Organizing Fiscal Capacity

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May 28, 2025

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Abstract

Which tax administration reforms can improve fiscal capacity in developing countries? This paper evaluates a reform of the Brazilian tax authority that centralized the structure of local tax offices, closing some offices and expanding others. Leveraging regional variation in the reform, I employ a matched difference-in-differences strategy to document how a more centralized structure affects fiscal capacity. I obtain three main findings. First, tax revenues decline in areas previously served by an office that was shut down but increase in areas served by an expanded office; the net effect is an increase in revenues in centralized regions. Second, one reason for the decline in revenues is that increased distance between tax offices and the areas they oversee reduces tax agents' ability to visit these places and gather local information. Third, one reason for the rise in revenues is that the reform has improved the allocation of resources by enabling the tax authority to focus staff effort on high-revenue potential areas and to give talented managers a larger span of control. These findings suggest that centralization can increase overall revenues but exacerbate regional inequality in tax enforcement.

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1 — Introduction

Fiscal capacity is key to economic and political development (Besley and Persson 2011; Acemoglu and Robinson 2020). However, governments in low- and middle-income countries often struggle to increase tax revenues. To overcome this challenge, governments have become increasingly interested in how to design effective tax authorities (Okunogbe and Tourek 2024; Jensen and Weigel 2024). As with all large organizations, tax authorities face the fundamental decision of how to structure their local presence. 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). While several countries have recently shifted toward a more centralized structure (OECD 2015), there is limited empirical evidence on how this impacts tax revenues.[1]

This paper investigates the impact of centralizing a tax authority's local presence on fiscal capacity. Providing empirical evidence on this issue is challenging: it requires credible identification of organizational changes in the tax authority at a scale sufficient to affect tax enforcement operations and comprehensive measurement of these changes and their effects on tax revenues. To overcome these challenges, I examine a major reorganization of the Brazilian tax authority. The reform closed some local offices and expanded others, resulting in a more centralized structure in certain regions. Leveraging this regional variation and newly collected data, I employ a matched difference-in-differences strategy to evaluate the reform's impact on tax revenues and uncover the underlying mechanisms.

I establish three main findings. First, I document how the reform impacts tax revenues. Revenues decline in areas previously served by an office that was shut down but increase in areas served by expanded offices. On net, centralized regions see an increase in tax revenues. Second, one reason for the decline in revenues is that the reform makes it harder to collect revenues in

1. Examples of countries that have centralized their local office structures in recent years include Austria, Brazil, Croatia, Denmark, Greece, Norway, and Romania.

certain areas. Increased distance between tax offices and the areas they need to monitor makes it harder for agents to visit these places and gather local information. Third, one reason for the increase in revenues is that the reform has resulted in a better allocation of resources. By centralizing tax agents and decision-making power over larger regions in fewer offices, the reform has enabled the tax authority to focus staff effort on high-revenue potential areas and to give high-quality managers a larger span of control. Taken together, these findings contribute to the literature on tax administration in developing countries by highlighting the relevance of how tax authorities are organized across the territory for fiscal capacity.

Brazil is an ideal setting for this study for several reasons. First, it is one of the largest middle-income countries in the world and assessing the impact of a large tax administration reform in this context is of interest in itself. Second, Brazil's economy is vast and heterogeneous. This makes the problem of how to organize a tax authority's local presence particularly relevant and allows for leveraging within-country variation to establish causality.

The federal tax authority is responsible for the collection of most of the government tax revenues, including income tax, social security contributions, and taxes on firms. The entire country is partitioned into 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.

I combine various data sources to measure fiscal capacity and the tax authority's organization. I obtain yearly data on the amount of federal tax revenues collected in each municipality. By consulting the tax authority's internal regulations, I reconstruct changes of tax offices' locations and tax jurisdictions' boundaries over the period of 2016–2022. The resulting dataset enables me to track the evolution of tax revenues for the same geographic units–municipalities–as they switch offices and are exposed to different organizational structures. Moreover, I assemble a novel dataset on tax agents' career using information on their deployment across offices, their managerial positions, and their trips for work. These data allow me to better understand the mechanisms through which different organizational structures affect tax revenues.

To causally identify the effect of moving to a more centralized structure, I study a reform that was triggered by mandated budget cuts and altered the number and size of tax jurisdictions in

2020. One-fourth of the existing local offices were closed, and another one-fourth were expanded. Expanded offices absorbed most of the tax agents from the closed offices. Moreover, after the reform these offices were responsible for larger jurisdictions, which included the municipalities they were already serving (*receiving* municipalities) and the ones previously served by offices that were closed (*closing* municipalities). Because of the reform, some regions have a more centralized structure. In other regions, the organization is unchanged and the same number of local offices keeps serving the same municipalities (*unaffected* municipalities). The reform 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.

My empirical strategy compares the evolution of tax revenues between municipalities differentially exposed to the reform. Because the 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.[2]

I start by documenting how the reform impacts 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 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 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 either before the reform, which just

2. Importantly, this approach allows me to assess whether this is indeed the case in the pre-reform periods.

confirms that the matching procedure is successful, or after the reform, which offers reassurance that the changes in tax revenues are not due to changes in the taxable activity. Additionally, I assemble data on local taxes (as opposed to federal taxes) collected by the municipalities themselves; these taxes are not influenced by federal tax enforcement and can serve as a useful placebo. Reassuringly, when using local taxes as an outcome, I do not observe a pattern similar to the main results.

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. To explain the revenue drop in *closing* municipalities, I test the hypothesis that increased distance between tax offices and the areas they monitor reduces tax agents' ability to gather local information. To understand why revenues increase in *receiving* municipalities, I examine the possibility that centralization leads to a more efficient allocation of resources by enabling the tax authority to focus staff effort on high-revenue potential areas and to give talented managers a larger span of control.

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; I find that municipalities that end up one hour farther from a tax office experience a 1% larger reduction in tax revenues.[3]

Why does distance matter? 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. As local information is more valuable in settings with less third-party reporting (Jensen 2022), I test this hypothesis leveraging regional variation in the intensity of third-party reporting enforcement. I use microdata on labor inspections conducted by the Ministry of Labor in the years before the reform to classify municipalities based on the intensity of third-party reporting enforcement. Heterogeneity with respect to this measure shows that the negative effects of distance on tax revenues are larger where third-party reporting enforcement is

3. The instrument leverages the baseline variation in distance between a municipality and the closest *other* office. I describe this approach and discuss its validity in Section 6.

weaker and local information is thus more valuable. This suggests that distance acts as a friction that prevents tax agents from gathering local information. Consistent with this mechanism, I geocode data on the trips for work carried out by tax agents and show that municipalities farther away receive fewer visits.

To understand why the reform increases tax revenues in *receiving* municipalities, I then examine how the centralization affects the allocation of enforcement resources. As a result of the reform, centralized offices have more tax agents and oversee a larger jurisdiction. However, they may find it 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. These are the jurisdictions in which focusing staff effort on *receiving* municipalities should be more efficient. 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 (Bergeron et al. 2022; Kapon, Del Carpio, and Chassang 2024).

To further explore the possibility that centralization leads to economies of scale, I focus on the role of local offices managers. By reducing the number of office managers, a centralized structure can be an opportunity to retain only the most talented ones and give them a larger span of control. To take a first step in testing this hypothesis, I investigate 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 Minni (2024) and define high-quality managers as those promoted to manager at a relatively younger age. 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 tax 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 experience a 2.7% increase in tax revenues. As a comparison, the additional revenues 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 document that centralization increases overall revenues but exacerbates regional inequality in tax enforcement. Moreover, by uncovering some forces underlying the trade-off between a centralized or decentralized structure, this paper can offer guidance to policymakers interested in implementing a similar reform in other contexts. For instance, a centralized structure may lead to larger revenue gains in contexts characterized by geographically concentrated economic activity, high levels of formality, and limited geographical frictions. More broadly, the results underscore the importance of how a bureaucracy's local presence is organized for its effectiveness.

Related Literature This paper relates and contributes to several strands of the literature. First, it contributes to the literature on taxation in developing countries.[4] Many papers have highlighted the importance of third-party reporting for tax enforcement (Gordon and Li 2009; Pomeranz 2015; 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), governments have become increasingly interested in which administrative reforms can increase tax capacity (Jensen and Weigel 2024).[5] Past work has studied the role of incentives for tax collectors (Khan, Khwaja, and Olken 2016, 2019), their selection (Moreira and Pérez 2024), specialization (Basri et al. 2021), and the potentialities of technology (Fan et al. 2018; Dzansi et al. 2022; Okunogbe and Tourek 2024). This paper highlights the relevance of how tax authorities are organized across the territory for fiscal capacity. In a related paper, Balán et al. (2022) study property tax collection in the Democratic Republic of the Congo and find that delegating tax collection to local city chiefs boosts revenues by enabling the provincial tax ministry to leverage chiefs' local information. I document that, even without delegating to external actors, tax authorities can leverage local information for successful tax enforcement in contexts

4. It is also related to a large literature on tax administration and tax compliance in the Unites States and other high-income countries (Allingham and Sandmo 1972; Kleven et al. 2011; Luttmer and Singhal 2014; Keen and Slemrod 2017; Slemrod 2019; Rubolino 2023; Boning et al. 2024; Elzayn et al. 2024; Battaglini et al. 2024).

5. A related literature has examined how administrative reforms have impacted fiscal capacity in a historical context (Xu 2018; Chambru, Henry, and Marx 2021; Chiovelli et al. 2024; Cantoni, Mohr, and Weigand 2024).

with limited third-party reporting.[6] However, tax agents' ability to gather local information is constrained by how far they are from the areas they need to monitor.[7] One implication of these results is that differences in the observability of economic activity may determine the best way of organizing a tax authority, which is in line with the literature studying how observability affects the creation and evolution of states (Sánchez De La Sierra 2020; Garfias and Sellars 2021; Mayshar, Moav, and Pascali 2022). Moreover, I complement several papers that investigate the relevance of public sector managers (Rasul and Rogger 2018; Fenizia 2022) by documenting their importance for tax authorities.

Second, I contribute to the literature on how polity size affects government performance and economic growth[8] (Alesina and Spolaore 1997; Bardhan 2002; Boffa, Piolatto, and Ponzetto 2016). Recent empirical studies using quasi-experimental methods highlight the role of increased political accountability and reduced political neglect (Tricaud 2021; Dahis and Szerman 2024; Narasimhan and Weaver 2024; Cassidy and Velayudhan 2024). However, this channel is absent in my context, as I focus on bureaucratic jurisdictions rather than political units, and bureaucrats are not elected. This allows me to isolate the trade-off between gathering local information and a more efficient allocation of resources. By examining changes in local structure within a large organization, I also relate to the literature on decentralizing decision-making power within firms (Kala 2019; Aghion et al. 2021) and public sector bureaucracies (Bandiera et al. 2021; Vannutelli 2022). While these studies explore the allocation of autonomy across layers, my focus is on a reform that retains decision-making power within the same layer but alters its geographical scope. By highlighting that this dimension can impact how the organization decides to allocate staff effort, I complement recent research on how to optimally target limited resources for tax enforcement (Bergeron et al. 2022; Kapon, Del Carpio, and Chassang 2024) and environmental policies (Assunção et al. 2023).

6. In doing so, I speak to the literature on the value of local information in governance across various domains (Duflo et al. 2018; Basurto, Dupas, and Robinson 2020; Dal Bó et al. 2021; Rogger and Somani 2023). 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).

7. This finding relates 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).

8. A large literature instead examines the delegation of fiscal responsibility to lower-tier administrative units (Tiebout 1956; Oates 1993; Gadenne and Singhal 2014). In my context, fiscal responsibility remains within the government, and the reform changes how the tax authority's local presence is organized. This removes the political competition and yardstick competition channels.

More broadly, I contribute to the literature on public sector personnel (Finan, Olken, and Pande 2017; Besley et al. 2022) by leveraging microdata on an entire bureaucracy to evaluate how its effectiveness is impacted by a system-wide reform.

The rest of the paper is organized as follows: Section 2 describes the institutional setting and the reform of the tax authority. Section 3 introduces the data sources. Section 4 lays out the research design and empirical strategy. Section 5 presents evidence on the reform's impact on tax revenues, showing how a centralized structure leads to a decline in revenues in certain areas and to an increase in revenues in other areas. Section 6 explores the mechanisms that contribute to these differential effects. Section 7 discusses the net effects of the reform. 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 in the world 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 (Spilimbergo and Srinivasan 2019). It wasn't until 2021, after the COVID crisis, that GDP returned to 2014 levels.

Brazil has three levels of government: federal, state, and municipal. As of 2024, there are 27 states and 5,570 municipalities.[9] The provision of public services is highly decentralized,

9. There are 26 states and one federal district, which includes the capital, Brasilia. 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.

with municipalities responsible for essential services like education, health, and transportation. Funding for these services comes mainly from intergovernmental transfers from the state and federal governments.

Most tax collection responsibilities lie with the federal government, which, as of 2021, manages 56% of total tax collection. This includes personal and corporate income taxes, social security contributions, VAT on gross revenues and manufactured products, financial transaction contributions, and taxes on net revenues. Smaller components include taxes on fuel, insurance, and rural land. States handle VAT on sales and services, vehicle taxes, and inheritance taxes, accounting for about 39% of total tax revenue. Municipalities collect urban property and service taxes, representing approximately 5% of total taxation.

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 tax authority manages tax administration, customs, and the fight against illicit trafficking (Ezequiel 2014, 2018). The primary roles within the tax authority are tax auditors and tax analysts. Tax auditors are the most prestigious and highest-paid role. Salaries for tax agents 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 tax authority is composed of central units in Brasília and regional units across the country.

The regional structure is illustrated in figure A1. First, the country is divided into 10 fiscal regions.[10] Each region is managed by a regional headquarters (light green diamonds in the figure). Second, each fiscal region is partitioned into tax jurisdictions. Each tax jurisdiction spans several municipalities and is overseen by a local office (dark green dots in the figure)[11]. As of 2019, there are 94 tax jurisdictions.

Tax enforcement responsibilities are shared between central units in Brasília, regional head-

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

11. 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*) within their fiscal region, while local offices, along with headquarters, oversee the lower-tier offices within their jurisdiction.

quarters, and local offices (Receita Federal do Brasil 2020).[12] Large taxpayers are managed by specialized teams at regional headquarters or by two dedicated offices in São Paulo and Rio de Janeiro. All the 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 information.

2.3. The reform

The 2020 reform reshaped the regional component of the tax authority by closing 24 local offices and expanding 24 local offices (see figure A3c).[13] The expansion occurs because municipalities and tax agents from closed offices are reassigned to other offices. 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 total personnel size (figures A3a and A3b), leading to a shrinking of the average local offices size (see figure A3d), likely played significant roles in triggering the 2020 reform.

Figure A4 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 is closed by the reform—the *closing* municipalities. Second, municipalities whose jurisdiction tax office is expanded by 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. The map in figure 2 depicts which municipalities belong to each group and shows that there is considerable regional variation in how the reform pans

12. 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.

