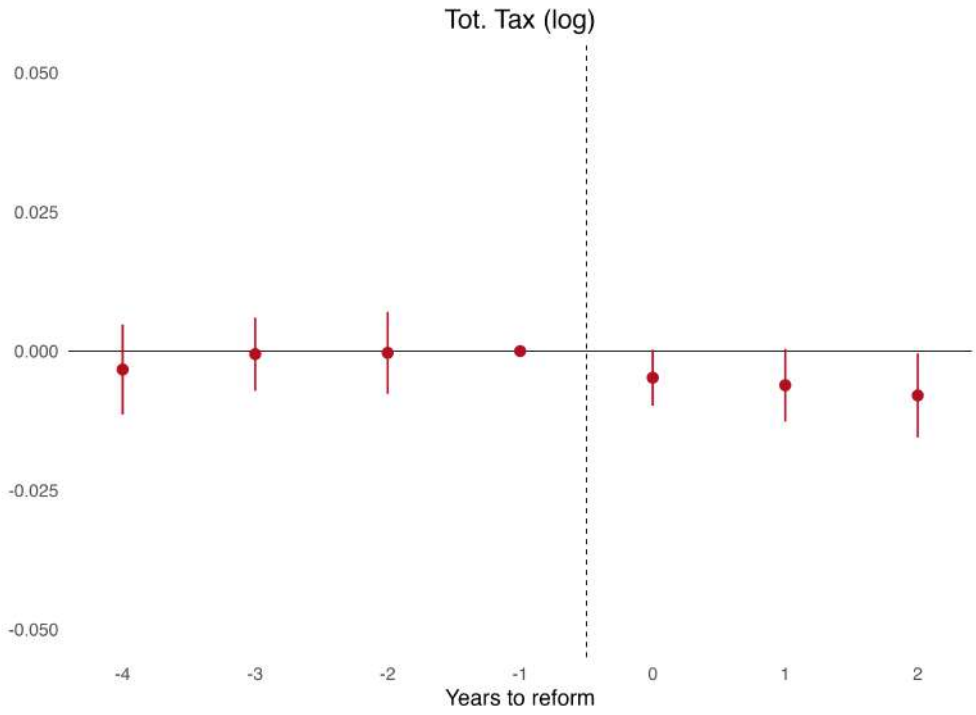
Notes. Observations are at the municipality level. The variable on the horizontal axis is the distance from the closest office in a different tax jurisdiction but in the same fiscal region (at baseline). The variable on the vertical axis is the distance from the jurisdiction tax office, before (top panel) or after (bottom panel) the reform. Green dots denote municipalities whose tax jurisdiction closed in 2020, red dot municipalities whose jurisdiction tax office did not close in 2020.

Table 3: Distance from the local office reduces tax revenues

	Distance office	Tot. Tax (log)		
	FS	OLS	RF	2SLS
	(1)	(2)	(3)	(4)
Distance other office (pre)	0.951***			
	(0.021)			
Distance other office (pre) × Closing Post	0.470***		-0.005*	
	(0.015)		(0.003)	
Distance office		-0.011**		-0.010*
		(0.004)		(0.006)
Municipality		✓	✓	✓
Region-Year FE		✓	✓	✓
Controls		All	All	All
Dep. Var. Mean	3.6	16.1	16.1	16.1
K-P First Stage, Distance office				510.3
R^2	0.95	0.99	0.99	0.99
Clusters	5,395	5,395	5,395	5,395
Observations	37,765	37,765	37,765	37,765

Notes. Observations are at the municipality-year level. The dependent variable in column 1 is the distance from the jurisdiction tax office; in columns 2, 3, and 4 is the log of federal tax collection. *Distance office:* Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *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 first stage. Column 2 reports the regression with the potentially endogenous variable *Distance office*; column 3 reports the reduced form estimates; column 4 reports the 2SLS estimates. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. *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. Not-matched and *receiving* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 16 — Distance from the local office reduces tax revenues: dynamic specification



Notes. Dynamic version of column 3 of table 3. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *closing*. The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *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. *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.*Fixed effects:* municipality and mesoregion-by-year fixed effects. An indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level.

