ORGANIZING FISCAL CAPACITY

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Abstract

This paper investigates how the spatial organization of a tax authority shapes fiscal capacity. I study a reform of the Brazilian tax authority that closed one-fourth of its local offices tasked with tax enforcement, and consolidated tax agents and decision-making power in fewer, larger offices. Leveraging regional variation in the reform and geographically disaggregated data on tax collection and local office organization, I find that consolidation increased tax revenues by 3.3%. The increase was largest where the reform allowed for more efficient utilization of managerial and enforcement resources. However, the reform led to a divergence in tax revenues within consolidated regions, particularly in peripheral areas farther from their new office and with weaker third-party reporting. My findings suggest that consolidation can lead to efficiency gains but also to amplified geographic inequality in tax enforcement.

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1 — Introduction

State capacity is key to economic and political development (Besley and Persson 2011). However, it is often unfeasible for governments to augment state capacity by investing additional resources, especially in low- and middle-income countries that are severely budget constrained. Therefore, governments have become increasingly interested in organizational reforms that do not require additional investments but can nonetheless improve the effectiveness of their administrative apparatus by improving the allocation of the existing resources.

This paper focuses on one essential function of the state—taxation—and examines a key choice for tax authorities: how to structure their spatial organization. One option is to staff many small local offices. This brings agents closer to the areas they need to monitor, potentially improving access to local information (Balán et al. 2022). Accessing this information is especially important for tax enforcement in contexts with limited third-party reporting (Jensen 2022). Alternatively, tax authorities can staff fewer but larger offices. This *consolidated* structure may enable better utilization of limited managerial and enforcement resources (Basri et al. 2021; Bergeron et al. 2022). While several countries have recently shifted toward a more consolidated structure (OECD 2015), there is limited empirical evidence on how this affects tax revenues.[1]

Providing empirical evidence on this issue is challenging. First, it requires disaggregated data on tax revenues and on the local organization of a tax authority. Second, it requires credible identification of organizational changes at a scale large enough to affect tax enforcement operations.

I overcome both challenges by examining a major reorganization of the Brazilian federal tax authority. The reform concentrated tax agents and decision-making power over larger jurisdictions in fewer local offices, resulting in a more consolidated structure in certain regions. Leveraging this regional variation and geographically disaggregated data on tax collection and local office organization, I employ a matched difference-in-differences strategy to evaluate the reform's impact on tax revenues and uncover the underlying mechanisms.

The Brazilian federal tax authority is responsible for collecting most government tax revenues, including income taxes, social security contributions, and taxes on firms. The entire country is partitioned into tax jurisdictions. Each tax jurisdiction spans several municipalities—the lowest-

1. Examples of countries that have consolidated their local offices in recent years include Austria, Brazil, Croatia, Denmark, Greece, Italy, Norway, and Romania.

tier administrative unit—and is overseen by a local office. Among other tasks, local offices are responsible for detecting tax evasion. They do so by combining available data with additional information, which can be collected either remotely or through physical inspections.

To measure the spatial organization of the tax authority, I harmonize information from internal regulations to reconstruct changes in tax office locations and tax jurisdiction boundaries over the period 2013–2023. Moreover, I assemble novel microdata on tax agents' careers using information on their deployment across offices, their managerial positions, and their work-related travel. I complement this with yearly data on the amount of federal tax revenues collected in each municipality. 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.

To causally identify the effect of changing the tax authority's spatial organization, I study a reform that was triggered by mandated budget cuts and involved half of the existing local offices. Twenty-four offices were closed, and the municipalities in their jurisdictions were reassigned to twenty-four other offices. Most of the tax agents previously deployed in closed offices were moved to these expanded offices as well. Because of the reform, the consolidated regions have fewer, larger offices; in other regions, the organization is unaffected, and municipalities continue to be served by the same local office. The reform allows me to leverage within-country variation in the tax authority's spatial organization while holding 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 random, municipalities in *consolidated* and *unaffected* jurisdictions display differential trends in economic activity and tax revenues before the reform. To overcome this issue, in my preferred specification I follow Basri et al. (2021) and use the entropy-balancing method of Hainmueller (2012) to create matched treatment and control groups balanced 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 similar trajectories 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 then evaluate the impact of the reform using a matched difference-in-differences design.

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

The main result of this paper shows that consolidation improved tax enforcement. Tax revenues in *consolidated* municipalities increase on average by 3.3% after the reform. The effect appears immediately after the reform and persists until the end of my sample period. A back-of-the-envelope calculation reveals that the aggregate increase in tax revenues due to the reform would be sufficient to fund Bolsa Família, the main social assistance program in Brazil, for almost one million additional households.[3]

The increase in tax revenues could be due to an increase in the amount of taxes recovered through auditing or to a reduction in evasion due to a behavioral response by taxpayers. I provide suggestive evidence of a 34% increase in the total tax credit assessed through auditing in *consolidated* jurisdictions after the reform; this increase is driven by audits being on average of larger amounts rather than more frequent. These findings suggest that the observed change in tax revenues is due, at least in part, to more effective tax auditing.

While the average effect of the reform is positive, it may mask considerable heterogeneity, as municipalities in consolidated jurisdictions are exposed to the reform in different ways. Some—the *center* municipalities—are still served by the same office, which has now expanded: it has more tax agents and oversees a wider jurisdiction. Others—the *periphery* municipalities—switch to a new office: this office is larger but tends to be farther away. To explore these potential differences, I apply the same strategy described above but consider two separate treatments based on exposure to the reform.

The second main result is that the reform amplified geographic inequality in tax enforcement. *Center* municipalities experience a 5% increase in tax revenues, whereas *periphery* municipalities experience no significant change. This divergence occurs despite the two groups displaying no differential pre-trends in revenues or differential trends in economic activity, either before or after the reform. This amplified inequality may have distributional consequences, potentially creating an efficiency-equity trade-off (Bachas et al. 2024), and may induce geographic sorting of firms and workers, leading to misallocation of economic activity and even to an erosion of the tax base in the long term (Fajgelbaum et al. 2019; Dix-Carneiro et al. 2021).

Several pieces of evidence support the validity of the empirical strategy underpinning these findings. 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

3. Moreover, I find no evidence of an increase in personnel expenses in *consolidated* jurisdictions after the reform; if anything, there are some savings in managerial costs.