13. A smaller reform happened in 2017, closing three local offices which were serving 55 municipalities. Apart from closing local offices, both the 2017 and the 2020 reforms 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 during the period 2016-2022 for reasons other than their office being closed down because of the 2020 reform.

out. Figure A5 describes the reform with data. The reform redraws jurisdiction boundaries and reallocates tax agents. *Closing* municipalities are assigned to a new jurisdiction tax office (figure A5a). 85% of tax agents from closed offices are allocated to the expanded offices (figure A5b), increasing their staff size (figure A5c).

Because of the reform, certain regions end up with a more centralized structure. They have fewer, larger offices serving wider jurisdictions. Other regions are instead left unaltered. My empirical strategy leverages this regional variation to study how centralization impacts tax revenues.

3 — Data

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

Geographical units I use the 5,565 municipalities present in 2010 as geographic units.[14] 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's 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 also obtain the locations of the local tax offices (*delegacias da receita federal do brasil*). I complement this organizational structure with regulations

14. 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).

specifying the boundaries of tax jurisdictions for each local office (Receita Federal do Brasil 2012).[15]

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 A2), in the analysis I use its log as the main outcome. Additionally, I use data on the collection of municipal taxes (urban property tax and tax on services) from the Brazilian Public Sector Accounting and Tax Information System (Siconfi) accessed through Base Dos Dados (2022).

Tax agents' career I collect and harmonize 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 over this period. 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 collect and harmonize data on the receipts of the 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.[16] Additionally, I geo-reference the destination for 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

15. Every year there are many regulations about these jurisdictions. However, outside the years in which there are office closings, the jurisdiction re-drawing are minimal. 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.

16. 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

(agriculture, secondary, services), the share of formal employment,[17] 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 the measure of municipality-level GDP (available up to 2021) from the statistical office (IBGE 2023; Base Dos Dados 2022). I also use data on the value of total agricultural production at the municipality level from IPEA (2023). I deflate nominal variables to 2018 prices. As an additional proxy of economic activity, I use VIIRS Nighttime Lights data from the Earth Observation Group (EOG 2023), aggregated at the municipality level.

Sample selection As some outcomes for my analysis are available only from 2016 on, I focus on the period 2016–2022. 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). To avoid contamination of the analysis by other tax jurisdiction changes, I exclude municipalities that change jurisdiction during the period 2016–2022 for reasons other than their office being shut down by the 2020 reform. 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 an analysis sample of 5,395 municipalities.[18]

4 — Research Design

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

17. 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.

18. 27 dropped municipalities 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.

4.1. Overview

As described above and depicted in figure A4, municipalities can be grouped into three groups based on how they are exposed to the reform: *closing*, *receiving*, and *unaffected* municipalities. It seems reasonable to expect that the reform's impact on tax enforcement differs between *closing* and *receiving* municipalities. In *closing* municipalities, tax enforcement responsibilities are shifted to a new office, which tends to be located farther away. On the other hand, *receiving* municipalities continue to be served by the same office, but with a larger staff and a wider jurisdiction to oversee. Therefore, I conduct two distinct comparisons. First, I compare the evolution of tax revenues between *closing* and *unaffected* municipalities. Second, I compare the evolution of tax revenues 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 local GDP (displayed in figures A6a) and of tax revenues (A6b) suggests that the reform occurred in regions where economic activity and tax collection were growing relatively faster. However, if there is a subset of *unaffected* municipalities that were on a similar trend to *closing* or *receiving* municipalities, 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 (Jäger and Heining 2022; Fenizia and Saggio 2024), I employ nearest-neighbor propensity score matching without replacement 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 trajectory of 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.[19]. 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) In figure A7, I report a map displaying for each municipality how it is exposed to the reform and, for *unaffected* municipalities, whether they are selected by the matching algorithm and, if yes, for which comparison. This approach matches on the trajectory of local GDP before the reform. The motivation for this approach is that municipalities on parallel trends in economic activity are more likely to be on parallel trends in tax revenues as well. Since the matching is not performed on the outcome, I can provide evidence in support of the parallel trends assumption by inspecting whether the matched groups follow similar tax revenue trends before the reform.

4.3. Econometric Specification

To estimate the impact of the reform on tax revenues, I estimate the following equation on the full analysis 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 fixed effects α_{mt}, which account for regional time-varying shocks (e.g. economic shocks affecting a region).[20] D_t is an indicator for each year between 2016 ($t = -4$) and 2022 ($t = 2$).[21] $\text{Treatment}_{i(m)}^{C,R}$ is an indicator which takes value 1 if the municipality i is treated and 0 otherwise. Depending on the comparison, the treatment is either 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 receive a different treatment or are *unaffected* but not selected by the

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

20. 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

21. 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

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 treated municipalities and the untreated municipalities selected by the matching algorithm.

4.4. Validity of the research design

I assess whether the matching algorithm successfully selects municipalities with similar economic activity trajectories. Figure 3 examines the economic trends in the comparison between *closing*, *receiving*, and *unaffected* municipalities. It reports the dynamic coefficients from two separate regressions that are the equivalent of equation 1, but using the log of local GDP as an outcome variable.[22] One can appreciate how for both comparisons the coefficients in the years before the reform are not statistically different from 0. This suggests that the matching algorithm is successful and enables to compare groups of municipalities with similar trajectory of local economic activity. Moreover, it is reassuring that, while the algorithm seeks balance on the pre-reform trajectory only, there is no significant difference in the post-reform periods either. In figure A8, I repeat the same analysis using other proxies of economic activity to address concerns about the reform affecting the statistical office ability to measure local GDP. The top 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 panel, I use the log of the value of agricultural production as measured by the Brazilian statistical office. There is no evidence of differential trends among *closing*, *receiving*, and *unaffected* municipalities when using these other proxies for economic activity, though it is worth noting

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

that the estimates for agricultural production are quite noisy.

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.

5 — Main Findings

This section presents the main results on how centralization impacts tax revenues. I start by employing the matched difference-in-difference strategy to evaluate how *closing* and *receiving* municipalities are impacted and then discuss potential threats to identification and several sensitivity analysis.

5.1. The effects of centralization on tax revenues

I start by comparing the evolution of revenues between *closing* and the matched *unaffected* municipalities. Table 1 reports the estimated coefficients for equation 1. Column 1 reports the specification without any control. The average of the post-reform coefficients 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.[23] After the inclusion of these controls, the estimates are statistically significant at the 5% level. The average effect implies a 3.6% reduction in tax revenues.

23. Building on the existing literature, I use information from the 2010 census and code as formal the workers who report having a *work card*, paying social security contributions, or being employed in the public sector.

In the rest of the analysis I refer to the specification in column 2 as baseline specification.

I them compare the evolution of revenues between *receiving* and the matched *unaffected* municipalities. Table 1 reports the estimated coefficients for equation 1. Column 3 reports the specification without any control. The average of the post-reform coefficients 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 estimates are statistically significant at the 1% level. The average effect implies a 3.4% increase in tax revenues.

Figure 4 reports the coefficients and the 95% confidence interval from the baseline specification for two separate regressions, one reporting the coefficients for *closing* (red dots) and one for *receiving* (blue triangles). In both cases, 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. After the reform, the divergence is stark and evident already in the first period: *closing* municipalities experience a decline in revenues, while *receiving* municipalities experience an increase.

5.2. 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 figure 4 suggests that there is no differential pre-trends in tax revenues. 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 A4 show that the results are unaffected. Another concern is that the results might be driven by the exclusion of capitals and large cities. I include

also capitals and large cities, repeat the matching procedure, and conduct the analysis on this alternative matched sample. Columns 2 and 4 of table A4 show that the results are overall very similar. Additionally, one may be worried that results are driven by specific geographic regions. I thus estimate the baseline specification in different samples by dropping one-by-one each of the 137 mesoregions. For each regression, figures A9a and A9b report the average coefficients separately in the pre-reform and in the post-reform periods. The plot displays the treatments effects are remarkably stable across all the different estimation samples, both for *closing* and *receiving* municipalities.

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.[24] 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 because, as one can see from the dynamic coefficients in figure 4, the second period is when the effects seem more evident. Figure A10a 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 A10b 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, one 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 matched difference-in-differences approach, I also investigate the effects of the reform using a trajectory balancing design and a synthetic difference-in-differences design. The trajectory balancing design (Hazlett and Xu 2018) 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 evolution of the treatment effect on *closing* versus *unaffected* municipalities in figure A11a and the effects of the reform on

24. 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 of the treatment.

receiving versus *unaffected* municipalities in figure A11b. The effects are overall similar to the matched difference-in-differences results, but they exhibit a different dynamics. The estimate treatment effect for *closing* municipalities is a 2% reduction in tax revenues, statistically significant at the 5% 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 5% 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).

The synthetic difference-in-differences design (Arkhangelsky et al. 2021) builds a synthetic control by reweighing *unaffected* municipalities so that their pre-reform trends in tax revenues are approximately similar to the ones of the treated ones (*closing* or *receiving*).[25]. Also in this case the effects are overall similar to the matched difference-in-differences results. Figure A12a reports the evolution of tax revenues for *closing* and *synthetic unaffected* municipalities. The implied treatment effect is a 1.1% reduction in tax revenues, statistically significant at the 10% level (see first row of table A5). Figure A12b reports the evolution of tax revenues for *receiving* and *synthetic unaffected* municipalities. The implied treatment effect is a 2.2% increase in tax revenues, statistically significant at the 1% level (see second row of table A5).

Overall, the results in this section show that a more centralized structure leads to heterogeneous effects. *Closing* municipalities experience a reduction in tax collection after the reform. *Receiving* municipalities experience an increase in tax revenues. In section 7 I quantify the net impact on revenues by conducting the analysis at a more aggregate effect to capture all these differential effects. In the next section I turn instead to ask which mechanisms explain these differential effects.

25. This approach essentially allow controlling for flexible trends in socio-economic characteristics as in the baseline matched difference-in-difference design.

6 — Mechanisms

The previous section has documented the impact of the reform on tax revenues. 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. If this change involves reallocating resources from *closing* to *receiving* municipalities, it could account for both effects. In this section, I present evidence indicating that both explanations are at play and I conduct several empirical exercises to understand the underlying mechanisms.

6.1. Centralization hinders tax revenues in *closing* municipalities

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 A13 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 find that municipalities that are one hour farther from the new tax office experience a 1% larger reduction in 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.

Empirical strategy 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 $Distance^{i \to r}_{it}$. The instrument leverages the variation, at baseline, in distance between a municipality and the nearest other tax office; this distance becomes relevant only if and after the tax office serving a municipality closes. Figure A14 provides an illustration of how the instrument works. Each municipality (stylized towns in the figure) is part, at baseline, of a tax jurisdiction and is served by the corresponding office (colored dots in the figure). For each municipality, I compute the distance from the closest tax office in another tax jurisdiction but within the same fiscal region. $Distance^{i \to \bar{r}}_{i(-1)}$ (solid lines in the figure). After the reform, this distance becomes more relevant for municipalities served by closing offices (the crossed dots). Moreover, notice that for municipality D, the nearest other office would be located in a different fiscal region (see dashed line). Since municipalities cannot be assigned to an office in a different fiscal region, the instrument will not

consider that distance. However, cases like municipality D allows me to control for time-varying effects of distance from the nearest other office in *any* fiscal region. This can be interpreted as 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 from the tax office from other possible 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. $\text{Closing}_{i(m)}^{r}$ 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 closings. For instance, municipalities with declining tax potential could be assigned to an office farther away. Because it uses baseline distances, it also addresses the concern that municipalities tend to experience a larger increase in distance if many closings happen in a region (e.g., because a region has a declining tax potential).

First Stage The relevance of the instrument can be visualized in figure A15. 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 left 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 (red dots) and for those whose office remains open (blue and gray dots). However, the right 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 2 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 the relevance of the instrument, I employ it to estimate equation 2. Results are displayed in table 2. I report the coefficients from the baseline specification (column 2), the reduced form (column 3),[26] and the 2SLS (column 4). The 2SLS estimate is very similar to the potentially endogenous estimate of column 2.[27] 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 5. There is no evidence of differential pre-trends; this strengthens the validity of the exclusion restriction assumption.

If distance is the only factor driving the negative effects of the reform on tax revenues in *closing* municipalities, one would expect that municipalities that are near the new office do not experience a reduction in revenues. In order to test this, I examine the reduced form relationship between the instrument for distance and tax revenues in a more non-parametric way. In table A6, I consider various number of quantiles of the instrument, from 2 to 5.[28] 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.[29] While this effect is usually not statistically significant at the conventional levels, the magnitude and direction of the coefficients suggests that distance may not explain the whole effect and there may be other factors behind the decline in revenues as well. For instance, there may be frictions associated with switching office; additionally, centralized offices may decide to allocate fewer enforcement resources to these municipalities.

That said, I conduct several additional exercises to confirm that distance *does* matter. 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

26. Notice that the coefficient ι_1 is absorbed by municipality fixed effects.

27. Notice that the F-statistic on the first stage is very high, above the standard threshold.

28. 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*.

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

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 agents from the previous office differs across the new offices. Reduced forms and 2SLS estimates are reported in table A7. Notice that these specifications are very demanding because of the combination of numerous fixed effects and smaller sample. Yet, coefficients are always negative, large, and precisely estimated.

6.1.1. Distance and 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 a reason why the reform reduces revenues in *closing* municipalities is that it makes it harder to gather local information, one would expect a larger decline 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). This suggests that local information is less valuable in areas with stronger enforcement of third-party reporting. 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. Compli-

ance with all aspects of labor regulations is assessed. One such aspect is ensuring that workers are formal (Almeida and Carneiro 2012).[30] The presence of informal employees within formal firms is widespread (Ulyssea 2018). By enforcing workers' 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. For instance, information on newly registered workers enter the administrative records that are available to the government agencies, including the tax authority.

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 tax authority's reform 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 municipality in their mesoregion. With this measure at hand, I test for heterogeneity in the reduced form dynamic specification:

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

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 A8 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

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

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 6. The effect of distance on tax enforcement is driven by municipalities with weak enforcement of third-party reporting. 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. In figure A16, I repeat the analysis but classifying municipalities based on whether they received more or less inspections than the 75th percentile municipality in their mesoregion. I obtain very similar results: no differential pre-trends and divergence after the reform.

6.1.2. Distance and tax agents' visits

One way in which tax agents can collect this local information is by physically inspecting taxpayers' 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 3 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.[31] Municipalities farther from a local office receive less tax agents' visits. In figure 7 I report the dynamic coefficients for the Poisson specification: there is no evidence of pre-trends and the coefficients after the reform are negative (but not always statistically significant at the 5% level). In figure A17, I report the dynamic coefficients of the reduced form for the extensive and intensive margins separately. In neither case there is evidence of pre-trends. After the reform there are mixed effects on the extensive margin (top panel), but negative effects on the intensive margin (bottom panel).

Summing up, this section has shown that increased distance between municipalities and local

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

offices is a relevant channel explaining the negative effects on *closing* municipalities. The effects are stronger in areas where gathering local information is more valuable for tax enforcement. 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 visit *closing* municipalities and gather local information.

6.2. Allocation of Resources

6.2.1. Allocation of Enforcement Resources

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).