Table 4: Distance from the local office reduces tax revenues: non-parametric specification

	Tot. Tax (log)			
	(1)	(2)	(3)	(4)
Distance other office (pre) Q1 × Closing Post	-0.028*	-0.026	-0.026	-0.024
	(0.014)	(0.018)	(0.021)	(0.025)
Distance other office (pre) Q2 × Closing Post	-0.048***	-0.040***	-0.028*	-0.035**
	(0.016)	(0.014)	(0.015)	(0.016)
Distance other office (pre) Q3 × Closing Post		-0.045***	-0.046***	-0.031**
		(0.017)	(0.016)	(0.016)
Distance other office (pre) Q4 × Closing Post			-0.052**	-0.042**
			(0.020)	(0.018)
Distance other office (pre) Q5 × Closing Post				-0.054***
				(0.021)
Municipality	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Controls	All	All	All	All
Dep. Var. Mean	16.2	16.2	16.2	16.2
R^2	0.99	0.99	0.99	0.99
Clusters	5,152	5,152	5,152	5,152
Observations	36,064	36,064	36,064	36,064

Notes. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. 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 are computed in the support of *closing* municipalities only. Thus, other municipalities outside the support are dropped by this analysis. Quantiles column 1: Q1: 0.19 - 3.18; Q2: 3.18 - 13.36. Quantiles column 2: Q1: 0.19 - 2.50; Q2: 2.50 - 3.92; Q3: 3.92 - 13.36. Quantiles column 3: Q1: 0.19 - 2.13; Q2: 2.13 - 3.18; Q3: 3.18 - 4.42; Q4: 4.42 - 13.36; Quantiles column 4: Q1: 0.19 - 1.91; Q2: 1.91 - 2.77; Q3: 2.77 - 3.60; Q4: 3.60 - 4.85; Q5: 4.85 - 13.36. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. *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. Not-matched and *receiving* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 5: Distance from the local office reduces tax revenues: additional specifications

	Tot. Tax (log)					
	OLS	RF	OLS	RF	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Distance other office (pre) × Closing Post	-0.023***		-0.019**		-0.018**	
	(0.008)		(0.008)		(0.008)	
Distance office		-0.056**		-0.050**		-0.046**
		(0.022)		(0.024)		(0.023)
Municipality	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓
2020 Tax Jur - Year			✓	✓		
Tax Jur Pair - Year					✓	✓
Controls	All	All	All	All	All	All
Dep. Var. Mean	15.9	15.9	15.9	15.9	15.9	15.9
K-P First Stage, Distance office		38.3		29.3		30.9
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

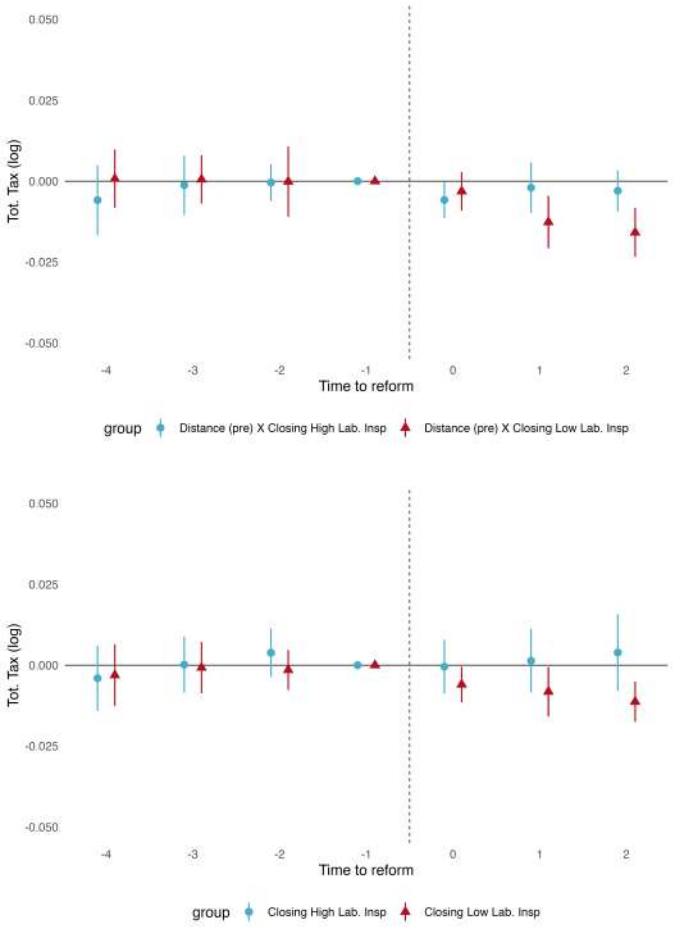
Notes. Observations are at the municipality-year level. The sample contains treated municipalities only. Odd columns report reduced form estimates, even columns report 2SLS estimates. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Distance 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, period and mesoregion-by-year fixed effects. *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. Standard errors are clustered at the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 6: Characteristics municipalities where local information is more valuable

	Low Labor Inspection		High Labor Inspection			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Erro
Agric. share (2010)	0.38	0.17	0.29	0.18	-0.09***	0.0
Second. share (2010)	0.02	0.03	0.02	0.04	0.01***	0.0
Services share (2010)	0.03	0.01	0.04	0.02	0.01***	0.0
Formal share (2010)	0.44	0.18	0.46	0.19	0.03***	0.0
Population (2010)	8.80	0.80	9.82	1.10	1.02***	0.0
Area (log)	5.92	1.16	6.43	1.31	0.52***	0.0
Nightlights (2016)	5.38	0.95	6.49	1.36	1.11***	0.0
GDP (2016)	11.60	0.86	12.69	1.45	1.09***	0.0
Agric. Prod. (2016)	8.35	2.04	8.40	2.56	0.06	0.0
Distance other tax office (hours)	3.61	3.03	5.10	16.82	1.49***	0.3