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 taxable activity. Additionally, the results are robust to alternative matching procedures and to controlling for local characteristics that may affect tax revenues, such as municipality size, income, employment structure, formality rate, and market access. Moreover, evaluating the reform using a synthetic difference-in-differences method (Arkhangelsky et al. 2021) delivers similar results.

The second part of the paper investigates the mechanisms behind the reform's effects by testing the empirical predictions of a simple conceptual framework. In the model, the tax authority is conceptualized as a knowledge hierarchy responsible for revenue collection across two locations (Garicano 2000; Snowberg and Ting 2019; Gumpert et al. 2022). Tax enforcement requires processing problems of varying difficulty: easier problems are solved by frontline agents, while harder ones are escalated to office managers, who resolve them if they fall within their knowledge set and their time constraint is not binding. The authority can either staff one office in each location or consolidate agents and decision-making power in a single office responsible for both locations. The latter option (i) allows leveraging the most talented manager's knowledge across a larger share of regional problems (*managerial utilization channel*), but (ii) exacerbates distance-related frictions in gathering local information (*distance channel*); moreover, (iii) depending on the baseline allocation of agents, it can shift the share of problems most likely to be resolved (*enforcement utilization channel*). These three channels jointly shape the reform's impact on aggregate revenues and their spatial distribution.

First, to explore the managerial utilization channel, I investigate whether higher manager quality leads to larger revenue 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. Following Minni (2024), I define high-quality managers as those promoted to manager at a relatively young age. Consistent with the managerial knowledge channel, I find that revenue gains are larger in *consolidated* municipalities overseen by high-quality managers. However, I also show that these gains are attenuated when the consolidated office's total staff is too large and the manager is more likely to become unable to handle all escalated problems. This finding suggests that even if distance-related frictions were fully eliminated, a fully consolidated structure covering the entire country would be unlikely to be optimal.

Second, I investigate the relevance of distance frictions. I begin by documenting that after the reform, *periphery* municipalities are typically served by a new office that is farther away—on

average, 114 minutes farther than before, an 85% increase relative to the pre-reform mean. I then test whether aggregate revenue gains are larger when a greater share of the total tax base does not experience this increase in distance. Using pre-reform average GDP as a proxy for the tax base, I find that revenue gains are indeed stronger in jurisdictions where the tax base is concentrated in *center* municipalities.

The framework also predicts that distance frictions should amplify spatial inequality within consolidated jurisdictions, with *periphery* municipalities ending up farther from the new office experiencing less revenue gains. To isolate the effect of distance from confounding factors due to geographical remoteness, I leverage variation in the baseline distance between each municipality and the nearest *other* office that could serve it, combined with office closings, and control for a proxy of market access.[4] I find that *periphery* municipalities ending up one hour farther from the new jurisdiction tax office experience 0.8% slower growth in tax collection relative to *center* municipalities.

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 not be. The framework therefore predicts stronger effects of distance on revenues where local information is more valuable—namely, in areas with less third-party reporting. I test this hypothesis using geographic variation in workers' formality rates (Jensen 2022). The results show that the negative effects of distance on tax revenues are larger where formality is lower and local information thus more valuable, suggesting that distance hinders tax agents' ability to gather local information. To further support this mechanism, I use data on work-related trips by tax agents in the audit department as a proxy for physical inspections of taxpayer premises. I find suggestive evidence that municipalities farther from the office receive fewer inspections.

Third, to examine the enforcement utilization channel, I show that consolidation effects are stronger in jurisdictions where the reform redirects staff effort toward the *center* tax base—which benefits from the highest-quality manager without being subject to distance frictions. For each consolidated jurisdiction, I construct a measure of the baseline mismatch between the number of agents deployed in a pre-reform office and the size of its tax base. The results suggest that gains from consolidation are larger when the *center* office had fewer agents relative to its tax base before the reform. This highlights how the deployment of agents across the territory matters for the efficient use of limited enforcement resources (Basri et al. 2021; Bergeron et al. 2022; Kapon

4. I discuss the advantages and limitations of this approach in detail in Section 5.2.

et al. 2024).

Taken together, these findings show that consolidation raises overall revenues but exacerbates geographic inequality in tax enforcement. By uncovering the underlying mechanisms, the paper offers guidance to policymakers considering similar reforms elsewhere. For instance, consolidation may lead to larger revenue gains in contexts with high heterogeneity in managerial talent, geographically concentrated economic activity, limited geographic frictions, and high formality rates. More broadly, the results underscore the importance of bureaucratic spatial organization for state capacity.

Related Literature This paper contributes to several broad literatures. First, by showing how the organization of tax authorities matters for tax capacity, it speaks to the extensive body of work on taxation in developing countries (Besley and Persson 2014; Jensen and Weigel 2024; Okunogbe and Tourek 2024; Pomeranz 2015; Naritomi 2019; Best 2025; Almunia et al. 2024; Dzansi et al. 2022; Knebelmann et al. 2023; Aman-Rana and Minaudier 2024; Bachas et al. 2025).[5] By showing that the relocation of staff effort across locations is an important channel through which spatial organization shapes tax revenues, this paper is closely related to Basri et al. (2021), who show that allocating dedicated tax agents to large firms increases their compliance. The reform I evaluate allows me to quantify the net effects of this effort reallocation by accounting for revenue changes both among taxpayers who receive additional dedicated resources and among those who receive fewer.[6]

The findings in this paper complement recent work showing that delegation to local elites can allow the government to leverage local information for tax collection (Balán et al. 2022). I document that, even without delegating to external actors, tax authorities can leverage local information for successful tax enforcement in contexts with limited third-party reporting, but their ability to do so is constrained by how far they are from the areas they need to monitor. One implication of these results is that differences in the prevalence of third-party reporting affect not only the optimal tax structure (Best et al. 2015; Jensen 2022) but also the optimal way of organizing

5. It is also related to a large literature on public finance in high-income countries (Allingham and Sandmo 1972; Kleven et al. 2011; Rubolino 2023; Boning et al. 2024; Dwenger and Gumpert 2025).