After the reform, expanded offices have a larger staff and can decide how to allocate staff effort over a wider region, which includes both *receiving* municipalities and the *closing* municipalities which merged in after the reform.

Offices should find it efficient to focus more staff effort on high-revenue potential areas. Even if the total number of tax agents in the region is unchanged, centralization may lead to a more efficient targeting of enforcement resources particularly in jurisdictions where revenue potential is concentrated in *receiving* municipalities.[32]

Empirical strategy If centralization enables the tax authority to focus staff effort on areas with higher-revenue potential, one would expect a larger increase in tax revenues for *receiving* municipalities in jurisdictions where *receiving* municipalities have higher revenue potential than the *closing* municipalities newly assigned to the same office. To test this hypothesis, for each post-reform jurisdiction I rank municipalities by a proxy of their revenue-potential, the average level of tax collection 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 that were served by the jurisdiction office before the reform. Tax jurisdictions for which this share is above the median are the ones for which revenue potential is more concentrated in *receiving* municipalities and there is more room for improved targeting. I then conduct a heterogeneity analysis using the following specification:

32. Figure A18 shows that there is a small and not statistically significant increase in tax agents per population after the reform in expanded offices.

$$y_{i(m)t} = \alpha_{i(m)} + \alpha_{mt} + \sum_{t=-4}^{2} \delta_t^I(\text{Receiving} \times \text{Improved targeting}_{i(m)} \cdot D_t) + \sum_{t=-4}^{2} \delta_t^N(\text{Receiving} \times \text{No improved targeting}_{i(m)} \cdot D_t)_{i(m)} \cdot D_t) + \sum_{t=-4}^{2} \eta_t(\text{Not-Matched}_{i(m)} \cdot D_t) + X_{i(m)t}\psi + \epsilon_{i(m)t} \quad (5)$$

where everything is like in equation 1, except that now I estimate the effects of being a *receiving* municipality separately for jurisdictions where there is room for improved targeting (*Improved targeting*) or not (*No improved targeting*). If centralization leads to a better allocation of enforcement resources, one would expect a larger positive effect on revenues for receiving municipalities in jurisdictions with room for imporved targeting ($\delta^I > \delta^N$).

Results Figure 8 reports the point estimates and the 95% confidence interval. In line with the hypothesis of better allocation of enforcement resources, the estimates suggest that the increase in tax revenues for *receiving* municipalities is driven by jurisdictions in which there is more room for improved targeting. 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 A19).

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).

6.2.2. Allocation of Managerial Resources

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.[33] After classifying high-quality managers using data on tax agents' careers, I conduct a heterogeneity analysis for the baseline difference-in-differences model. I find that the increase of tax revenues in *receiving* municipalities is stronger where high-quality managers oversee the expanded office.

Empirical strategy Building on Minni (2024), I construct a proxy for high-quality managers based on their promotion speed. In this context, promotions reflect how higher-level managers perceive a tax agent's success and are not solely based on seniority. I define high-quality 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 A20 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-quality status correlates with future personal success. While wage increases are limited in this context, being deployed to regional or central headquarters can be seen as a promotion. Table A9 shows that high-quality status is positively correlated with future deployment to the 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 (two years before and one year after). Twenty-nine out of 70 jurisdictions meet this criterion. I then conduct the following heterogeneity analysis:

33. This is a first step, as if higher-quality managers do not affect tax revenues, then a centralized structure cannot lead economies of scale arising from improved managerial quality. The next step to demonstrate that a centralized structure benefits from economies of scale due to managerial quality is to show that the tax authority retains only skilled managers and that average managerial quality increases. To test this, future research should (i) create a continuous measure of manager quality and (ii) include additional post-reform periods to capture potential improvements over time.

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

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 ($\delta^H > \delta^L$).

Results Figure 9 presents the coefficients from the dynamic specification. Note that the high-quality 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-quality managers see a small but precisely estimated increase in tax revenues. In contrast, the effect is much larger in municipalities managed by high-quality managers. When conducting the same heterogeneity but including also offices which change manager in the period 2018-2020 (figure A21), the results are noisier but the point estimates for municipalities managed by high-quality managers are larger. I then repeat the analysis but further exploring heterogeneity by whether *receiving* municipalities are in jurisdictions with more or less room for improved targeting of enforcement resources (as described in section 6.2.1). The results are reported in figure 8. In jurisdictions in which there is no room for improved targeting 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.[34] In jurisdictions with a low-quality managers, there is no change in tax revenues. On the other hand, in jurisdictions in which there is room for improved targeting, there are large and positive effects on tax revenues for *receiving* municipalities also when overseen by a low-quality manager. However, the effects are larger if the jurisdiction is overseen by a high-quality manager. Finally, in figure A23 I show that *closing* municipalities experience a larger decline in revenues if before the reform they were served by an office overseen by a high-quality

34. It should be pointed out that offices are classified as having a high-quality manager based on whom was in charge until the first period only.

manager. This further corroborates the hypothesis that talented manager can impact tax collection.

In summary, this section documents that giving a larger span of control to talented managers leads to tax collection gains. This provides further evidence on how a centralized structure may generate economies of scale by enabling a better utilization of managerial talent.

7 — 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 post-reform tax jurisdictions as the unit of analysis. Specifically, I compare the aggregate evolution of tax collection between jurisdictions (rather than municipalities) that became more centralized and those that did not. 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 controlling for regional shocks which may bias the results.

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

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

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.[35]

35. I include the same controls of the baseline specification

Results I report the results in figure 10. First, keeping in mind the caveats mentioned above, there is no evidence of differential pre-trends in tax revenues between centralized and not centralized jurisdictions. Second, 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 10% level. The average net effect is a 2.7% increase in tax revenues. This corresponds roughly to 301 millions Reais (2018 prices).

Moreover, policymakers may want to incorporate the savings on managerial costs originated by closing local offices. To quantify these savings, I estimate equation 7 but using the total managerial wages in a jurisdiction as dependent variable. Results are reported in figure A25 and imply an average reduction of 1.1 millions Reais (2018 prices).[36]

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 leverages a reform of the Brazilian tax authority as a natural experiment and documents how a more centralized structure impacts fiscal capacity. Tax revenues decline in areas previously served by an office that was closed by the reform, but increase in areas served by an office that expanded. Additionally, the paper sheds light on the mechanisms explaining these differences and provides evidence of a trade-off between gathering local information and a better allocation of resources across the territory. On the one hand, a centralized structure increases the distance between tax offices and the areas they need to monitor; this makes it harder to gather local information on taxable activity and reduces revenues. On the other hand, by concentrating tax agents and decision-making power over larger regions in fewer offices, a centralized structure allows the tax authority to allocate more staff effort toward high-revenue potential areas and to give talented managers a larger span of control.

Overall, centralization leads to an increase in revenues. By uncovering some forces underlying

36. I focus on expenditure for managerial wages because it is hard to attribute the post-reform decline in frontline tax agents' wages to centralization, given the presence of a declining trend before the reform (see figure A24).

the trade-off between a centralized or decentralized structure, this paper offers guidance to policymakers interested in implementing a similar reform in other contexts. Specifically, they should consider the severity of geographical frictions, how important is access to local information, the geographic distribution of revenue potential, and the heterogeneity in the quality of the available office managers. Additionally, they should consider whether their objective is only to maximize tax revenues or also to consider 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 to areas where is easier to evade taxes and lead to distortions in the allocation of economic activity (Fajgelbaum et al. 2019; Dix-Carneiro et al. 2021). Moreover, regional inequality in tax enforcement could have downstream consequences on political accountability and participation (Weigel 2020). Future work should provide empirical evidence on this.

More broadly, this paper shows that the effectiveness of a bureaucracy 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 (Balboni et al. 2023; Assunção et al. 2023) and with delivering social assistance programs (Muralidharan, Niehaus, and Sukhtankar 2023; Banerjee et al. 2024). Investigating how the effect of different organizational choices is shaped by the available monitoring and enforcement technologies is an interesting area for future research.

9 — Tables

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 an office that was closed 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 2: Distance from the local office and tax revenues

	Distance office	Tot. Tax (log)		
	(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 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

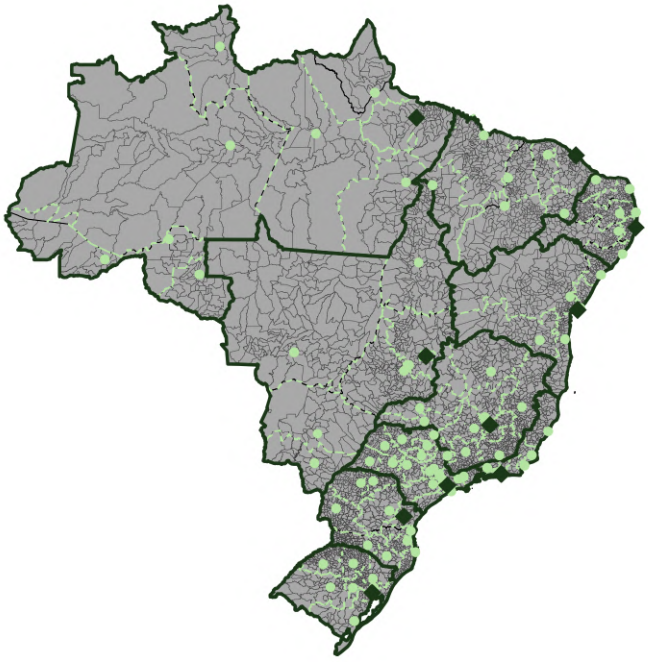
Table 3: Distance from the local office and tax agents' visits to municipalities

	Any trip			N. trips (log)			N. trips	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	Poisson	Poisson
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. Columns 4, 5, and 6 include only observations with at least one trip. *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 *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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

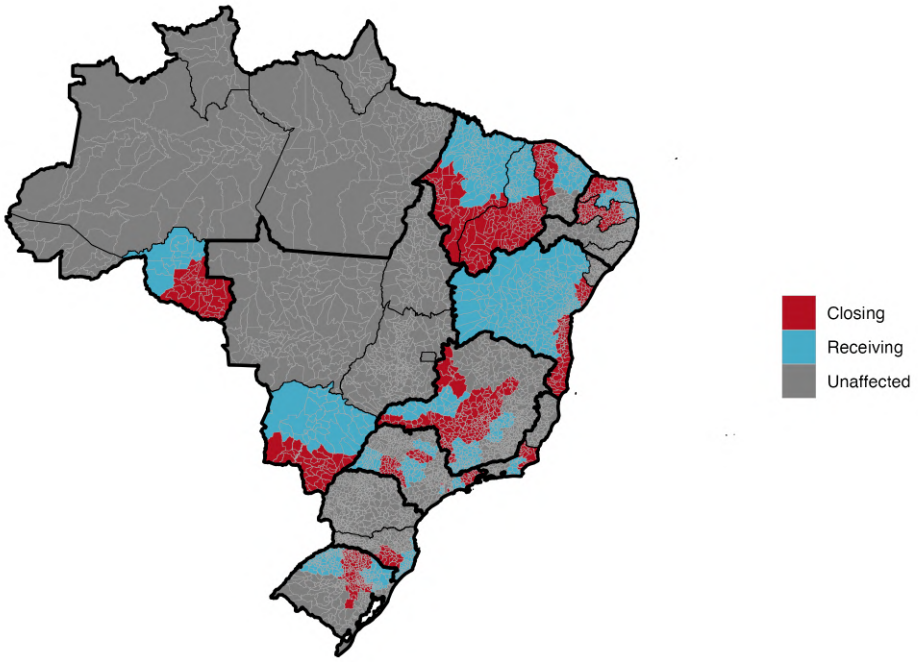
10 — Figures

Figure 1 — Map of the tax authority regional units



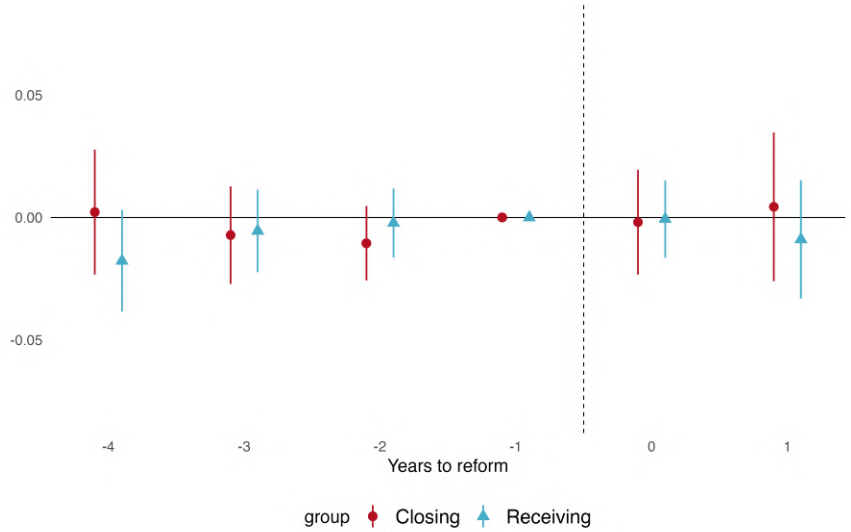
Notes. This map displays the 10 fiscal regions boundaries (dark green lines), regional headquarters (dark green diamonds), tax jurisdictions (light green dashed lines) and local offices (light green dots). Thin black lines represent states borders. Thin white lines represent municipalities borders. Tax jurisdictions and local offices are as of 2019 (pre-reform).

Figure 2 — Map of municipalities by group



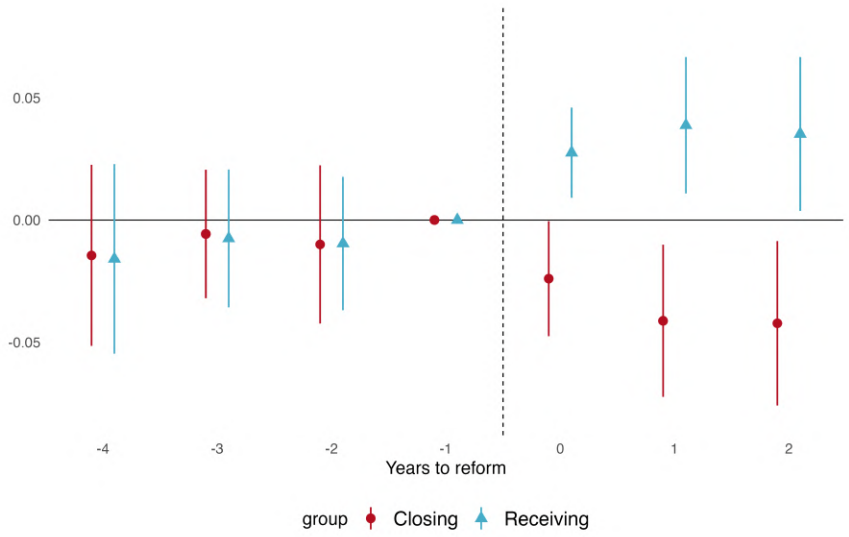
Notes. This map displays how each municipality is affected by the reform. Thick black lines represent the ten fiscal regions borders. Thin black lines represent states borders. Thin white lines represent municipalities borders. *Closing* municipalities are the ones whose jurisdiction tax office closes 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 and do not absorb municipalities from the closed offices.