Notes. Observations are at the municipality level. The table reports mean and standard deviation for observations with *High Labor Inspection* equal to 0 or 1, as well as the differences between the means in the two groups. *High Labor Inspection*: indicator equal to 1 for municipalities above the 50th percentile in the distribution of the number of labor inspections carried out in a mesoregion in the years 2016-2019. $^{*}\ p < 0.1$; $^{**}\ p < 0.05$; $^{***}\ p < 0.01$

Figure 17 — Effect of distance stronger where local information is more valuable



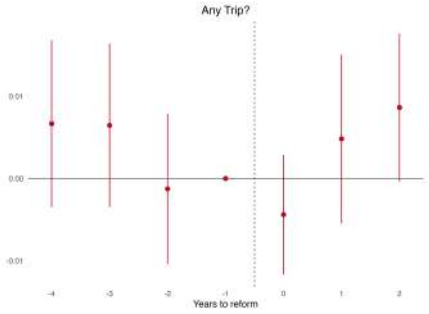
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *closing* separately for municipalities with high and low number of labor inspections. *High Lab. Insp.*: indicator equal to 1 for municipalities above the 50th (top panel) or the 75th (bottom panel) percentile in the distribution of the number of labor inspections carried out in a mesoregion in the years 2016-2019. *Closing Low Lab. Insp.*: indicator equal to 1 for municipalities below the 50th (top panel) or the 75th (bottom panel) percentile in the distribution of the number of labor inspections carried out in a mesoregion in the years 2016-2019. The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *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. *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.*Fixed effects:* municipality and mesoregion-by-year fixed effects. An indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level.

Table 7: Distance from the local office and tax agents visits to municipalities

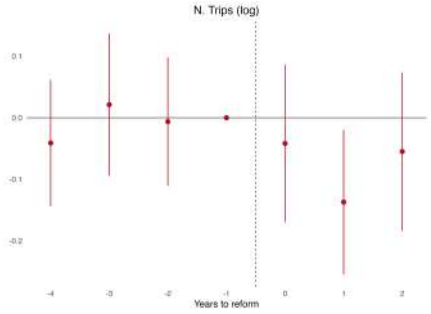
	Any trip			N. trips (log)			N. trips	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	RF	2SLS	OLS	RF	2SLS	Poisson	Poisson RF
Distance office	-0.004		0.000	-0.151**		-0.173*	-0.255***	
	(0.004)		(0.006)	(0.068)		(0.092)	(0.091)	
Distance other office (pre) × Closing Post		0.000			-0.079*			-0.121**
		(0.003)			(0.042)			(0.061)
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
K-P First Stage, Distance office			510.3			164.4		
R^2	0.56	0.56	0.56	0.84	0.84	0.84		
Clusters	5,395	5,395	5,395	1,999	1,999	1,999	1,999	1,999
Observations	37,765	37,765	37,765	4,949	4,949	4,949	13,652	13,652

Notes. Observations are at the municipality-year level. *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 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 potentially endogenous variable and column 8 reports the reduced form. *Fixed effects:* municipality, period and mesoregion-by-year fixed effects. *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. Not-matched and *receiving* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

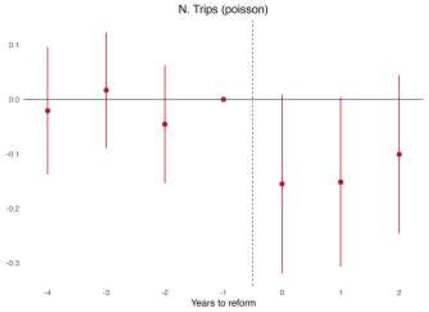
Figure 18 — The effect of distance on tax agents trips to municipalities



Notes. Dynamic version of column 2 of table 7.