6. In Basri et al. (2021), the revenue effect arises primarily from firms reducing evasion in response to a salient increase in detection probability. In my setting, the shock is less salient and part of the revenue increase is attributable to auditing. Furthermore, because enforcement varies across geographic units rather than across firms, this reform allows me to study the role of geographic frictions arising from distance.

a tax authority.[7]

Furthermore, I connect to the related strand of the literature examining the effects on fiscal capacity of administrative reforms in historical settings (Chambru et al. 2024; Chiovelli et al. 2024; Cantoni et al. 2024; Moreira and Pérez 2024). I provide complementary evidence from a contemporary context on the role of organizational choices in affecting fiscal capacity, and I show that administrative remoteness continues to affect the performance of bureaucracies in modern times.[8]

Second, the results of this paper relate to a vast literature studying state capacity through the lens of the personnel economics of the public sector (Dal Bó et al. 2013; Finan et al. 2017; Khan et al. 2016, 2019; Besley et al. 2022; Caria et al. 2022; Deserranno et al. 2025). This paper leverages microdata on the entire Brazilian federal tax authority to provide empirical evidence on how a system-wide reform affects bureaucratic effectiveness. I show that the spatial organization of a bureaucracy can affect its effectiveness, complementing recent research on how to optimally allocate limited resources for tax, law, and environmental enforcement (Basri et al. 2021; Bergeron et al. 2022; Kapon et al. 2024; Assunção et al. 2023; Facchetti 2025).[9] In doing so, I uncover a trade-off between efficient resource utilization and local information gathering. This trade-off may be relevant for bureaucracies and large organizations in other contexts beyond tax administration.

A related strand of this literature has focused on the allocation of decision-making power within the public sector (Bandiera et al. 2021; Vannutelli 2022; Kala 2024). 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.[10] By highlighting that this dimension can improve how the organization benefit from scarce managerial talent, I connect to recent papers studying the role of individuals as drivers of state capacity (Rasul and Rogger 2018; Otero and Munoz 2022; Bergeron et al. 2022; Fenizia 2022; Best et al. 2023).

The rest of the paper is organized as follows: Section 2 describes the institutional setting, the

7. This is in line with the literature studying how the observability of economic activity affects the creation and evolution of states (Scott 1998; Sánchez De La Sierra 2020; Garfias and Sellars 2021; Mayshar et al. 2022).

8. This finding relates to a broad literature across the social sciences discussing how physical distance limits state capacity and state reach (Mann 1984; Herbst 2014; Michalopoulos and Papaioannou 2014; Müller-Crepon 2023; Henn 2023; Mastrorocco and Teso 2025).

9. In a related study, Facchetti 2025 examines a reform of local police stations in London, highlighting a trade-off between officers' proximity to crime scenes and the deterrence benefits of larger, better-staffed units. This paper provides complementary evidence from a different institutional setting—tax administration in a developing country.

10. Relatedly, several papers have studied the role of administrative unit size for development (Narasimhan and Weaver 2024; Cassidy and Velayudhan 2024; Tricaud 2025; Dahis and Szerman 2025), as well as the delegation of fiscal responsibility to lower-tier administrative units (Gadenne and Singhal 2014).

reform of the tax authority, and outlines a simple conceptual framework. Section 3 introduces the data sources and lays out the empirical strategy. Section 4 presents evidence on the reform's impact on tax revenues. Section 5 investigates the mechanisms that explain these effects. Section 6 concludes.

2 — Context and Conceptual Framework

2.1. Background

Brazil is the fifth-largest country in the world by area and the sixth by population. 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 et al. 2022).

Brazil has three levels of government: federal, state, and municipal. As of 2024, there are 27 states and 5,570 municipalities.[11] The provision of public services is highly decentralized, with municipalities responsible for essential services like education, health, and transportation. Funding for these services comes mainly from intergovernmental transfers from the state and federal governments. Most tax collection responsibilities lie with the federal government, which, as of 2021, manages 56% of total tax collection. This includes personal and corporate income taxes, social security contributions, VAT on gross revenues and manufactured products, financial transaction contributions, and taxes on net revenues. Smaller components include taxes on fuel, insurance, and rural land.[12]

The federal tax authority Federal tax collection is overseen by the Federal Revenue of Brazil (*Receita Federal do Brasil*). Established in 1968, the tax authority manages tax administration, customs, and the fight against illicit trafficking (Ezequiel 2014, 2018). Salaries for tax agents account for around 65% of the tax authority'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 A1a. First, the country is divided into 10

11. There are 26 states and one federal district, which includes the capital, Brasília. Five municipalities were created in 2013. I use geographical units based on the 2010 municipalities as a consistent unit of observation. For brevity, I refer to them simply as municipalities. See Section 3.1 for more details.

12. 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.

fiscal regions.[13] Each region is managed by a regional headquarters (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 (yellow dots in the figure).[14] As of January 2020, there are 94 tax jurisdictions.

Tax enforcement responsibilities are shared between central units in Brasília, regional headquarters, and local offices.[15] Large taxpayers are managed by specialized teams at regional headquarters or by two dedicated offices in São Paulo and Rio de Janeiro. All other taxpayers are handled by local tax offices in collaboration with regional headquarters. Tax enforcement occurs in two steps: initial selection of potential evasion cases and subsequent auditing. The selection is done by teams that operate across the entire fiscal region. 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.

2.2. The Consolidation Reform

The 2020 reform reshaped the spatial organization of the tax authority. It consolidated 48 of the existing 94 local offices by shutting down 24 offices and expanding the personnel and jurisdiction of 24 other offices.[16] While discussions about cost rationalization and organizational restructuring had been ongoing since at least 2015, a decline in personnel due to centrally mandated budget cuts likely played a significant role in triggering the reform. These austerity measures led to a decline in both the total personnel size and in the number of agents deployed across local offices (Figure A2b), causing a shrinking in the number of tax agents per inhabitant (Figure A2c) and in the average local office size (Figure A2d).

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

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

15. 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, and local offices. It is important to stress that lower-tier offices are not tasked with tax enforcement. Thus, since this paper focuses on tax enforcement, I do not consider lower-tier offices in my analysis.

16. A smaller reform occurred in 2017, closing three local offices that were serving 55 municipalities. See the number of local offices by year in Figure A2a. Apart from closing local offices, both the 2017 and 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 that switch jurisdictions during the period 2016–2023 for reasons other than their office being closed down because of the 2020 reform.