Figure 3 — The matching procedure is effective at selecting municipalities on parallel trends in economic activity



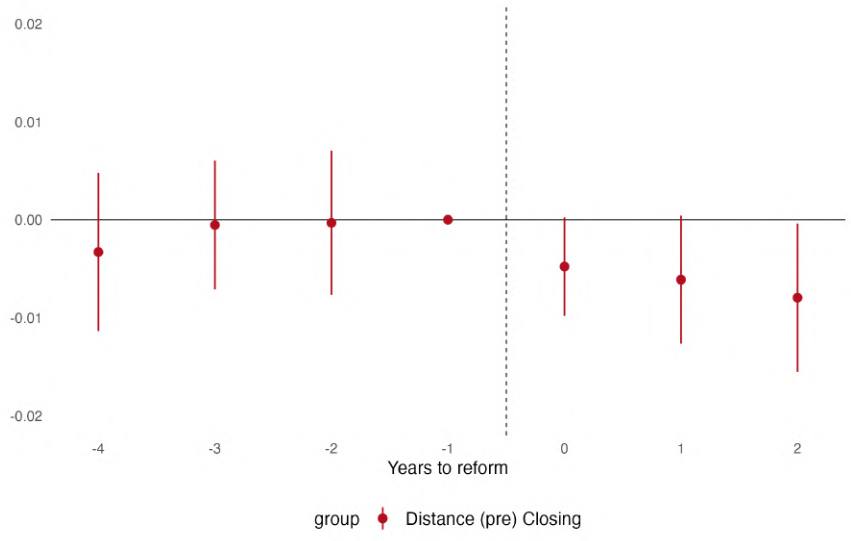
Notes. Observations are at the municipality-year level. The dependent variable is the log of local GDP (available up to 2021). The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (red dots) or *receiving* (blue triangles). The coefficients are from two separate regressions. 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 an office closed in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. For the regression estimating the *Closing* coefficients, an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. For the regression estimating the *Receiving* coefficients, 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.

Figure 4 — The effects of the reform on tax revenues: dynamic coefficients



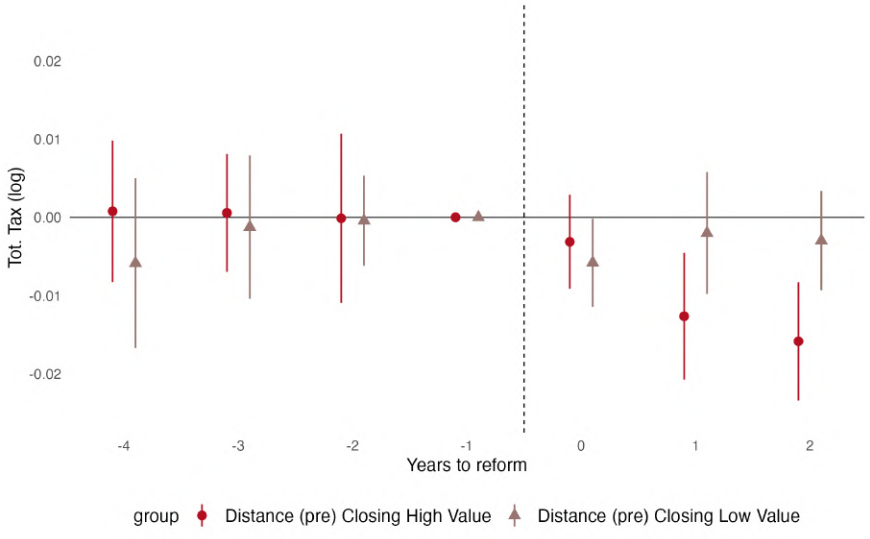
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (red dots) or *receiving* (blue triangles). The coefficients are from two separate regressions. 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 an office closed 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. For the regression estimating the *Closing* coefficients, an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. For the regression estimating the *Receiving* coefficients, 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.

Figure 5 — Distance from the local office and tax revenues: dynamic specification



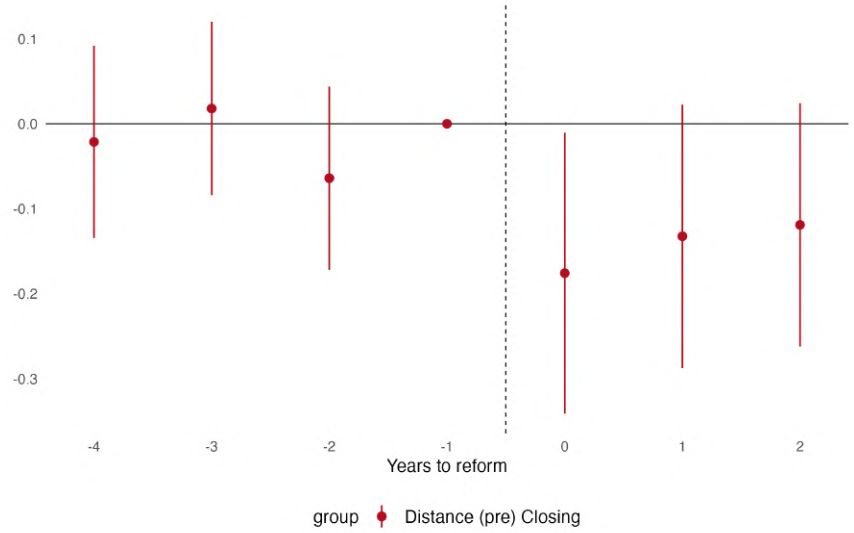
Notes. Dynamic version of column 3 of table 2. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *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.

Figure 6 — Distance from the local office, local information, and tax revenues



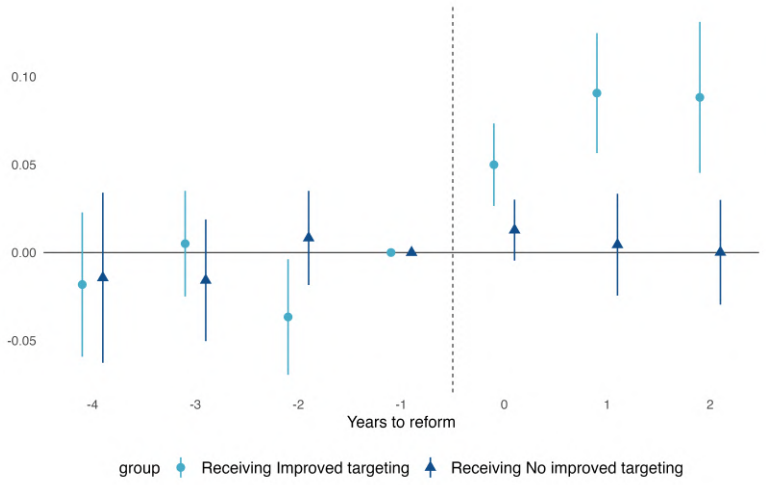
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *Closing* separately for municipalities with high (red dots) and low (red triangles) number of labor inspections. Local information is more valuable in municipalities with a low number of labor inspections. *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. *High Lab. Insp.*: 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. *Low Lab. Insp.*: indicator equal to 1 for municipalities below the 50th 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. *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 7 — Distance from the local office and tax agents' visits



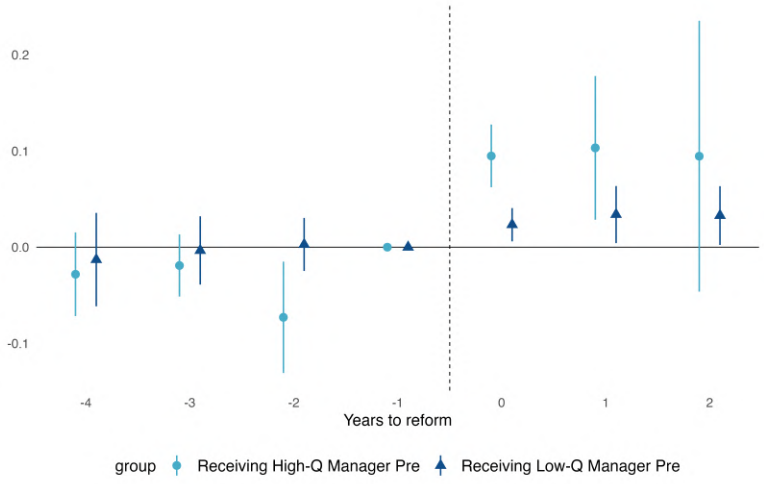
Notes. Observations are at the municipality-year level. The dependent variable is the number of tax agents trips from a local office visited a municipality in a year. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* $\times$ *closing*. The coefficient for $t = -1$, the year before the reform, is normalized to 0. The coefficients are from a Poisson specification. *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 8 — Allocation of enforcement resources and tax revenues



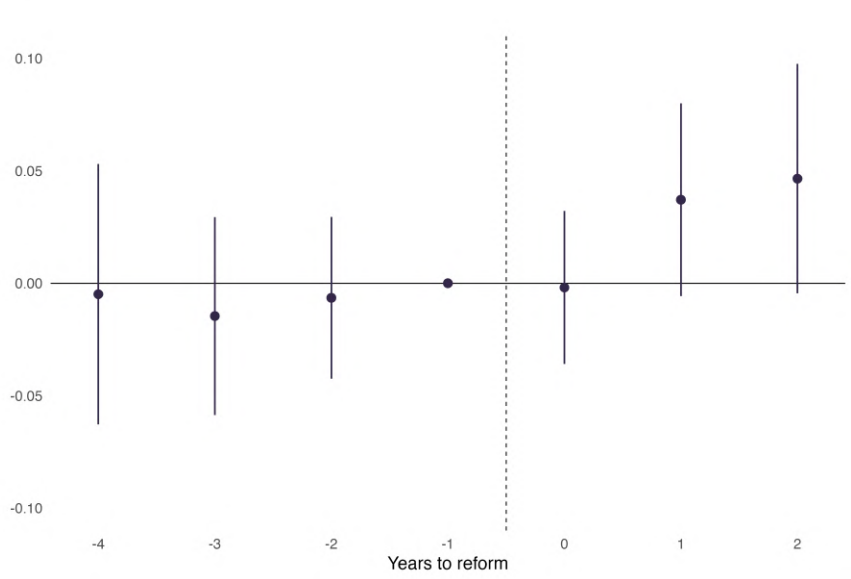
Notes. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. This plot reports point estimates and 95% confidence intervals for the interaction between year and *Receiving* separately for municipalities in jurisdictions with room for improved targeting (blue dots) and with no room for improved targeting (blue triangles). *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office closed in 2020. *Improved targeting* is an indicator equal to 1 if in a jurisdiction revenue potential is concentrated in *receiving* municipalities. *No improved targeting* 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 9 — Allocation of managerial resources and tax revenues



Notes. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. This plot reports point estimates and 95% confidence intervals for the interaction between year and *Receiving* separately for municipalities in jurisdictions overseen by a high-quality (blue dots) and low-quality (blue triangles) manager. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office closed in 2020. *High-quality manager pre* 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 A20. *Low-quality manager pre* 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 above to the 25th percentile in the distribution for figure A20. *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 10 — The net effect of centralization on tax revenues: aggregate level



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

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A — Appendix

A.1. Additional tables

Table A1: Characteristics *closing* municipalities

	Unaffected (N=2741)		Closing (N=1197)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Agric. share (2010)	0.31	0.18	0.35	0.18	0.04***	0.01
Second. share (2010)	0.02	0.03	0.02	0.04	0.00***	0.00
Services share (2010)	0.04	0.02	0.03	0.02	0.00	0.00
Formal share (2010)	0.47	0.18	0.43	0.19	-0.04***	0.01
Population (2010)	26555.62	56560.95	21328.74	48689.71	-5226.88***	1774.17
Area (log)	6.32	1.38	6.13	1.11	-0.19***	0.04
Nightlights	1230.01	2826.37	963.05	2483.67	-266.96***	89.82
GDP	768070.65	2363338.38	632552.90	2603491.27	-135517.76	87751.59
Agric. Prod.	29150.12	71389.91	20196.64	47710.42	-8953.48***	1939.34
Distance tax office (hours)	4.43	17.73	2.27	1.66	-2.16***	0.34
Distance other tax office (hours)	5.68	17.98	3.21	1.91	-2.48***	0.35
All tax	85622332.62	633319221.54	81041432.64	753873185.41	-4580899.98	24922318.27

Notes. Observations are at the municipality level. The table reports mean and standard deviation for *closing* and *unaffected* municipalities, as well as the differences between the means in the two groups. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A2: Characteristics *receiving* municipalities

	Unaffected (N=2741)		Receiving (N=1457)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Agric. share (2010)	0.31	0.18	0.35	0.19	0.04***	0.01
Second. share (2010)	0.02	0.03	0.03	0.05	0.01***	0.00
Services share (2010)	0.04	0.02	0.03	0.02	0.00***	0.00
Formal share (2010)	0.47	0.18	0.42	0.20	-0.05***	0.01
Population (2010)	26555.62	56560.95	28032.81	61776.30	1477.19	1945.88
Area (log)	6.32	1.38	6.09	1.17	-0.22***	0.04
Nightlights	1230.01	2826.37	1217.42	2787.44	-12.59	90.81
GDP	768070.65	2363338.38	757587.13	3132793.50	-10483.52	93668.25
Agric. Prod.	29150.12	71389.91	18312.67	46166.47	-10837.45***	1822.69
Distance tax office (hours)	4.43	17.73	2.58	1.96	-1.85***	0.34
Distance other tax office (hours)	5.68	17.98	3.22	1.82	-2.46***	0.35
All tax	85622332.62	633319221.54	105890218.02	1021242705.63	20267885.40	29362254.90

Notes. Observations are at the municipality level. The table reports mean and standard deviation for *receiving* and *unaffected* municipalities, as well as the differences between the means in the two groups. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A3: Characteristics predicting the likelihood that an office is affected by the reform

	Closing		Expanded	
	(1)	(2)	(3)	(4)
Constant	2.101***	0.593***	-0.456	0.170**
	(0.239)	(0.070)	(0.316)	(0.058)
Distance other office	-0.039***	-0.035**	-0.021	-0.022*
	(0.010)	(0.011)	(0.012)	(0.011)
Distance from reg. HQ (hrs)	0.001	0.000	0.003*	0.004**
	(0.001)	(0.001)	(0.002)	(0.001)
Size Staff	-0.376***		0.162**	
	(0.044)		(0.070)	
Office Size > Med.		-0.477***		0.245**
		(0.061)		(0.083)
Dep. Var. Mean	0.26	0.26	0.26	0.26
R^2	0.34	0.29	0.09	0.10
Clusters	10	10	10	10
Observations	94	94	94	94

Notes. Observations are at the office level. The dependent variable is an indicator equal to 1 if an office is closed (columns 1 and 2) or expanded (columns 3 and 4). *Distance other office:* distance (in hours) from the closest other local offices in the fiscal region. *Distance from reg. HQ:* distance (in hours) from the regional headquarter of the fiscal region. *Size Staff:* number of tax agents deployed in the office in 2016. *Office Size > Med:* indicator equal to 1 if the number of tax agents deployed in the office in 2016 is above the national median. Standard errors are clustered at the fiscal region level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A4: Effects of the reform on tax revenues: additional robustness