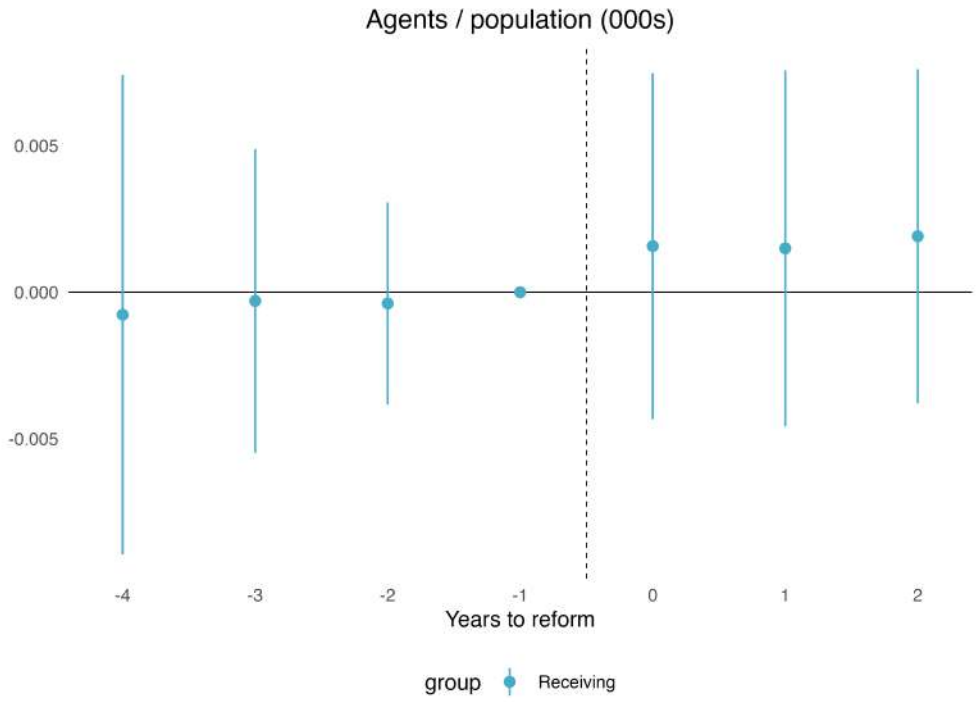
Notes. Dynamic version of column 5 of table 7.



Notes. Dynamic version of column 8 of table 7.

Notes. Observations are at the municipality-year level. The dependent variable is *Any Trip* (top left), *Log N. trips* (top right), or *N. trips* (bottom left, poisson specification). *Any Trip:* Indicator equal to 1 if any tax official from a local office visited a municipality in a year. *Log N. trips:* Log of number of times a 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. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *closing*. The coefficient for $t = -1$, the year before the reform, is normalized to 0. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *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. *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. *Fixed effects:* municipality and mesoregion-by-year fixed effects. An indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the municipality level.

Figure 19 — Number of tax agents per 1000 inhabitants



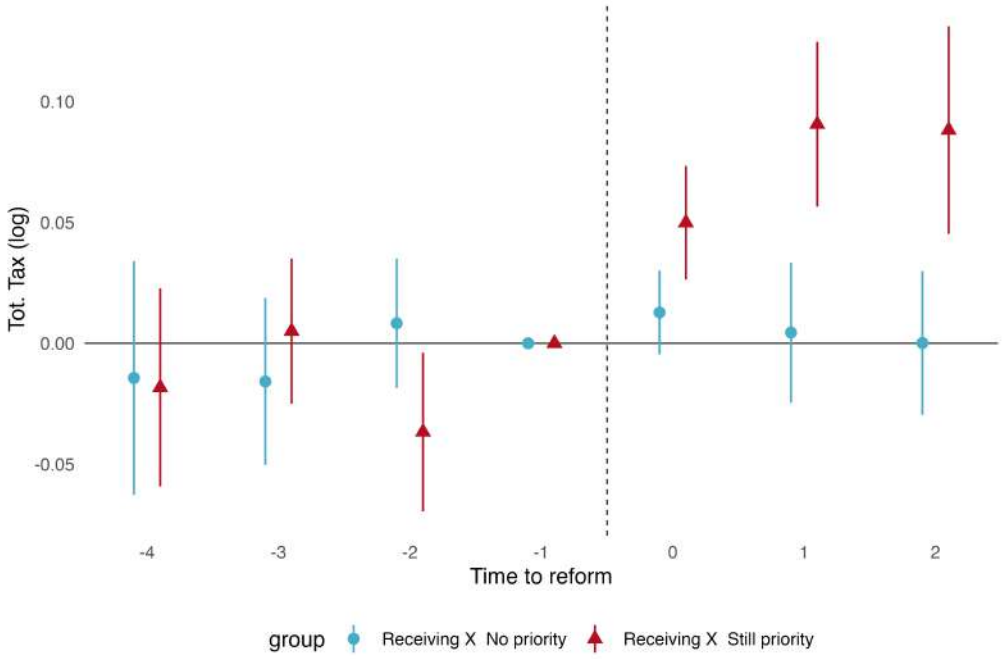
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable is the number of tax agents per 1000 inhabitants. This variable is computed using the post-reform jurisdiction boundaries.