The reform occurred at the end of July 2020.[17] Figure 1 illustrates it schematically using three offices as a stylized example. At baseline, each office serves the set of municipalities in its jurisdiction (the stylized towns at the bottom). Because of the reform, one office (the red one) is shut down. The municipalities in its jurisdiction are assigned to the jurisdiction of another office (the blue one). Similarly, its tax agents are redeployed to the expanded office. After the reform, there are only two tax jurisdictions (and offices). One jurisdiction experienced the consolidation, while the other was unaffected. Consistent with this illustration, Figure A3a shows that the area of the jurisdiction of closing offices drops to zero in 2020 and that the area of the jurisdiction of expanded offices increases. Similarly, Figure A3b shows that the number of tax agents in closing offices drops to zero in 2020 and that the number of tax agents in expanded offices increases. Ninety-two percent of the 1,057 tax agents from closed offices were redeployed to the same expanded office to which the majority of their municipalities were assigned.[18] Notice that the redeployment of agents occurred in the month after the reform, in August 2020.

While there is no official information on the criteria used to select which offices to close and which to expand, in Table A1 I explore which baseline characteristics predict the reform. I find that the reform is more likely to shut down offices with fewer than 100 tax agents and when they are closer to another office in the same fiscal region. Symmetrically, an office is more likely to be expanded when it has more than 100 tax agents and when its nearest other office has fewer than 100 tax agents.

The map in Figure 2a allows visualizing how municipalities are exposed to the reform and shows that there is considerable regional variation in how the reform plays out. 2,851 municipalities are *unaffected* by the reform, while 2,712 municipalities are part of a jurisdiction that was consolidated by the reform. In Table A2a, I report summary statistics for these two groups. *Consolidated* municipalities tend to have a larger share of agricultural employment, a smaller share of formal employment, and a larger area. They are also less distant from any tax office in general.

Consolidated municipalities can be further partitioned into two groups. 1,210 municipalities are the *periphery* of the new jurisdictions: these are the municipalities whose baseline jurisdiction office was shut down by the reform. The remaining 1,502 municipalities are the *center* of the new jurisdictions: these are the municipalities whose baseline jurisdiction office was expanded by

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

18. Of the remaining tax agents, 3% were deployed to another local office and 5% to another tier of the tax authority.

the reform. The map in Figure 2b shows which municipalities belong to each group. Table A2b shows that *periphery* municipalities have a smaller population and labor force and a lower level of economic activity than *center* municipalities.

2.3. Conceptual Framework

This section sets out a simple conceptual framework that guides the empirical analysis by explaining why and how different spatial organization structures of a tax authority affect aggregate fiscal capacity and its spatial distribution.

Following Snowberg and Ting (2019) and Cantoni et al. (2024), I model the tax authority as a knowledge hierarchy (Garicano 2000): a tax office faces problems of varying difficulty drawn from a known distribution, where each problem can be thought of as a tax audit. The office consists of two layers, each endowed with the ability to solve a subset of problems. All problems enter through the frontline layer, which independently resolves those within its knowledge set and escalates the remainder to the manager. The manager then resolves escalated cases that fall within her knowledge set, provided she is not already time-constrained.

There are two main departures from the standard model. First, following Gumpert et al. (2022), I introduce a spatial dimension by considering a tax authority that collects revenues across two locations. In a not consolidated structure, each location is served by a dedicated office with frontline agents and a manager, who address taxable problems drawn from that location's pool.[19] In a consolidated structure, a single manager oversees all frontline agents, concentrated in one office serving both locations; agents address problems drawn from a common pool, with each location's share proportional to its tax base. Agents face an additional friction when handling problems originating in the location farther from the office, capturing the idea that, where third-party reporting is limited, agents may need to conduct in-person audits to gather the local information required to resolve a case.

Second, I take the stock of workers and their knowledge as fixed, reflecting the fact that the tax authority operates under a hiring freeze and can only decide how to allocate existing personnel and decision-making power across offices. In this setting, the framework illustrates how consolidation affects tax revenues and through which channels.

19. This is consistent with the institutional setting I study, where regional headquarters select audit cases and assign them to local offices based on jurisdiction.

Environment. To collect revenues, a tax authority must resolve a continuum of revenue collection problems. Each problem has a difficulty level z, with lower values representing simpler problems. There are two locations ℓ: the center A and the periphery B. Let T_ℓ denote the taxable mass in location ℓ and define

$$s \equiv \frac{T_A}{T}, \qquad T = T_A + T_B, \qquad s \in (0,1).$$

Each successfully resolved problem yields one unit of revenue.

Tax authority. The tax authority is a knowledge hierarchy with two layers: the frontline layer, employing n frontline agents, and the managerial layer, employing two managers. All frontline agents are homogeneous with knowledge z_0. The two managers may be heterogeneous, with knowledge levels $z_1^{i,i'} > z_0$.

Tax enforcement. Every frontline agent draws one tax problem from the pool of problems in the office's jurisdiction. Problem difficulty z is distributed according to a CDF $F(z)$ with density $f(z) > 0$.

- A frontline agent independently resolves any problem with $z \leq z_0$ and escalates any problem with $z > z_0$. The escalation rate per agent is $\eta \equiv 1 - F(z_0)$.
- Managers receive escalated problems from frontline agents under their supervision. Each manager has a time endowment normalized to 1, so she can handle at most 1 unit of escalated problems. A manager with knowledge frontier z_1 resolves an escalated problem only if $z \leq z_1$; problems above her frontier go unsolved. A manager receiving $n_\ell \eta$ escalations handles at most $\min\{1,\, n_\ell \eta\}$ of them; the remainder go unsolved due to congestion. Of those she handles, a fraction $(F(z_1) - F(z_0))/\eta$ fall within her knowledge frontier and are resolved.

Frontline agents and managers also face a friction due to their distance from the problem's origin, which impedes access to local soft information. This friction is homogeneous across difficulty levels. Formally, an agent resolves a problem within their knowledge set with probability $\pi(D) \in (0,1]$, where D is the distance between the problem's origin and the agent's location. When the agent is co-located with the problem's origin, the friction is absent ($\pi(0) = 1$); the friction increases with distance ($\pi'(D) \leq 0$). Where third-party reporting is strong and local information matters less, this friction is more moderate.

The number of problems resolved by an office where the manager has knowledge z_1 and

supervises n_ℓ frontline agents is therefore

$$R^C = \pi(D)\left[n_\ell\, F(z_0) + \min\{1,\, n_\ell\eta\}\,\frac{F(z_1) - F(z_0)}{\eta}\right]. \tag{1}$$

Given this stock of agents and enforcement process, the tax authority chooses between a *not consolidated* and a *consolidated* structure.