	Tot. Tax (log)			
	Closing		Receiving	
	(1)	(2)	(3)	(4)
Treatment × Period = -4	-0.012	-0.009	-0.016	-0.011
	(0.019)	(0.016)	(0.020)	(0.016)
Treatment × Period = -3	-0.006	-0.010	-0.007	-0.022*
	(0.013)	(0.011)	(0.014)	(0.012)
Treatment × Period = -2	-0.010	-0.004	-0.010	-0.010
	(0.016)	(0.013)	(0.014)	(0.012)
Treatment × Period = 0	-0.025**	-0.028***	0.028***	0.023**
	(0.012)	(0.010)	(0.009)	(0.009)
Treatment × Period = 1	-0.040**	-0.037**	0.038***	0.031**
	(0.016)	(0.016)	(0.014)	(0.013)
Treatment × Period = 2	-0.043**	-0.032*	0.035**	0.030**
	(0.017)	(0.018)	(0.016)	(0.015)
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, but control for time-varying economic activity. Columns 2 and 4 include capitals and large cities. 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 an office closed 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A5: Effects of the reform on tax revenues: synthetic difference-in-differences

	Point estimate	95% CI
Closing	-0.011*	[-0.024, 0.002]
Receiving	0.022***	[0.010, 0.035]

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 row 1, and being a *receiving* municipality in row 2. *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 an office closed in 2020. In each comparison the control group is the synthetic control built using the approach in Arkhangelsky et al. (2021). *Fixed effects:* municipality and 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. Confidence interval are constructed using bootstrap standard errors (with 1000 replications). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A6: Distance from the local office and 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 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 A7: Distance from the local office and tax revenues: additional specifications

	Tot. Tax (log)					
	(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 *closing* municipalities only. Odd columns report reduced form estimates, even columns report 2SLS estimates. Columns 3 and 4 control for the post-reform jurisdiction interacted with year fixed effects. Columns 5 and 6 control for the pre-reform jurisdiction and post-reform jurisdiction pair, interacted with year fixed effects. *Tot. Tax (log):* total tax collection in a municipality in a year (federal taxes only, 2018 constant prices). *Distance office:* Distance (in hours of travel) between a municipality and the jurisdiction tax office in a year. *Distance other office (pre):* Distance (in hours of travel) between a municipality and the closest tax office in the same fiscal region but in a different tax jurisdiction, in the year before the tax offices closings. *Closing Post:* Indicator which takes value 1 for municipalities whose jurisdiction tax office closes in the period after the closings (2020). *Fixed effects:* municipality and mesoregion-by-year fixed effects; 2020 jurisdiction-by-year (columns 3 and 4) fixed effects; 2019 jurisdiction-by-2020 jurisdiction-by-year fixed effects (columns 5 and 6).*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 A8: Characteristics municipalities where local information is more valuable

	0		1			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Agric. share (2010)	0.38	0.17	0.29	0.18	-0.09***	0.00
Second. share (2010)	0.02	0.03	0.02	0.04	0.01***	0.00
Services share (2010)	0.03	0.01	0.04	0.02	0.01***	0.00
Formal share (2010)	0.44	0.18	0.46	0.19	0.03***	0.01
Population (2010)	8.80	0.80	9.82	1.10	1.02***	0.03
Area (log)	5.92	1.16	6.43	1.31	0.52***	0.03
Nightlights (2016)	5.38	0.95	6.49	1.36	1.11***	0.03
GDP (2016)	11.60	0.86	12.69	1.45	1.09***	0.03
Agric. Prod. (2016)	8.35	2.04	8.40	2.56	0.06	0.06
Distance other tax office (hours)	3.61	3.03	5.10	16.82	1.49***	0.31

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. Local information is more valuable in municipalities where *High Labor Inspection* equal to 0. *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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

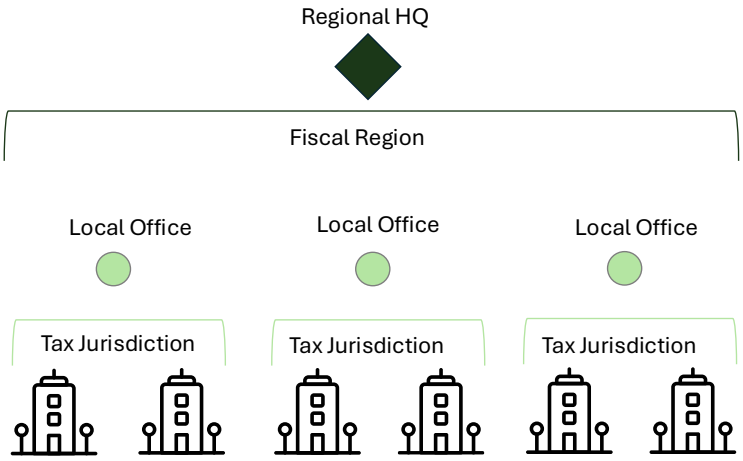
Table A9: High-quality managers: validation

	Achieve Central HQ	Achieve Regional HQ
	(1)	(2)
High-quality 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 deployed to a regional or to the central headquarter at any point in time up to 2022. *High-quality* 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 A20. The sample excludes all the managers who will be overseeing a local office in the period 2016-2022. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

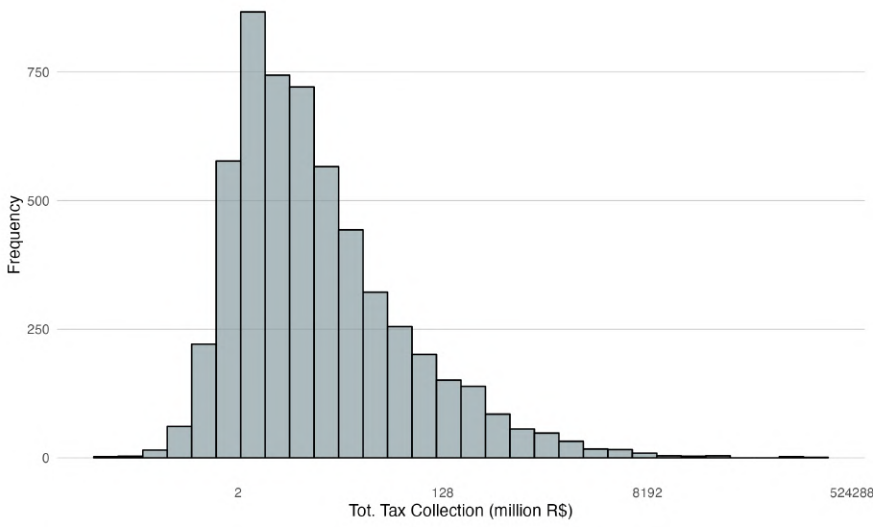
A.2. Additional figures

Figure A1 — Schematic representation of the regional structure of the tax authority



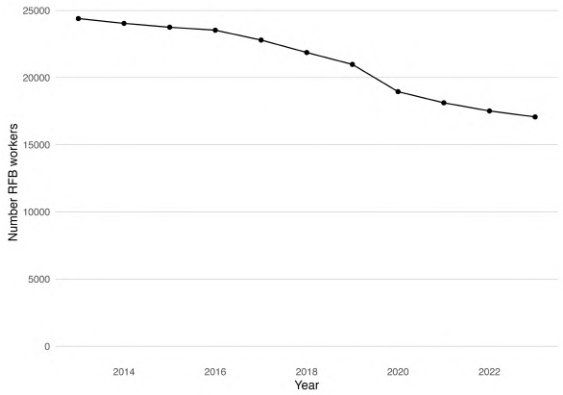
Notes. This scheme illustrates the hierarchy of the regional units of the Brazilian tax authority. There are 10 fiscal regions. Each region is managed by a regional headquarter (dark green diamonds). Each fiscal region is further partitioned in tax jurisdiction. There are 94 tax jurisdictions in 2019.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

Figure A2 — Federal tax collection in 2016 by municipality

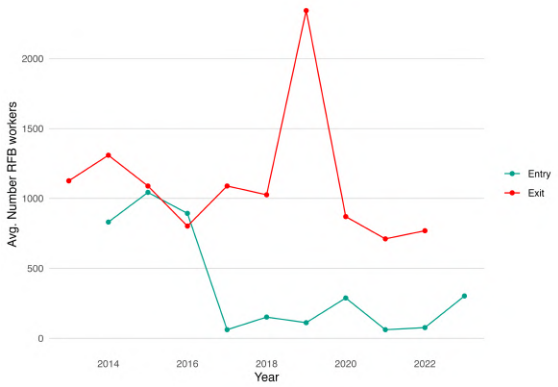


Notes. Federal tax collection (in millions of 2018 R$) in each of the 5,565 municipalities in 2016. As tax collection is highly skewed, the plot reports the log of tax collection (the horizontal axis reports the non-logged values).

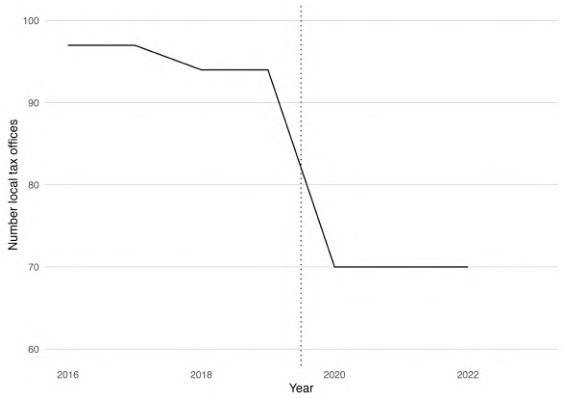
Figure A3 — The evolution of the tax authority



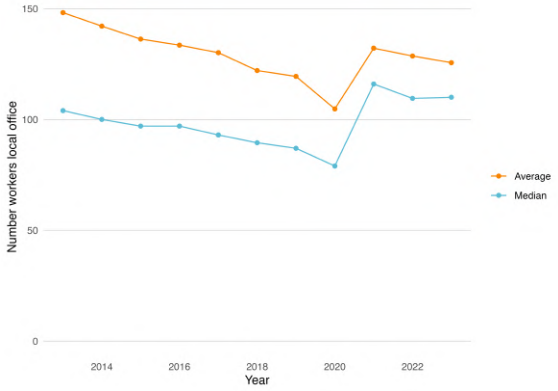
(a) Number of tax agents



(b) Number of tax agents entering and leaving



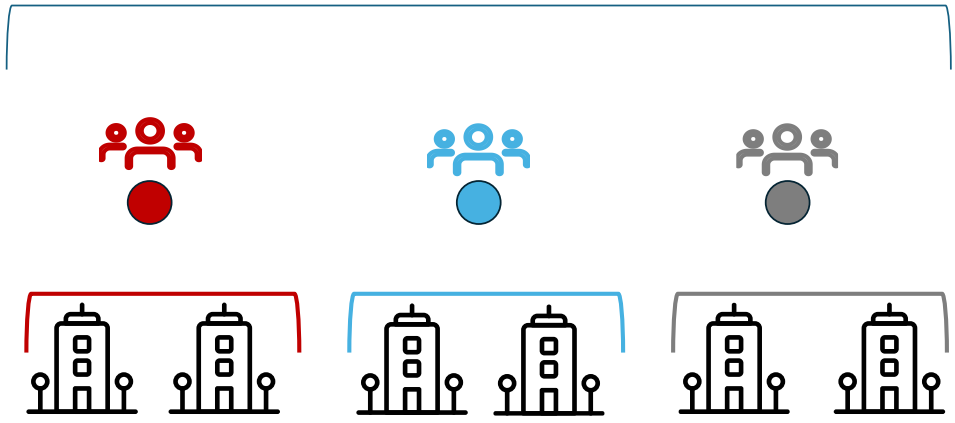
(c) Number of local tax offices



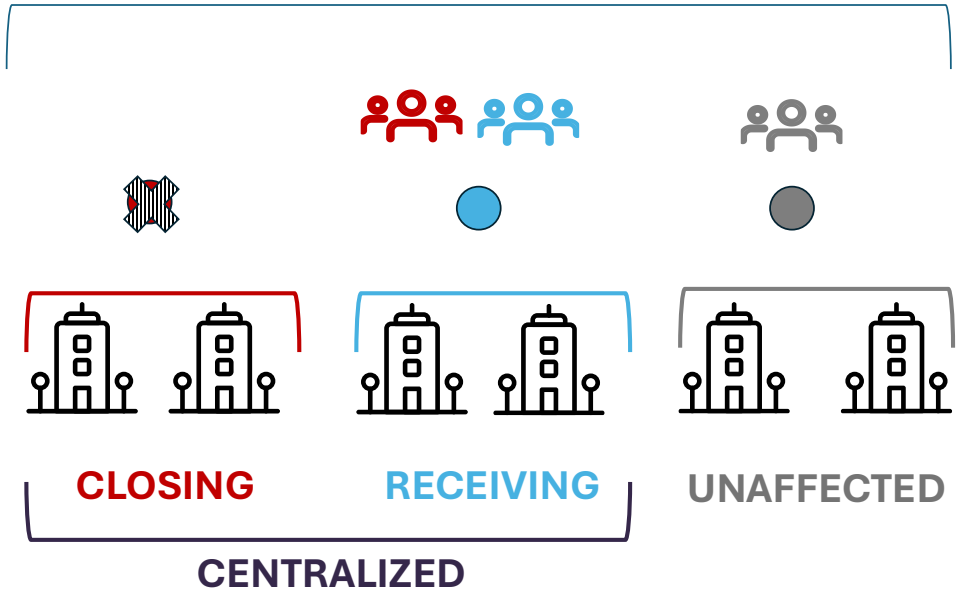
(d) Size of local tax offices

Notes. These graphs describe the evolution of the tax authority over time. The number of tax agents decline over time (top-left panel). Top right panel displays that this is due to retirements (red line) and a freezing in new hiring (green line). The 2020 reform closes 24 local offices (bottom-left panel). The bottom-right panel displays that after these closings, the size of local offices bounces back up after a few years of decline.

Figure A4 — Illustration of how the reform affects different municipalities



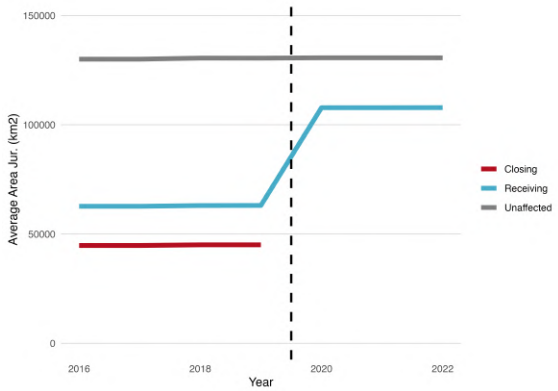
(a) Baseline



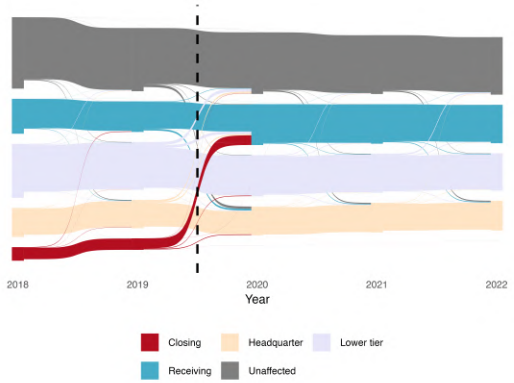
(b) Post reform

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. *Closing* and *receiving* municipalities are part of *centralized* jurisdictions.