Table 8: High-flyer managers: validation

	Achieve Central HQ	Achieve Regional HQ
	(1)	(2)
High-flyer Manager	0.046***	0.057**
	(0.017)	(0.025)
Dep. Var. Mean	0.07	0.16
R^2	0.006	0.004
Observations	1,172	1,172

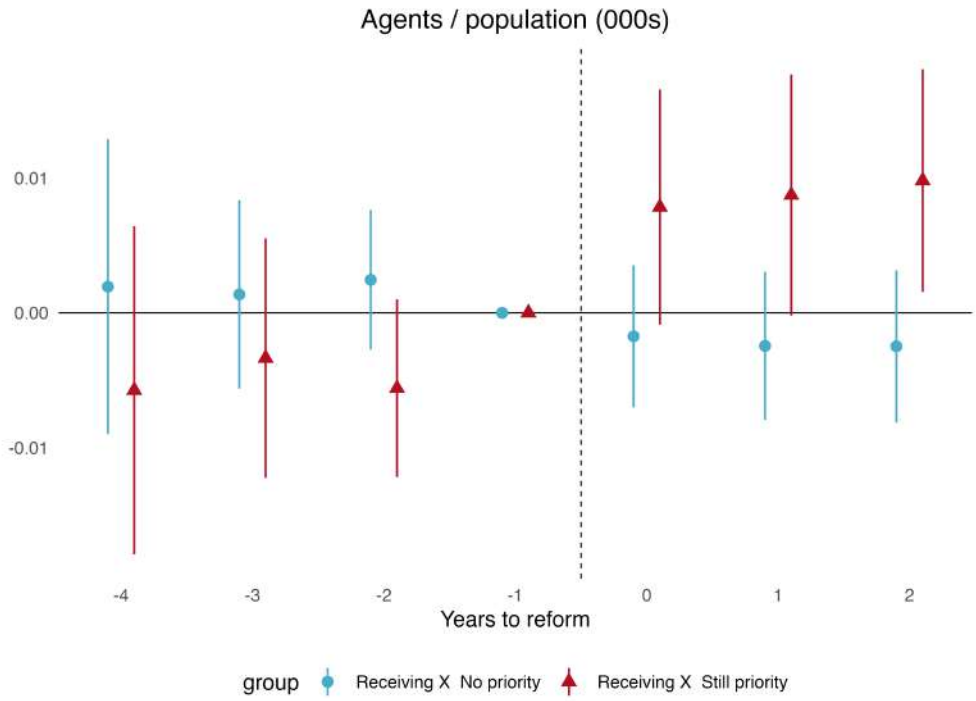
Notes. Observations are at the tax agents level. The dependent variable in the probability that a tax agent get moved to a regional or to the central headquarter at any point in time up to 2022. *High-Flyer* is an indicator equal to 1 for tax agents who got promoted to a managerial role at an age below to the 25th percentile in the distribution for figure 22. The sample excludes all the managers who will be overseeing a local office in the period 2016-2022. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 20 — Possibility of targeting more resources to receiving municipalities



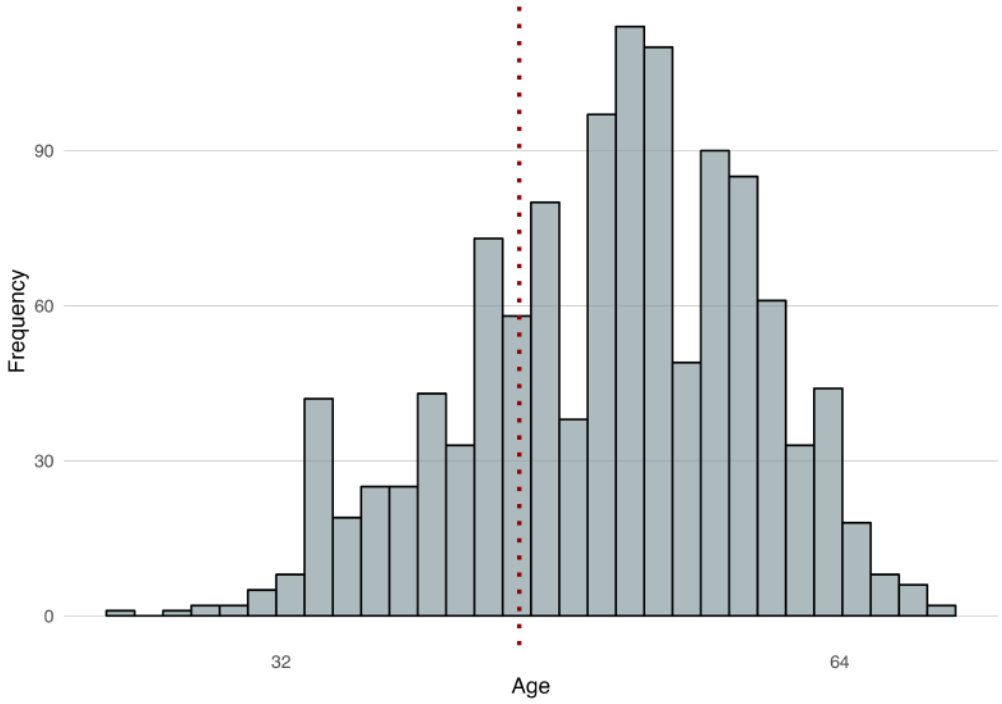
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Still priority* is an indicator equal to 1 if in a jurisdiction the *receiving* municipalities are more likely to be the priority for tax enforcement. *No priority* is an indicator equal to 1 if in a jurisdiction the *receiving* municipalities are less likely to be the priority for tax enforcement. To define this, I follow these steps: (i) for each *receiving* jurisdiction before the reform, let n be the number of municipalities (ii) for each *receiving* jurisdiction after the reform, rank in descending order all the municipalities based on their average tax revenues in the period 2016-2019 (iii) compute the share of *receiving* municipalities that after the reform are still in the first n positions of this ranking (iv) classify tax jurisdictions for which this share is above the median as the ones for which *receiving* municipalities are more likely to be the priority for tax enforcement. *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. Not-matched and *closing* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