Not Consolidated structure. In a not consolidated (NC) structure, each location has its own office that handles problems originating locally, so $\pi(D) = 1$ for all problems. Let $n_A = \alpha n$ and $n_B = (1-\alpha)n$ denote the number of agents in the center and periphery, respectively. Managers may be heterogeneous; I assume the center manager is at least as knowledgeable as the periphery manager ($z_1^A \geq z_1^B > z_0$). Applying (1) to each jurisdiction, local revenues are

$$R_A^{NC} = \alpha n\, F(z_0) + \min\{1,\, \alpha n\eta\}\,\frac{F(z_{1A}) - F(z_0)}{\eta},$$

$$R_B^{NC} = (1-\alpha) n\, F(z_0) + \min\{1,\, (1-\alpha) n\eta\}\,\frac{F(z_{1B}) - F(z_0)}{\eta},$$

and aggregate revenue is

$$R^{NC} = n\, F(z_0) + \min\{1,\, \alpha n\eta\}\,\frac{F(z_{1A}) - F(z_0)}{\eta} + \min\{1,\, (1-\alpha) n\eta\}\,\frac{F(z_{1B}) - F(z_0)}{\eta}. \tag{2}$$

Consolidated structure. In a consolidated (C) structure, a single office in the center covers both locations. Only the center manager is retained and all n agents are concentrated in the center.[20] The consolidated manager faces $n\eta$ total escalations and resolves

$$\min\{1,\, n\eta\} \cdot \frac{F(z_{1A}) - F(z_0)}{\eta}$$

of them. Define the congestion threshold $n^* \equiv 1/\eta$. Because there is only one manager, there exists an interval of total staff sizes ($n^* < n < \min\{n^*/(1-\alpha),\, n^*/\alpha\}$) for which the consolidated manager is congested while neither manager in the not consolidated structure is.

Problems are drawn from a common pool, with each location's share proportional to its taxable

20. For simplicity and in line with the institutional setting, I assume the retained manager is moved to the regional headquarters. One can relax this assumption by reassigning this manager to the frontline or by introducing an additional intermediate managerial layer.

mass: a fraction s originates in A and a fraction $1-s$ in B. The distance friction $\pi(D) \leq 1$ applies to all problems from B. Aggregate revenue is therefore

$$R^C = \big[s + (1-s)\pi(D)\big] \left[n\, F(z_0) + \min\big\{1,\, n\eta\big\}\, \frac{F(z_{1A}) - F(z_0)}{\eta} \right]. \tag{3}$$

Change in aggregate revenues after consolidation. When the manager is uncongested ($n \leq n^*$), (2)–(3) reduce to

$$R^{NC} = n\big[\alpha F(z_{1A}) + (1-\alpha)F(z_{1B})\big], \qquad R^C = nF(z_{1A})\big[s + (1-s)\pi(D)\big].$$

The change in aggregate revenues $R^C - R^{NC}$ depends on three forces. First, all escalated cases reach the most knowledgeable manager, expanding the set of solvable problems (*managerial utilization channel*). Second, resolving cases from location B is harder due to distance frictions (*distance channel*). Third, depending on the baseline mismatch between agent deployment and tax mass, consolidation can either increase or decrease the number of cases escalated to the most knowledgeable manager without incurring distance frictions (*enforcement utilization channel*).[21] Because these forces countervail each other, the impact of consolidation is ex ante ambiguous even when the manager is uncongested.

Once the manager becomes congested, the balance tilts further toward the not consolidated structure. Congestion implies that $n - n^*$ problems cannot be escalated to the manager, are handled only by frontline agents, and still incur the distance friction.

This framework also clarifies why consolidation affects the center and the periphery differently. The center (location A) is exposed to the *enforcement utilization channel*, which can be positive or negative, and may experience manager congestion. The periphery (location B) is exposed to the countervailing *managerial utilization* and *distance* channels. This highlights the importance of examining whether consolidation affects spatial inequality in revenues within consolidated jurisdictions.

21. When the center office was underserved relative to its tax base ($\alpha < s$), consolidation enables to solve more problems from the center; relative to the not consolidated case, these problems benefit from greater managerial knowledge and face no distance friction, so the enforcement utilization effect is positive. When the center was overstaffed ($\alpha > s$), consolidation shifts more problems to the periphery; relative to those previously handled in the center, these problems face the same managerial depth but incur distance frictions, so the effect is negative. When deployment was already proportional ($\alpha = s$), the enforcement utilization effect is absent.

Empirical Predictions The framework delivers several comparative statics that map to empirically testable predictions.

First, the *managerial utilization channel* implies that the increase in aggregate revenues should be increasing in managerial ability at the center, but attenuated when the manager becomes congested.

P1 *A higher-quality manager in the central office (larger Z_1^A) leads to a larger increase in aggregate revenues.*

P2 *The increase in aggregate revenues is attenuated when the manager becomes congested due to excessive total staff size (larger n).*

Second, the *distance channel* implies that aggregate revenue gains are larger when a smaller share of cases originates in the periphery, and that periphery locations experiencing more severe distance frictions diverge more from the center after consolidation.

P3 *The impact of consolidation on aggregate revenues is larger when the tax base is more concentrated in the center (larger s).*

P4 *Periphery locations farther from the office (larger D) experience greater divergence in tax revenues from the center.*

P5 *The effect of distance on divergence is attenuated where local information is less relevant for resolving tax problems (smaller $\pi'(D)$).*

Third, the *enforcement utilization channel* implies that consolidation raises revenues when it redirects effort toward problems originating in the center, which are not subject to distance frictions.

P6 *The impact of consolidation on aggregate revenues is larger when there is a baseline negative mismatch between the share of agents and the share of taxable problems in the center ($\alpha < s$).*

The framework clarifies that the impact of consolidation on tax revenues is ex ante ambiguous and may vary across locations based on how they are exposed to the reform. Motivated by this, the next section introduces the data and research design used to evaluate the reform; results are discussed in Section 4. Section 5 then investigates the underlying mechanisms guided by the empirical predictions above.

3 — Data and Empirical Strategy

3.1. Data

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

Geographical units I use the 5,563 municipalities present in 2010 as geographic units.[22] I compute travel distances between the main towns of each municipality using information from *OpenStreetMap*. I also compute the area (in square kilometers) for each municipality.

Tax collection I use data on federal tax collection at the municipality-year level for the period 2013–2023 from Receita Federal do Brasil (2023). For all municipalities, I have information on the total amount collected.[23] I deflate nominal values to 2018 prices.