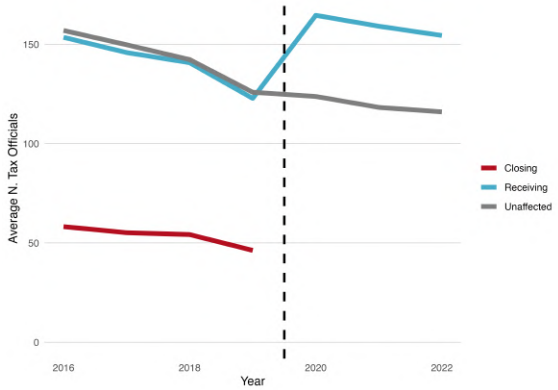
Figure A5 — The reform



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



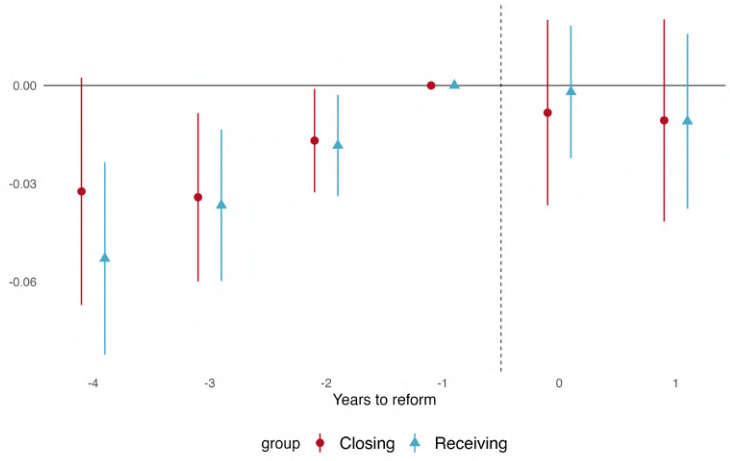
(b) Most tax agents from closing offices reallocated to a *receiving* office



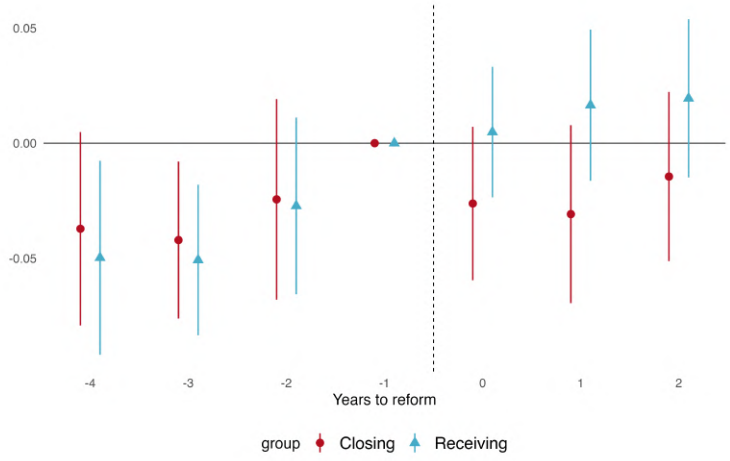
(c) Reallocation of tax agents increases personnel size in *receiving* offices

Notes. These graphs illustrate the reform. Municipalities are assigned to the jurisdiction of the expanded offices (*receiving*). Tax agents from closing offices are assigned to the expanded offices (*receiving*), increasing their size.

Figure A6 — Closing, receiving and unaffected municipalities are on differential trends in economic activity and tax collection



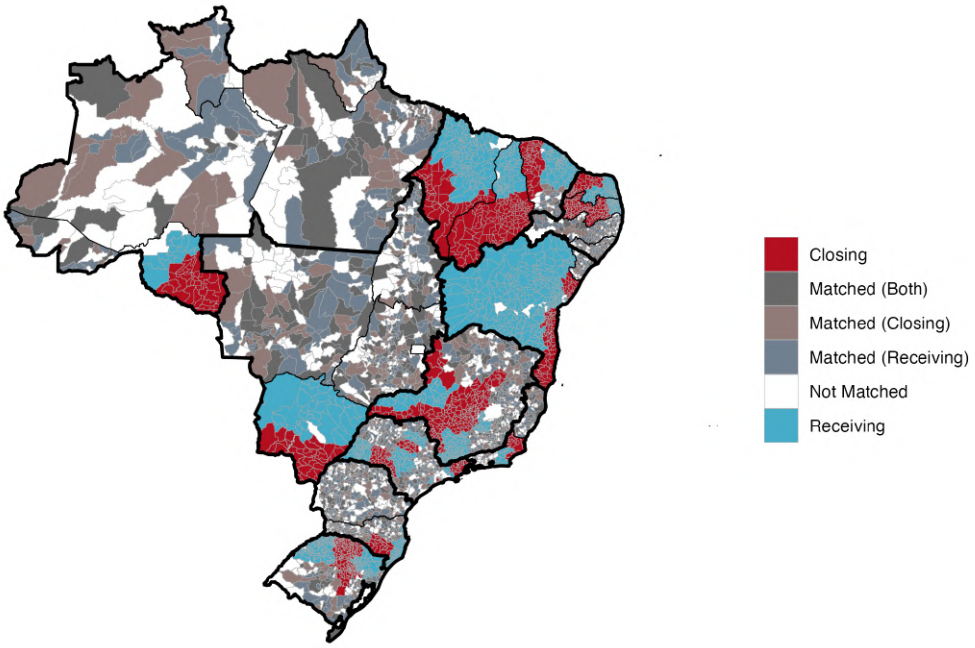
(a) Local GDP



(b) Federal tax collection

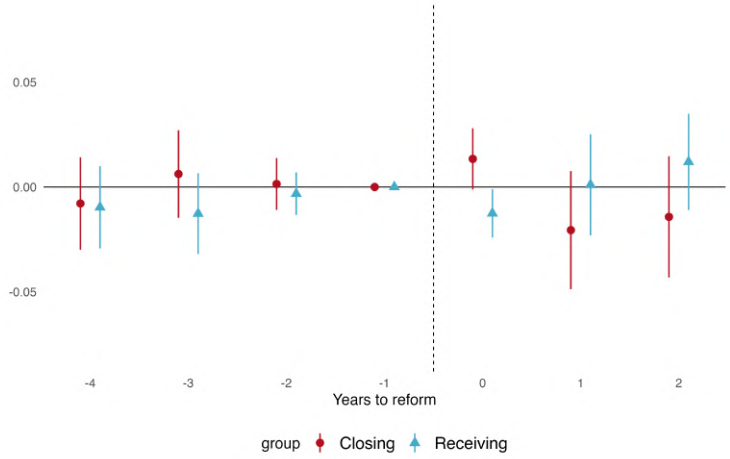
Notes. Observations are at the municipality-year level. The dependent variable is the log of local GDP (top panel) and federal tax collection (bottom panel). The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (red dots) or *receiving* (blue triangles). The coefficients are from two separate regressions. 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 an office closed in 2020. The regression for the bottom panel included 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. *Fixed effects:* municipality and mesoregion-by-year fixed effects. For the regression estimating the *Closing* coefficients, an indicator for *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. For the regression estimating the *Receiving* coefficients, 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 A7 — Map of municipalities by group, matched sample

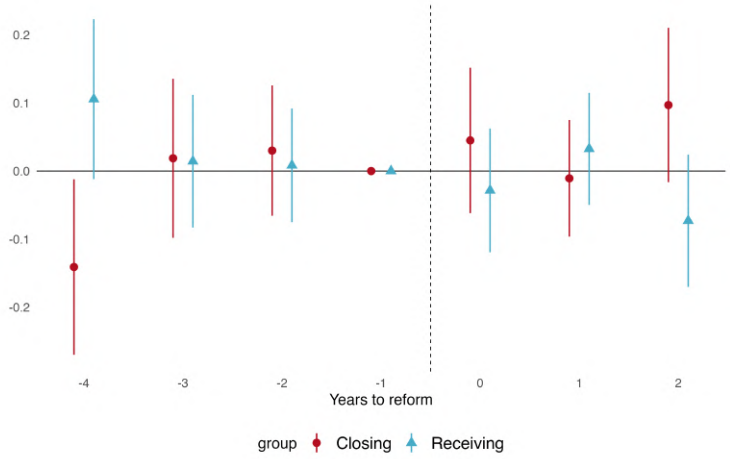


Notes. This map displays how each municipality is affected by the reform. Thick black lines represent the ten fiscal regions borders. Thin black lines represent states borders. Thin white lines represent municipalities borders. *Closing* municipalities are the ones whose jurisdiction tax office closes in 2020. *Receiving* municipalities are the ones whose tax offices absorbs municipalities and tax agents from the closing offices. The map also displays *unaffected* municipalities based on whether they are selected by the matching algorithm and for which comparison. *Unaffected* municipalities are the ones whose tax offices do not close nor absorb municipalities and tax agents from the closed offices.

Figure A8 — The matching procedure is effective at selecting municipalities on parallel trends in economic activity: alternative proxies



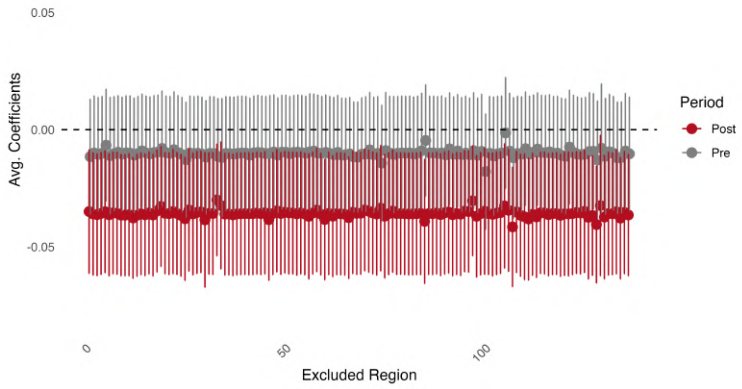
(a) Nightlights



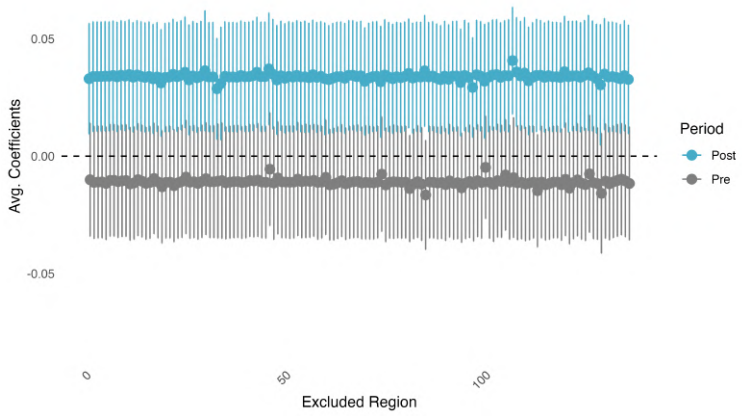
(b) Agricultural production

Notes. Observations are at the municipality-year level. The dependent variable is nightlights (log) in top panel and the value of agricultural production (log) in the bottom panel. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *closing* (red dots) or *receiving* (blue triangles). The coefficients are from two separate regressions. 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 an office closed in 2020. *Fixed effects:* municipality and mesoregion-by-year fixed effects. For the regression estimating the *Closing* coefficients, an indicator for not-matched and *receiving* municipalities interacted with year fixed effects ensures that they are not part of the control group. For the regression estimating the *Receiving* coefficients, 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.

Figure A9 — The effects of the reform on tax revenues: dropping one region at the time



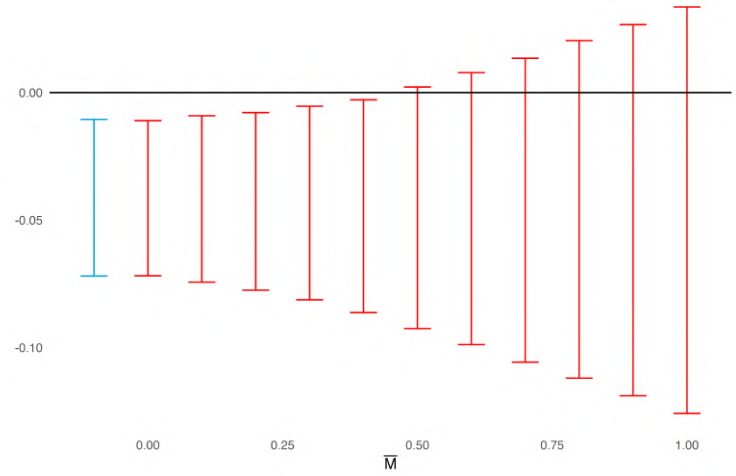
(a) Closing vs Unaffected municipalities



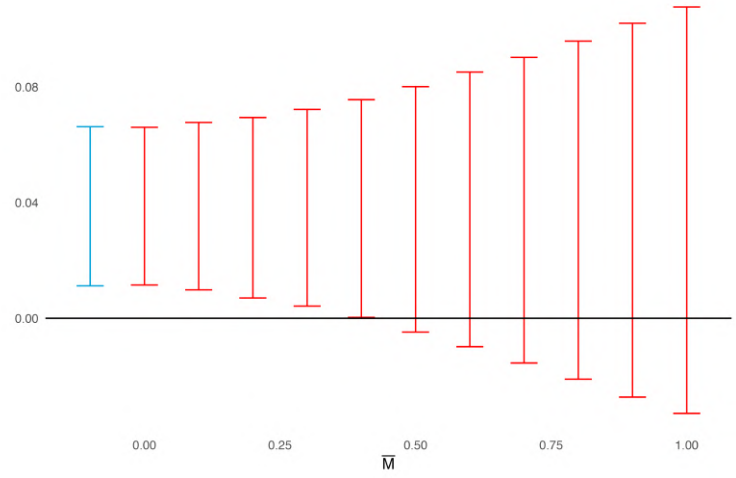
(b) Receiving vs Unaffected municipalities

Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the average and 95% confidence interval for the interaction between year treatment status separately for the pre-reform periods (gray) and for the post-reform periods (red or blue). In the top panel the treatment is being a *closing* municipality. In the bottom panel the treatment is being a *receiving* municipality. Each pair of coefficients come from a regression dropping sequentially one of the 137 mesoregions.