Figure 21 — Number of tax agents per 1000 inhabitants by jurisdiction type



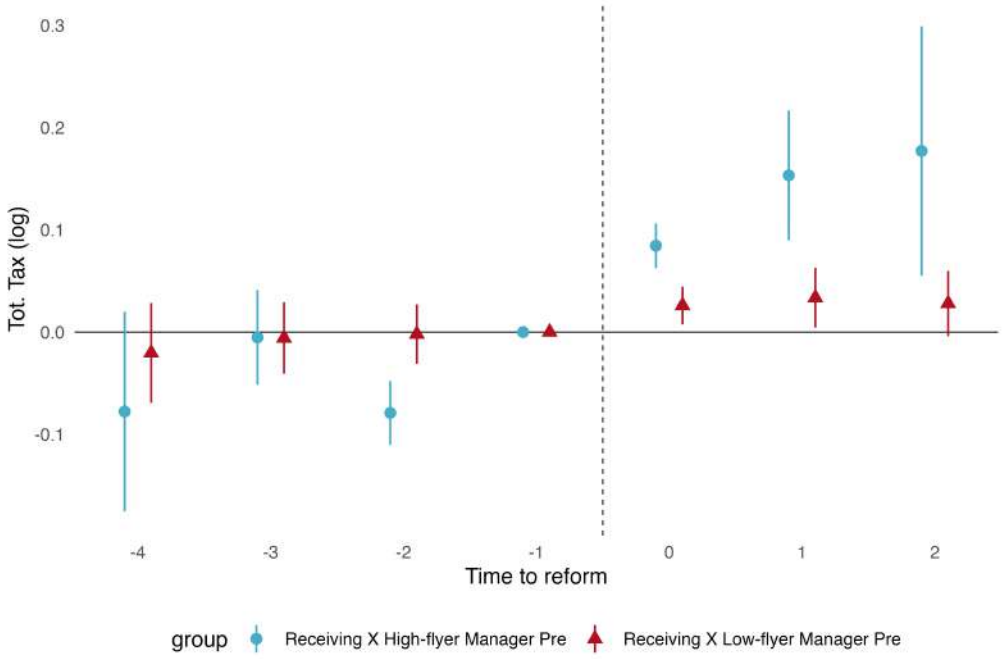
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable is the number of tax agents per 1000 inhabitants. This variable is computed using the post-reform jurisdiction boundaries. I report separate coefficients for *Still priority* and *No priority* jurisdictions as defined in figure 20.

Figure 22 — Distribution of age at which tax agents are promoted to manager



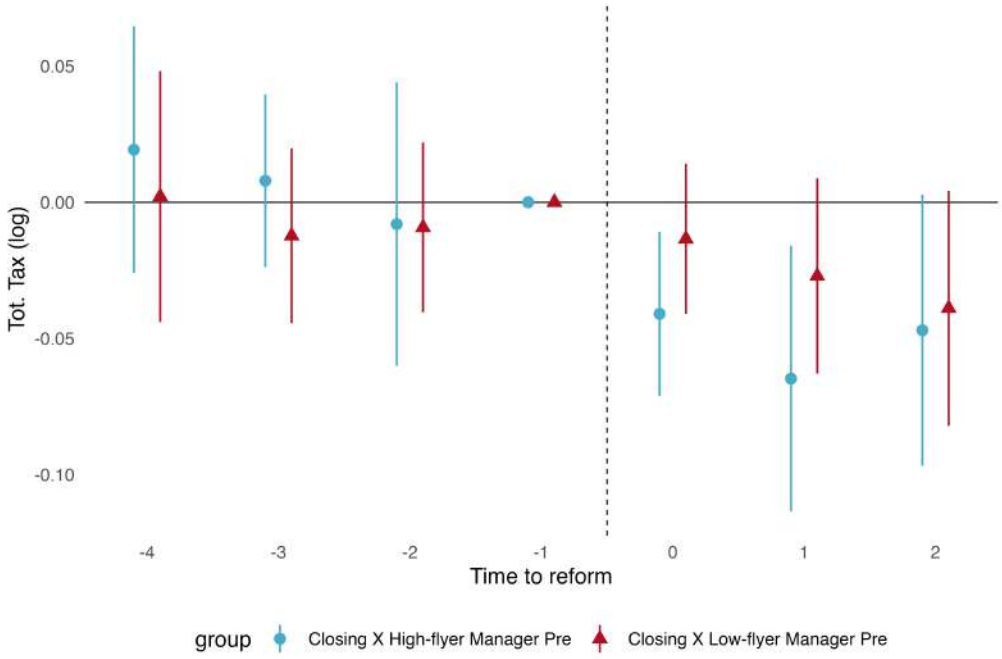
Notes. The histogram plots the age at which tax agents in local offices and lower-tier offices are promoted to a manager level. This managerial level is the first at which tax agents could oversee a local office. The red line represents the 25th percentile in the distribution. The sample excludes all the managers who will be overseeing a local office in the period 2016-2022.