Tax audits I obtain office-level data on tax audits through a FOIA request to the tax authority. These data contain information on 93,863 tax audits carried out in the period 2014–2023. For each audit, I have information on the (post-reform) tax jurisdiction in which the audit was carried out, the year in which it was concluded, and the amount assessed by the procedure.[24] I aggregate this information at the (post-reform) tax jurisdiction-by-year level.

Tax authority organization I consult the tax authority's internal regulations to reconstruct its organization and evolution over the period 2013–2023. Regulations on the organizational structure (*Regimento Interno*) report the organizational charts, names, and the functions of the various departments (Ministério da Fazenda 2012, 2017, 2020). Moreover, they contain the lists of all the regional units. From these lists, I obtain the locations of the regional headquarters and the boundaries of the fiscal regions, which do not vary in the sample period. I also obtain the locations of the local tax offices and whether they get closed down or not during my sample period. I complement this organizational structure with separate regulations defining the boundaries of tax jurisdictions for each local office (Receita Federal do Brasil 2012).[25]

22. The five municipalities created in 2013 are assigned to the 2010 borders based on the spatial overlap. As one of these municipalities overlaps with three 2010 municipalities, I combine these three municipalities as well. All GIS data are obtained from Pereira and Goncalves (2024).

23. For two small municipalities, in some years before 2015, the total collection amount is negative due to the accounting method of tax amendments. I drop these observations from my sample.

24. The dataset provided by the tax authority includes an identifier of the audited taxpayer' jurisdiction reflecting the post-reform boundaries. Notice that I do not have data on whether the amount assessed was actually paid by the taxpayer.

25. Every year there are many regulations about these jurisdictions. However, outside the years in which there are office closings, the jurisdiction redrawings are minimal. For each year, I use the jurisdiction that is in place on

Tax agents I collect data on the payroll of federal civil servants from the Transparency Portal of the Brazilian government (Brazilian Federal government 2023) for each month in the period 2013 to 2023. 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 30,660 unique tax agents over this period. As each tax agent has a unique identifier, I can track them over time and across organizational units. I then match organizational units to local offices and harmonize information on the wage, managerial position, age, and years of experience in the tax authority.[26] The resulting dataset allows me both to track tax agents' careers and to reconstruct precisely the size (and the identity) of the staff deployed in each local office.

Tax agents' trips I obtain data on the receipts of work-related trips undertaken by federal civil servants in the period 2014 to 2023 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 civil servant who made the trip. Additionally, I georeference the destination for each trip. Because I know who carried out the trip, I can assign each trip to an origin local office. Moreover, I classify a trip as being related to a tax audit if it is carried out by a tax agent working in the tax auditing department.[27] I aggregate this variable at the destination municipality-year level.

Socioeconomic characteristics I build measures of local economic structure using the 2010 demographic census (IBGE 2010; Base Dos Dados 2022). I compute employment shares by sector (primary, secondary, tertiary), the formality rate,[28] and the average income. I employ two time-varying measures of economic activity. I obtain a measure of municipality-level GDP (available up to 2021) from the statistical office (IBGE 2023; Base Dos Dados 2022). As an additional proxy of economic activity, I use VIIRS Nighttime Lights data from the Earth Observation Group (EOG

January 1.

26. As the payroll data do not report age or year of birth, I match them with administrative matched employer-employee data from the Relação Anual de Informações Sociais (Ministério do Trabalho e Emprego 2018). I match using name and the partially anonymized social security number. I retain matches where the disclosed digits of the social security number match perfectly and for which the Jaro-Winkler distance between the names is less than 0.2. In this way, I am able to match 94% of the tax agents.

27. I assign tax agents to the tax auditing department in two steps. I first identify the departments linked to tax auditing in the organizational chart of the tax authority. I then search for keywords related to these departments in the string with the information on the organizational unit of each tax agent.

28. Following the literature, I code a worker as formal if they reported being formal employee or paying social security contributions (Ulyssea 2018). I then compute the formality rate as the number of formal workers over the total number of workers.

2023), aggregated at the municipality-year level.

Sample selection To avoid contamination of the analysis by other tax jurisdiction changes, I exclude municipalities that change jurisdiction during the period 2016–2023 for reasons other than their office being shut down by the 2020 reform. Moreover, I drop two municipalities that at some point have a negative value of tax collection and eight 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. I also drop Brasília, Rio de Janeiro, and São Paulo, as these cities have a different tax enforcement regime.[29] This leaves me with a sample of 5,415 municipalities in the period 2013–2023.

3.2. Empirical Strategy

My objective is to study how the consolidation reform affects tax enforcement. I start by providing an overview of the empirical strategy. I then present my matched difference-in-differences design and the estimating equation for the municipality-level analysis before discussing the validity of the research design. Finally, I present the estimating equation for the complementary jurisdiction-level analysis.

Overview Tax revenues Y_i in municipality i depend on the strength of tax enforcement τ_i and on the tax base $\bar{Y}_i$. In this formulation, the strength of tax enforcement τ_i encompasses both the direct (the amount of taxes recovered after auditing given a level of tax evasion, which can be interpreted as capturing the enforcement described in Section 2.3) and the indirect (the deterrent effect of audits on the level of tax evasion) effects of tax enforcement (Allingham and Sandmo 1972).[30]

$$Y_i = \tau_i \bar{Y}_i \tag{4}$$

It follows from Equation 4 that it is possible to identify the impact on tax enforcement τ by comparing the evolution of tax revenues in *consolidated* and *unaffected* municipalities before and

29. Brasília is the federal capital and hosts a local office, a regional headquarters, and the federal headquarters. Rio de Janeiro and São Paulo are the two largest cities in Brazil and have a different tax enforcement regime because each of these two cities has more than one local office. Notice that the tax jurisdictions of these cities do not include any other municipality.

30. Because I focus on the short-run impact of the reform, I abstract away from the possibility that the reform induces additional behavioral responses such as firms shrinking in size or relocating to other municipalities.

after the reform if the two groups are on parallel trends in the evolution of their tax base $\bar{Y}$.

However, Brazil is a huge and highly heterogeneous country, and the reform was not implemented at random. While a plausible approach to address this issue could be to control for regional shocks (for instance, by including region-by-year fixed effects), the spatial nature of exposure to the reform would make it hard to identify the treatment effect with the remaining within-region variation.[31]

To overcome this challenge, I compute municipality-level balancing weights that match consolidated municipalities with non-consolidated municipalities based on the evolution of their economic activity in the years leading up to the reform. I then evaluate the reform using a municipality-level weighted difference-in-differences design with municipality and year fixed effects.