Figure A10 — The effects of the reform on tax revenues: robust confidence sets



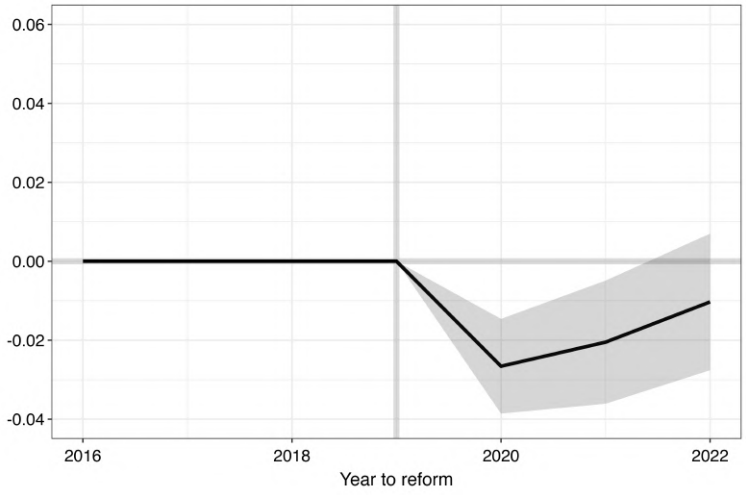
(a) Closing vs Unaffected municipalities



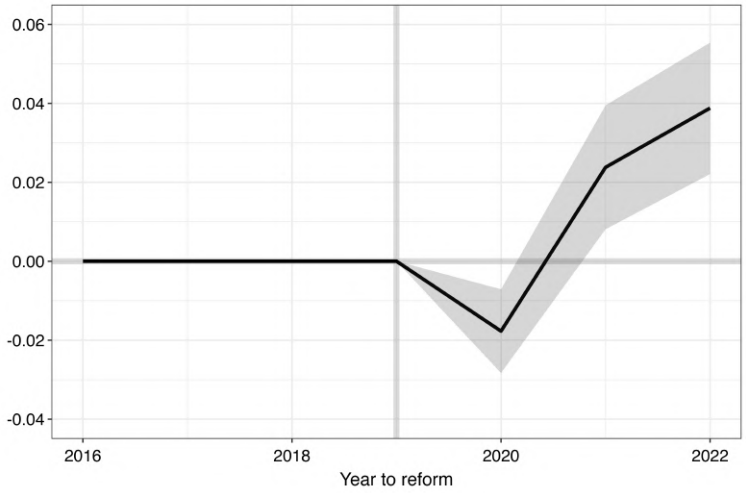
(b) Receiving vs Unaffected municipalities

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 bounds, built using the approach in Rambachan and Roth (2023). The specifications are equivalent to the regressions described in figure 4

Figure A11 — The effects of the reform on tax revenues: trajectory balancing approach



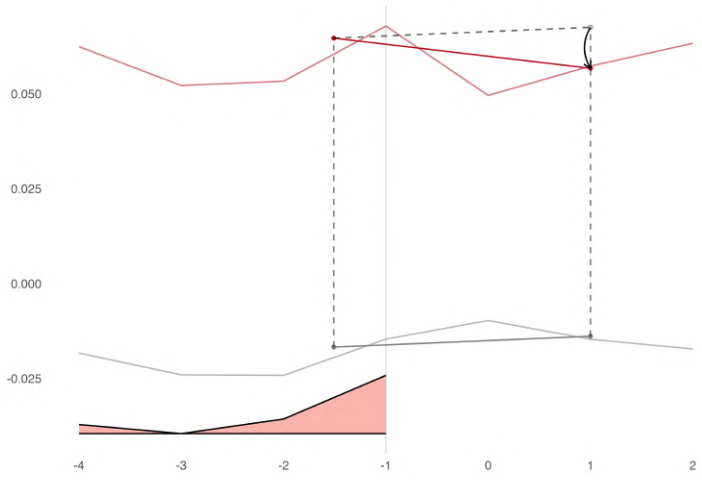
(a) Closing vs Unaffected municipalities



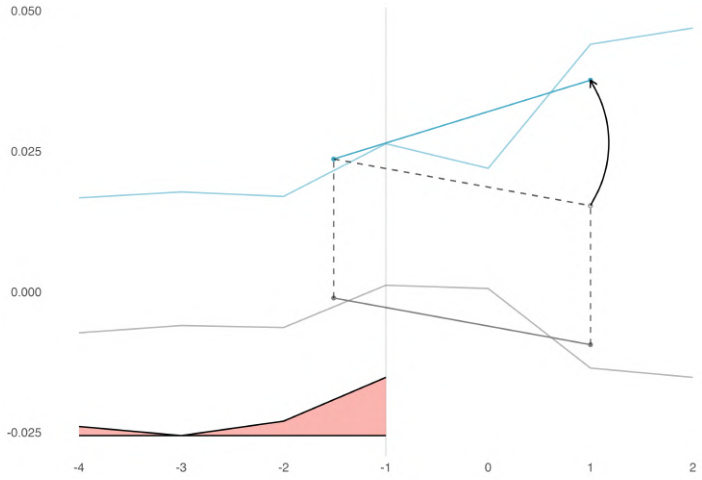
(b) Receiving vs Unaffected municipalities

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 an office closed 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 A12 — The effects of the reform on tax revenues: synthetic difference-in-differences approach



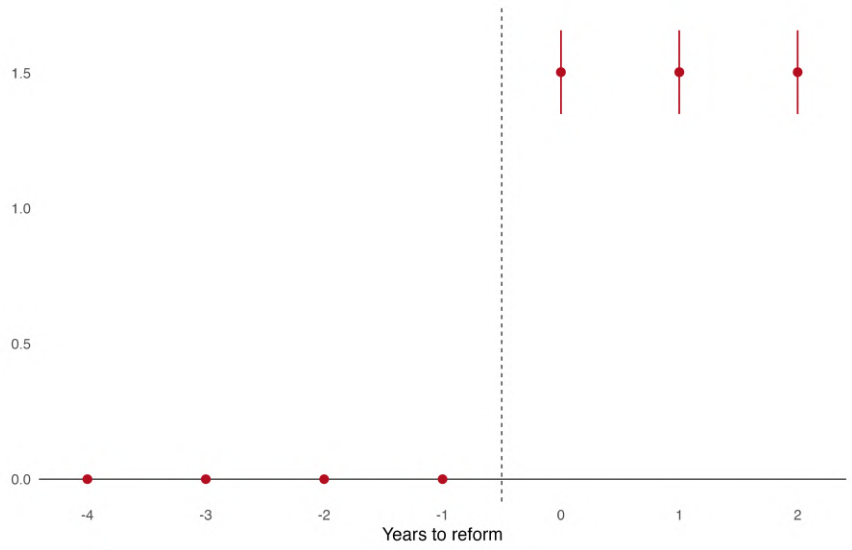
(a) Closing vs Unaffected municipalities



(b) Receiving vs Unaffected municipalities

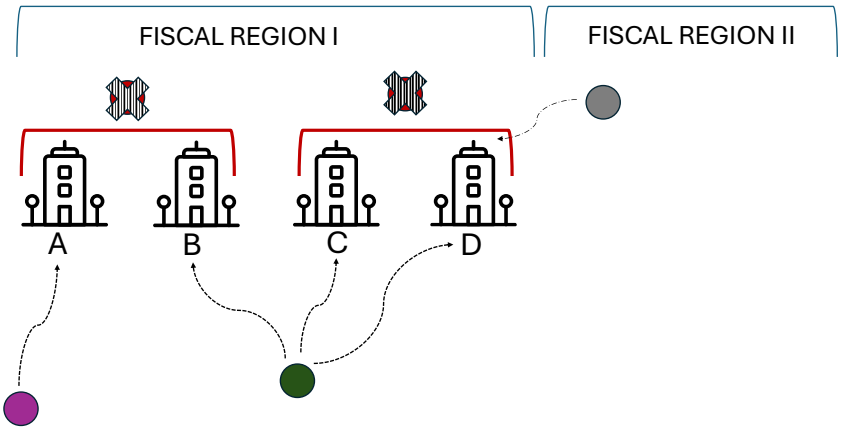
Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection (residualized on the baseline controls). The top panel reports the evolution of tax revenues between *closing* (red line) and *synthetic unaffected* (gray line) municipalities. The bottom panel reports the evolution of tax revenues between *receiving* (blue line) and *synthetic unaffected* (gray line) municipalities. In each comparison, the *synthetic unaffected* municipalities are built using the approach in Arkhangelsky et al. (2021). The colored dots represent the pre-reform and the post-reform average. The empty dots represent the counterfactual average for the treated group if the reform had not happened. The black arrow represents the implied average effect. The relative size of the time weights for pre-treatment periods are shown on the bottom (see Arkhangelsky et al. (2021) for more details).*Fixed effects:* municipality and 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. Confidence interval are constructed using bootstrap standard errors (with 1000 replications).

Figure A13 — Distance between municipalities and local offices increases after the reform



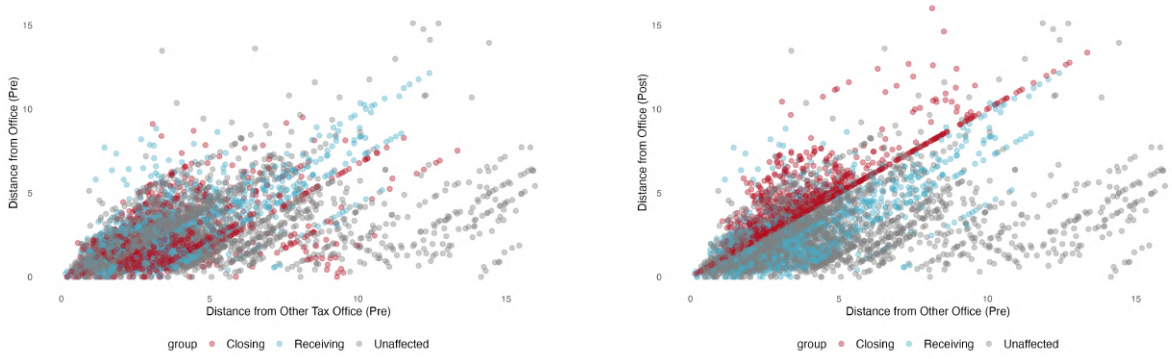
Notes. Observations are at the municipality-year level. The dependent variable is the distance (in travel hours) between a municipality and the jurisdiction tax office. The plot reports estimated coefficients and 95% confidence interval for the interaction between *Closing* and indicator for years. *Closing*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

Figure A14 — Illustration of the instrumental variable for distance



Notes. Schematic illustration of the variation underlying the instrumental variable approach for the relationship between distance and tax revenues. The illustration displays two fiscal regions, five local offices (dots), and four municipalities (stylized towns). The four municipalities are served by an office that is closed by the reform. For each municipality the instrument leverages the distance (solid arrow) from the closest office in a different tax jurisdiction but in the same fiscal region. This distance becomes more relevant if the jurisdiction tax office closes. For instance, municipalities A and B are served at baseline by the same office, but the closest other office is different and at different distances. Notice that for municipality D the closest other office is in a different fiscal region (gray dot). As municipalities cannot be assigned to offices in other fiscal regions, the actual distance picked up by the instrument is the one from the closest office in the same fiscal region (green dot). The distance from the gray office (dotted line) is used to proxy market access, as fiscal regions border to not obstacle goods' trade.

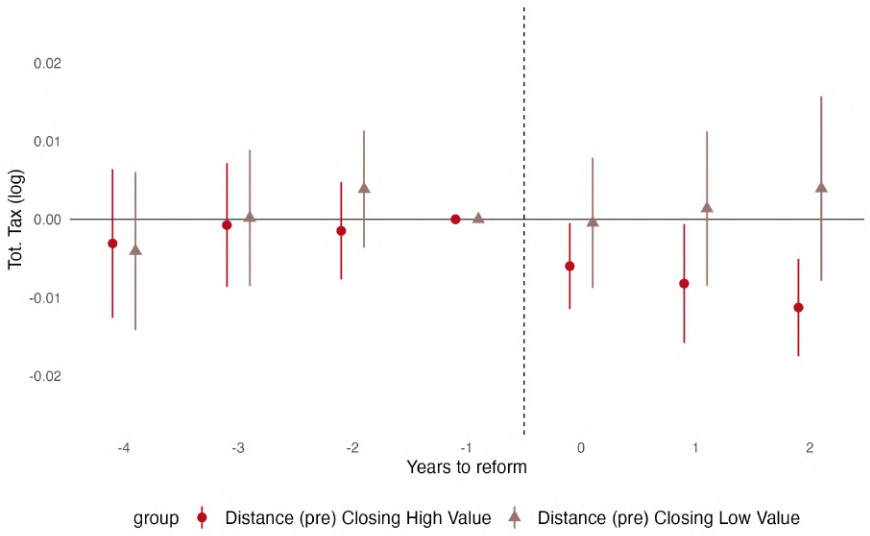
Figure A15 — Instrumental variable for distance: description



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

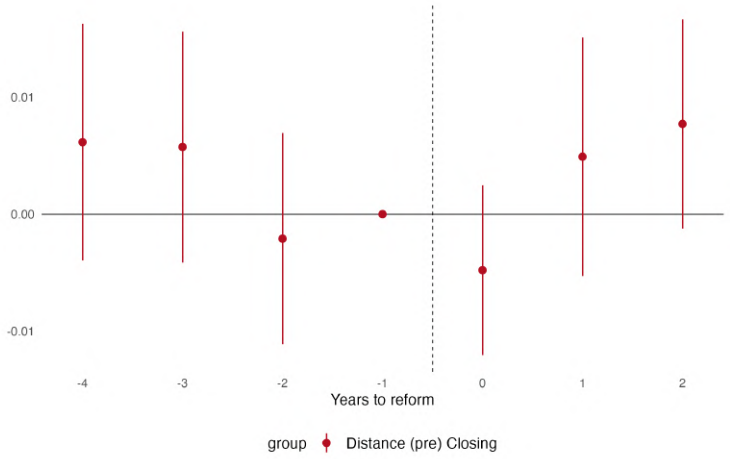
Notes. Observations are at the municipality level. The variable on the horizontal axis is the distance from the closest office in a different tax jurisdiction but in the same fiscal region (at baseline). The variable on the vertical axis is the distance from the jurisdiction tax office, before (top panel) or after (bottom panel) the reform. Red dots denote *closing* municipalities; blue dots denote *receiving* municipalities; gray dots denote *unaffected* municipalities.