Figure 23 — High-flyer managers and receiving offices



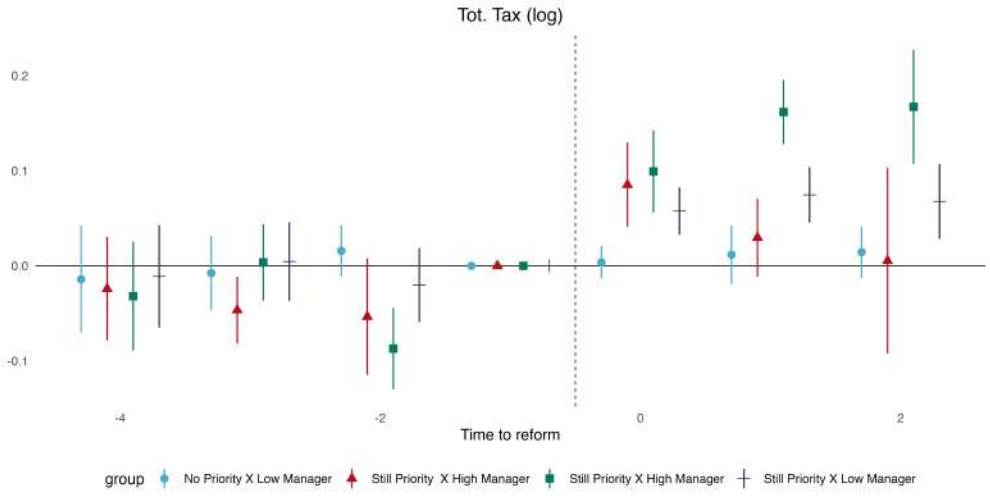
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. *High-Flyer* is an indicator equal to 1 if the manager office in charge in the period 2018-2020 got promoted to a managerial role at an age below to the 25th percentile in the distribution for figure 22. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. *Receiving* municipalities whose office tax manager changes in the period 2018-2020, not-matched and *closing* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

Figure 24 — High-flyer managers and closing offices



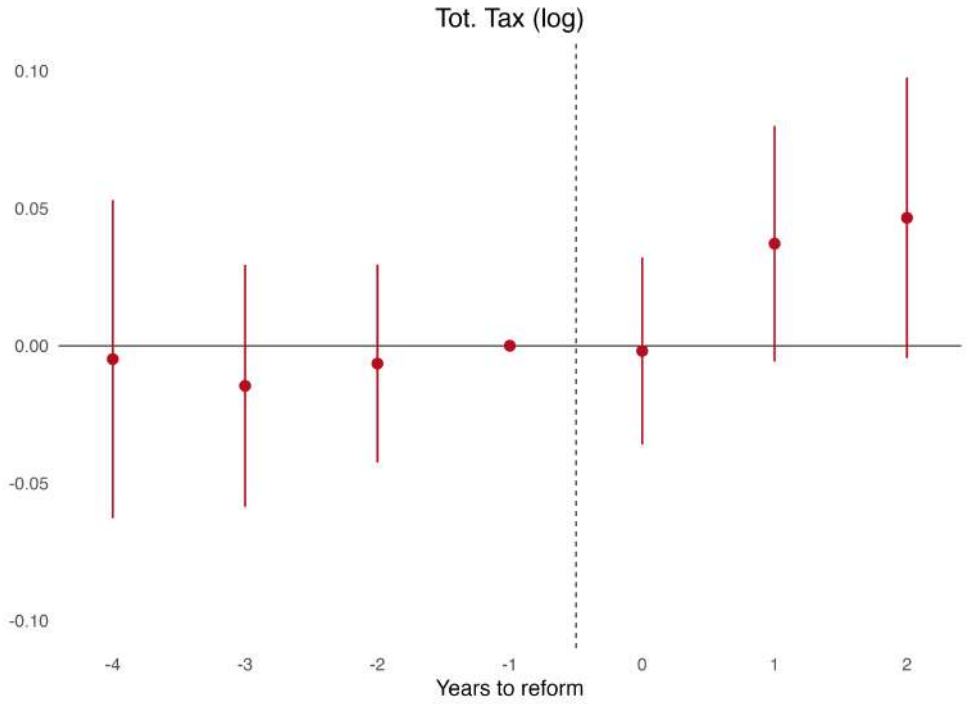
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. *High-Flyer* is an indicator equal to 1 if the manager office in charge in the period 2018-2019 got promoted to a managerial role at an age below to the 25th percentile in the distribution for figure 22. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. *Closing* municipalities whose office tax manager changes in the period 2018-2019, not-matched and *receiving* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