Matching and estimation To compute balancing weights, I follow Basri et al. 2021 and employ the entropy-balancing methodology proposed by Hainmueller 2012. Specifically, I match on the trajectory of municipality GDP in the years leading up to the reform, 2016–2020.[32]

As is standard in the matching literature, I impose a common support restriction on the variables used to match. In the main specification, I drop municipalities that fall within the top or bottom 1 percent of either the control or treatment distribution of the key matching variables; I also report robustness to more or less restrictive common support restrictions.[33]

I then estimate the effect of the reform as follows, where each municipality i in year t is weighted by its respective balancing weight:

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t(\text{Consolidated}_i \cdot D_t) + X_{it}\psi + \epsilon_{it} \tag{5}$$

where y_{it} is the log of federal tax revenues. I include a set of municipality fixed effects α_i, which capture time-invariant municipality-specific characteristics affecting tax enforcement, and year fixed effects α_t, which account for time-varying shocks (e.g., country-level economic shocks).

31. This challenge is particularly relevant for identifying the treatment effect separately for *center* and *periphery* municipalities.

32. Specifically, I first run a regression of log GDP on municipality and year fixed effects, and extract the residuals. I then match on the level of GDP in 2016 and on the residuals for the years 2017, 2018, 2019, and 2020.

33. In Figure A4 I report a map illustrating which municipalities are matched as well as their treatment status. Moreover, I report a balance table comparing *consolidated* and *unaffected* (Table A3a), and *center* and *periphery* municipalities after matching (Table A3b).

D_t is an indicator for each period between 2016 ($t = -5$) and 2023 ($t = 2$).[34] Consolidated$_i$ is an indicator that takes value 1 if municipality i is treated and 0 otherwise. The matrix $X_{i,t}$ includes a set of municipality-specific controls that I will introduce when describing the results. 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 2020, δ_{-1}, as the reform occurs in July of that year. Standard errors are clustered at the mesoregion level to allow for correlation in the error term across municipalities that share similar socioeconomic characteristics;[35] I will report robustness to alternative clustering levels and inference procedures.

I also report results from a standard difference-in-differences specification, where I estimate the coefficient for the interaction between the treatment and Post$_t$, an indicator that takes value 1 from 2021 onward.

$$y_{it} = \alpha_i + \alpha_t + \beta \text{Consolidated}_i \cdot Post_t + X_{it}\psi + \epsilon_{it} \tag{6}$$

When investigating how the effect of consolidation varies depending on municipalities' exposure to the reform, I estimate the following specifications:

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^C (\text{Center}_i \cdot D_t) + \sum_{t=-5}^{2} \delta_t^P (\text{Periphery}_i \cdot D_t) + X_{it}\psi + \epsilon_{it} \tag{7}$$

$$y_{it} = \alpha_i + \alpha_t + \beta_1 \text{Center}_i \cdot Post_t + \beta_2 \text{Periphery}_i \cdot Post_t + X_{it}\psi + \epsilon_{it} \tag{8}$$

where everything is as above, except that now I consider two separate treatments by including an indicator for municipalities whose jurisdiction office was expanded by the reform (Center$_i$) and one for municipalities whose jurisdiction office was closed by the reform (Periphery$_i$).

Validity of the research design The main identifying assumption is that tax enforcement in *consolidated* and *unaffected* municipalities would have followed parallel trends in the absence of the reform. Notice that I cannot observe tax enforcement directly but only tax revenues. However, building on Equation (4), parallel trends in the tax base and in tax revenues would imply parallel trends in tax enforcement.

34. In the baseline specification, I *bin* years 2013–2016 in the period $t = -5$.

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

To assess the plausibility of these assumptions, I start by showing that after matching, the evolution of local GDP in *consolidated* and *unaffected* municipalities is similar. In Figure 3a, I plot dynamic coefficients from Equation (5) for the log of local GDP, which is a plausible proxy for the tax base. Notice that we observe no difference in the evolution of GDP before the reform. As I was matching on the evolution of GDP in these years, this confirms that the matching procedure is successful. However, the figure also shows that there is no differential evolution of GDP after the reform. As I was not matching on post-reform GDP, this is not mechanical and provides evidence supporting the parallel trends assumption for the tax base.

As local GDP data are not available after 2021 and one may also be worried that changes in tax enforcement somehow affect the ability of the statistical agency to measure local GDP, I repeat the same exercise using the log of nightlights as an outcome. Nightlights have been widely used by economists as proxies for economic growth when subnational data are unavailable or when concerns arise about the reliability of official statistics (Henderson et al. 2012; Martinez 2022). I report the dynamic coefficients in Figure A5a. Again, there is no evidence of differential evolution of nightlights either before or after the reform. This provides further evidence supporting the parallel trends assumption for the tax base.

Similarly, Figures 3b and A5b show that the evolution of local GDP and nightlights in *center*, *periphery*, and *unaffected* municipalities is similar before and after the reform. Moreover, when reporting the main results, I will also show that the evolution of tax revenues in *consolidated* and *unaffected* municipalities is similar before the reform. Taken together, these facts suggest tax enforcement was evolving similarly across the three groups of municipalities before the reform.

In sum, any alternative explanation for the results I present in the next section should have the same timing as the reform, follow the same exposure pattern as the reform, not affect the evolution of economic activity, but affect the evolution of tax revenues.

Jurisdiction-level analysis I also examine the effects of the reform at the aggregate level, using post-reform tax jurisdictions as the geographic unit of analysis. This complementary exercise allows me to investigate the impact on tax audits (for which I have only data at the office level) and to evaluate the impact on personnel costs (that obviously vary at the office level).
I employ a standard difference-in-differences design:

$$y_{jt} = \alpha_j + \alpha_t + \rho \text{Consolidated}_j \times \text{Post}_t + X_{jt}\psi + \epsilon_{jt} \tag{9}$$

where the outcome is the outcome of interest in jurisdiction j in year t. I include jurisdiction fixed effects α_j to control for jurisdictions' time-invariant characteristics and year fixed effects α_t, which capture time-varying aggregate shocks. Consolidated$_j$ is an indicator equal to 1 if a jurisdiction was consolidated. Post$_t$ is an indicator equal to 1 from 2021 onward. In some specifications, I also control flexibly for the average municipality characteristics in a jurisdiction.