Figure A16 — Effect of distance stronger where local information is more valuable: alternative classification

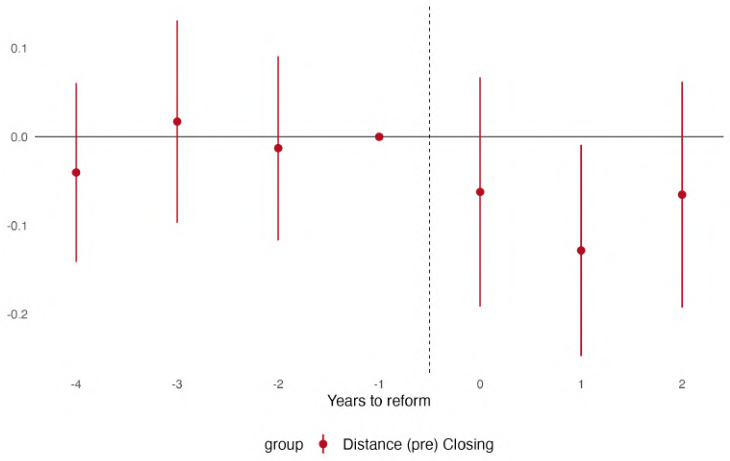


Notes. Observations are at the municipality-year level. The dependent variable is the log of federal tax collection. The plots report the estimated coefficients and the 95% confidence interval for the interaction between year and *Distance other office (pre)* × *Closing* separately for municipalities with high (red dots) and low (red triangles) number of labor inspections. Local information is more valuable in municipalities with a low number of labor inspections. *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. *High Lab. Insp.*: indicator equal to 1 for municipalities above the 75th percentile in the distribution of the number of labor inspections carried out in a mesoregion in the years 2016–2019. *Low Lab. Insp.*: indicator equal to 1 for municipalities below the 75th 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. *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 A17 — The effect of distance on tax agents trips to municipalities



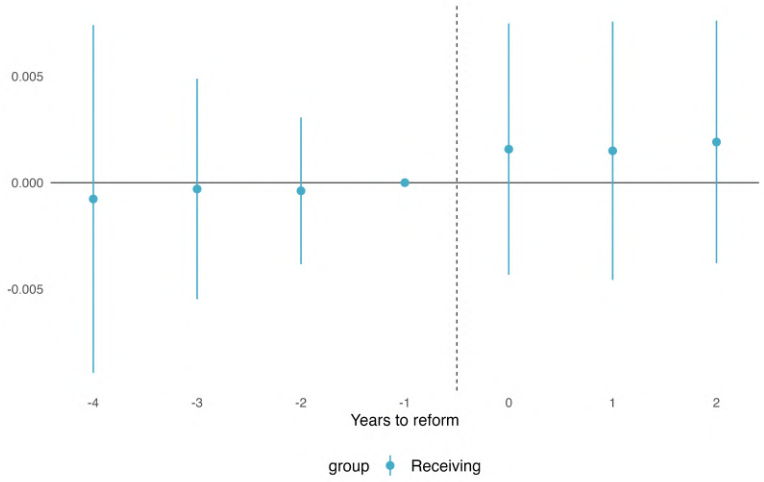
Notes. Dynamic version of column 2 of table 3.



Notes. Dynamic version of column 5 of table 3.

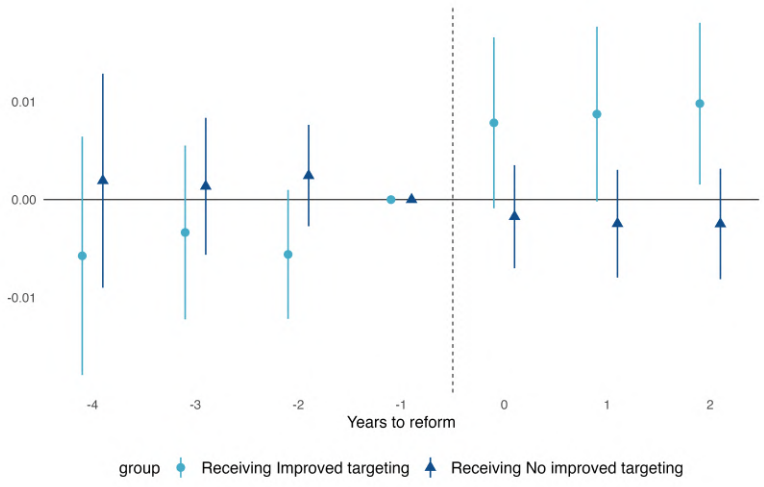
Notes. Observations are at the municipality-year level. The dependent variable is *Any Trip* (top panel) and *Log N. trips* (bottom panel)*Any Trip:* Indicator equal to 1 if any tax official from a local office visited a municipality in a year. *Log N. trips:* Log of number of times a tax official from 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 A18 — Number of tax agents per 1000 inhabitants



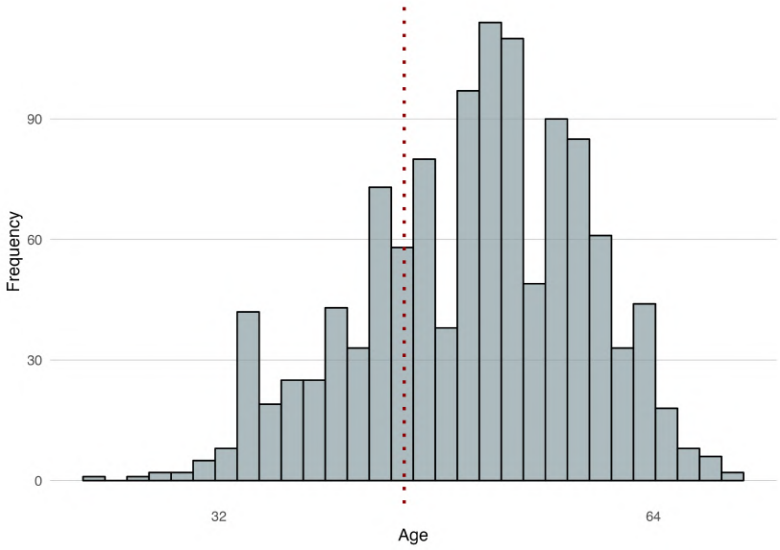
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable is the number of tax agents per 1000 inhabitants. This variable is computed using the post-reform jurisdiction boundaries.

Figure A19 — Number of tax agents per 1000 inhabitants by jurisdiction type



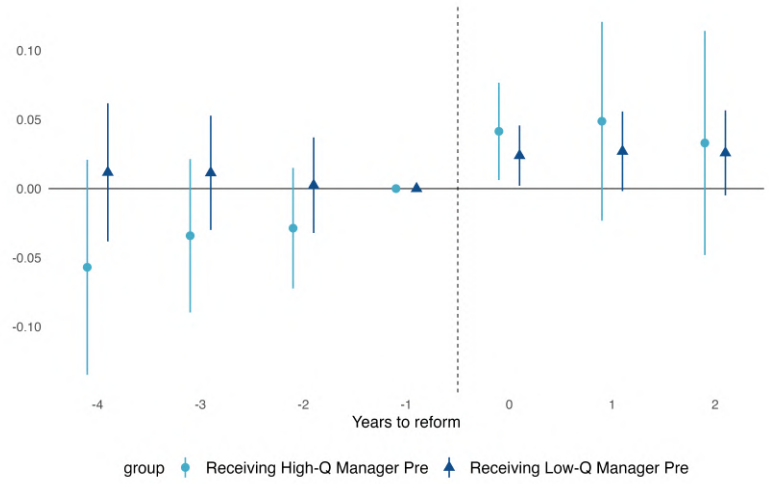
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable is the number of tax agents per 1000 inhabitants. This variable is computed using the post-reform jurisdiction boundaries. I report coefficients separately for *Improved targeting* and *No improved targeting* jurisdictions as defined in figure 8.

Figure A20 — Distribution of age at which tax agents are promoted to manager



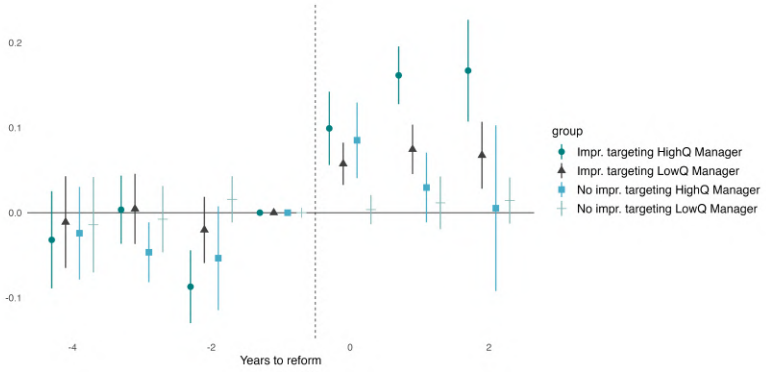
Notes. The histogram plots the age at which tax agents in local offices and lower-tier offices are promoted to a manager level. This managerial level is the first at which tax agents could oversee a local office. The red line represents the 25th percentile in the distribution. The sample excludes all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

Figure A21 — Allocation of managerial resources and tax revenues (allowing for managerial turnover)



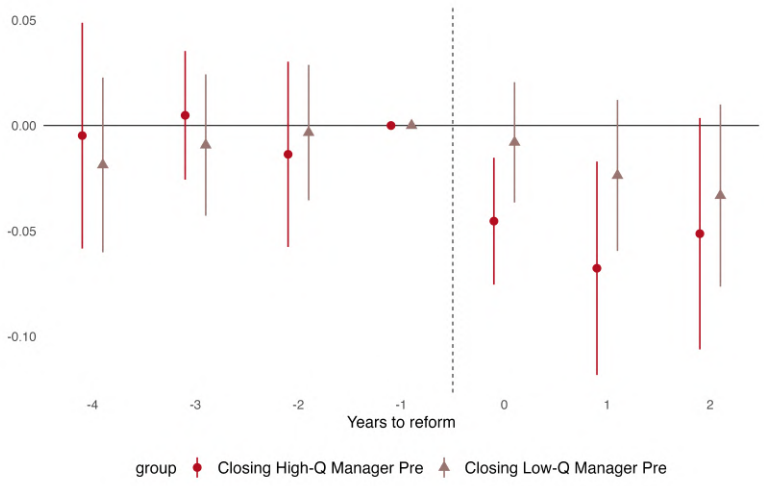
Notes. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. This plot reports point estimates and 95% confidence intervals for the interaction between year and *Receiving* separately for municipalities in jurisdictions overseen by a high-quality (blue dots) and low-quality (blue triangles) manager. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office closed in 2020. *High-quality manager pre* 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 A20. *Low-quality manager pre* is an indicator equal to 1 if the manager office in charge in 2019 got promoted to a managerial role at an age above to the 25th percentile in the distribution for figure A20. *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 A22 — Allocation of enforcement and managerial resources, and tax revenues



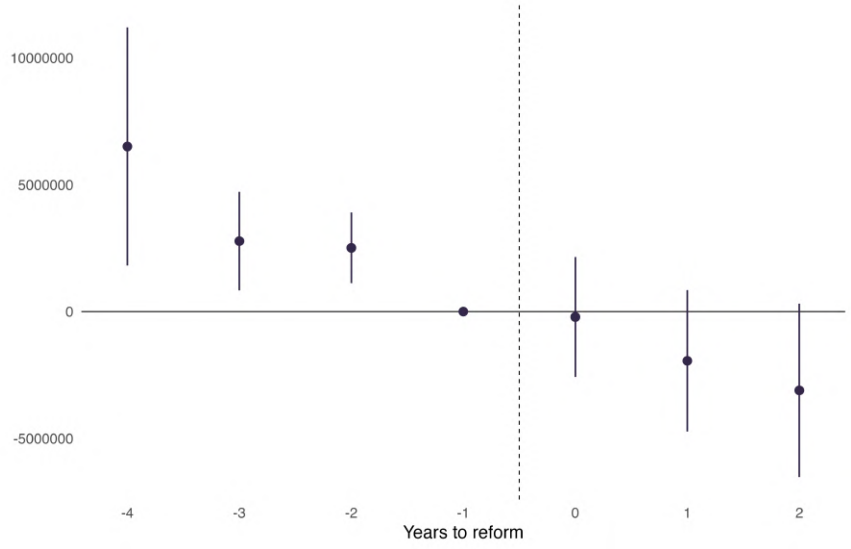
Notes. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. *Receiving* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office closed in 2020. This plot reports point estimates and 95% confidence intervals for the interaction between year and *Receiving* separately for municipalities in jurisdictions with room for improved targeting and overseen by a high-quality manager (blue dots), in jurisdictions with no room for improved targeting and overseen by a high-quality manager (blue squares), in jurisdictions with room for improved targeting and overseen by a low-quality manager (blue triangles), and in jurisdictions with no room for improved targeting and overseen by a low-quality managers (blue crosses). *Improved targeting* is an indicator equal to 1 if in a jurisdiction revenue potential is concentrated in *receiving* municipalities. *No improved targeting* is an indicator equal to 1 if in a jurisdiction the *receiving* municipalities are less likely to be the priority for tax enforcement. See definition in figure 8). *High-quality manager pre* 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 A20. *Low-quality manager pre* 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 above to the 25th percentile in the distribution for figure A20. *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 A23 — Closing offices overseen by high-quality managers and tax revenues



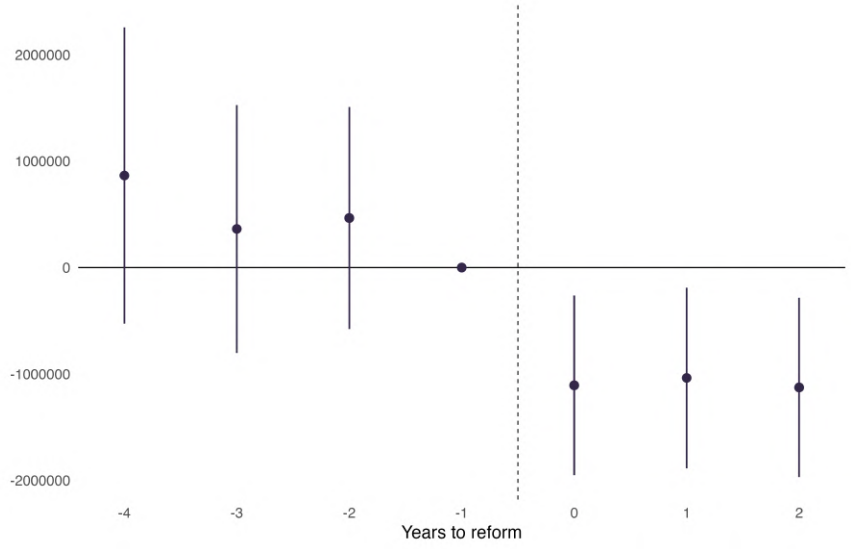
Notes. This plot reports point estimates and 95% confidence intervals. Observations are at the municipality-year level. The dependent variable in the log of federal tax collection. This plot reports point estimates and 95% confidence intervals for the interaction between year and *Closing* separately for municipalities in jurisdictions overseen by a high-quality (red dots) and low-quality (redtriangles) manager. *Closing* indicator equal to 1 for municipalities whose jurisdiction tax office is shut down in 2020. *High-quality manager pre* 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 A20. *Low-quality manager pre* 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 above to the 25th percentile in the distribution for figure A20. *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 A24 — The net effect of centralization on expenditure for frontline tax agents' wages: aggregate level



Notes. This plot reports point estimates and 95% confidence intervals for being a *centralized* jurisdiction. Observations are at the post-reform jurisdiction-year level. The dependent variable in the expenditure on wages for all the frontline tax agents deployed in the jurisdiction local office(s). Frontline tax agents are those without a managerial position. *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.

Figure A25 — The net effect of centralization on expenditure for managers' wages: aggregate level



Notes. This plot reports point estimates and 95% confidence intervals for being a *centralized* jurisdiction. Observations are at the post-reform jurisdiction-year level. The dependent variable in the expenditure on wages for all mangers deployed in the jurisdiction local office(s). This includes all the managers (not only the office top managers). *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.