Figure 25 — Possibility of targeting more resources to receiving municipalities and high-quality managers



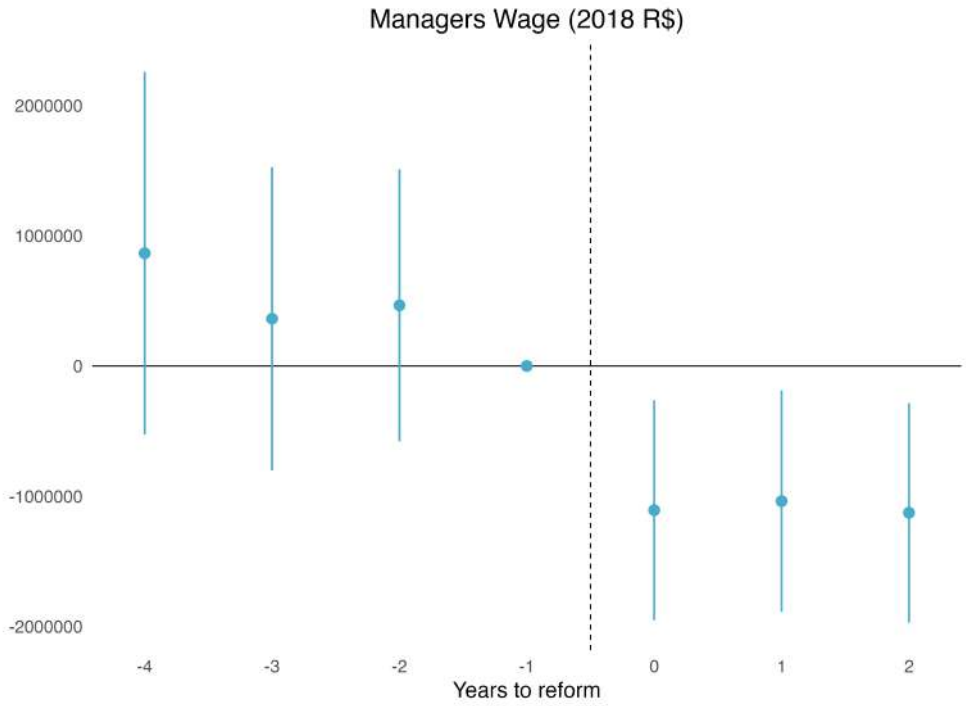
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by a closed office in 2020. *High-Give Up* is an indicator equal to 1 if in a jurisdiction the *receiving* municipalities are more likely to be the priority for tax enforcement (see definition in figure 20). *High manager* is a indicator equal to 1 if in a jurisdiction tax office is overseen by a high-quality manager (see definition in figure 23). *Fixed effects:* municipality and mesoregion-by-year fixed effects. *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. *Receiving* municipalities whose office tax manager changes in the period 2018-2020, not-matched and *closing* municipalities are in the estimation sample; an indicator interacted with year fixed effects ensures that they are not part of the control group. Standard errors are clustered at the level of the tax jurisdiction in the year before the reform.

Figure 26 — The net effect of centralization on tax revenues: aggregate level



Notes. This plot reports point estimates and 95% confidence intervals for *Centralized*. Observations are at the post reform jurisdiction-year level. The dependent variable in the log of federal tax collection. *Centralized* is indicator equal to 1 for jurisdictions which centralized in 2020. *Fixed effects:* jurisdiction and year fixed effects. The specification includes the average within the jurisdiction of baseline controls. *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. Standard errors are clustered at the level of the tax jurisdiction.

Figure 27 — The net effect of centralization on managerial wages: aggregate level



Notes. This plot reports point estimates and 95% confidence intervals for *Centralized*. Observations are at the post reform jurisdiction-year level. The dependent variable in the wage of mangers deployed in the jurisdiction local office(s). *Centralized* is indicator equal to 1 for jurisdictions which centralized in 2020. *Fixed effects:* jurisdiction and year fixed effects. Standard errors are clustered at the level of the tax jurisdiction.