4 — Impact of the reform

This section presents the main results on the effects of the reform. I first examine the impact on tax revenues at the municipality level by discussing the average effect, the differential effects based on exposure to the reform, and several robustness checks and additional evidence in support of the validity of the empirical strategy. I then present results on tax audits and personnel costs at the aggregate level.

4.1. The effects of consolidation on tax revenues

Average effects of the reform In Table 1, I report the main results on the average effect of consolidation on federal tax collection at the municipality level by estimating Equation 6. The coefficient in column 1 shows that tax revenues in *consolidated* municipalities increase by 3.3%, statistically significant at the 5% level. In column 2, I include a set of municipality characteristics interacted with year fixed effects. Specifically, I include controls for municipality size (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). After including this set of controls, the coefficient remains unchanged and precisely estimated.

Figure 4 displays dynamic coefficients from the specification in Equation 5. The graph shows no significant differential pre-trends. After the reform, *consolidated* municipalities experience a significant increase in tax revenues, with the effect being larger in the last two periods. This suggests a persistent improvement in tax enforcement in response to the reform.

A back-of-the-envelope calculation reveals that the aggregate increase in tax revenues due to the reform would be sufficient to fund Bolsa Família, the main social assistance program in Brazil, for 943,154 additional households, or 11% of the total number of households receiving it as of 2019.[36]

36. I compute this number as the ratio between the total gains in revenues associated with the reform and the subsidy paid to households (6,105 Brazilian reais, 2018 prices). I calculate the total gains in revenues as the number of

Differential effects by exposure to the reform I then investigate how the effect of the reform varies depending on municipalities' exposure to the reform by estimating Equation 8. The results are reported in Table 1. The coefficients in column 3 show that tax revenues in *center* municipalities increase by 5.1%, statistically significant at the 1% level, while the impact in *periphery* municipalities is smaller (1.2%) and statistically indistinguishable from zero. In column 4, I include the same set of municipality characteristics interacted with year fixed effects as above. After including these controls, the estimates imply a 5.3% increase in revenues for *center* municipalities and a 1.1% change in revenues for *periphery* municipalities (not statistically significant). In both cases a t-test confirms that the difference in effects between the two groups is different statistically significant at least at the 10% level.

Figure 5 displays dynamic coefficients from the specification in Equation 7 for both *center* (blue triangles) and *periphery* (red dots) municipalities. For both groups, the graph shows no significant differential pre-trends. After the reform, the point estimates imply a small increase in tax revenues for *periphery* municipalities; while this effect is larger in the last two periods, it is never statistically significant at conventional levels. On the other hand, *center* municipalities experience a sudden and persistent increase in tax revenues. The difference in the post-reform coefficients between the two groups is 0.042, and a t-test confirms that it is statistically significant at the 5% level.

This divergence may have distributional consequences, potentially creating an efficiency-equity trade-off.[37] Moreover, it may induce geographic sorting of firms and workers, leading to misallocation of economic activity (Fajgelbaum et al. 2019; Dix-Carneiro et al. 2021), and generate political grievances, deteriorating the quality of the social contract (Weigel 2020; Montenbruck 2023).

Effects by type of tax In Table A4 I explore the effect differentially by type of tax. One can observe that overall increase in revenues is largely driven by an increase in revenues from social

consolidated municipalities (2,712) times the average tax collection in the pre-reform period (64,079,984 Brazilian reais, 2018 prices) times the percent change in revenues implied by the estimated coefficient (3.3%).

37. The direction of distributional consequences is not obvious ex ante. On the one hand, *center* municipalities tend to be richer than *periphery* municipalities, suggesting that stronger tax enforcement in *center* municipalities leads to more progressive tax enforcement. On the other hand, if the reform shifts enforcement from the top of the income distribution in *periphery* municipalities to the middle of the income distribution in *center* municipalities, it may lead to either more progressive or more regressive tax enforcement depending on how the two distributions compare. Unfortunately, disaggregated data on tax collection by income type that would enable me to shed light on this question are unavailable.

security contributions, rather than from (personal or corporate) income tax or other taxes. This is true both when looking at the aggregate impact of consolidation and at the differential impact by exposure to the reform. Social security contributions are the largest component of federal revenues and include payroll taxes (levied both on employers and employee), which are easier to monitor thanks to third party reporting than other taxes.

4.1.1. Discussion and robustness checks

In this section, I conduct several exercises to verify the robustness of the findings. First, Figure A6 shows that the results are robust to alternative matching strategies. I reproduce the baseline matching strategy using entropy balancing (Hainmueller 2012) and the trajectory of GDP in the years 2016–2020 as matching variables, and then show results employing the same approach but matching on more years of GDP data (2013–2020), additional municipality characteristics (area, population, employment shares, and income in 2010), and using the baseline matching variables but with a propensity score (estimated via logit) and inverse probability weighting. The results are essentially unchanged.

Second, in Figure A7, I show that the results are unaffected by different common support sample restrictions (dropping 2.5% or 5% of the top and bottom of the distribution of the matching variables, or not dropping any observations).

Third, in Figure A8, I report robustness to clustering standard errors at alternative levels (at the municipality, at the pre-reform or post-reform tax jurisdiction level) or to allowing for spatial correlation (Conley 1999). Overall, the coefficients remain precisely estimated, but it should be noted that they become noisier when clustering at either tax jurisdiction level; however, the average post-reform effect for *consolidated* municipalities is still statistically significant at least at the 10% level, and for *center* municipalities it is still statistically significant at least at the 5% level.

Fourth, I assess the sensitivity of the results to the inclusion of additional controls. I report the baseline specification with no controls and the specification equivalent to the even columns in Table 1, controlling for 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). Additionally, I report a specification controlling for formality rate and market access. Specifically, I control for the share of workers who are formal and for the distance from the nearest local tax office in a different tax jurisdiction (both variables are measured at baseline and interacted with year fixed effects). As local tax offices are usually

located in cities, this controls for time-varying effects of market access.[38] I additionally control (at baseline and interacted with year fixed effects). Lastly, I report a specification controlling directly for a time-varying proxy of economic activity: nightlights. Results are robust to the inclusion of these controls, as shown in Figure A9.

Fifth, as one may be worried that results are driven by specific geographic regions, I estimate the baseline specification in different samples by dropping each of the 136 mesoregions in the sample one by one. For each regression, Figure A10 reports the average coefficients separately in the pre-reform and post-reform periods. The plot shows that the treatment effects are remarkably stable across all the different estimation samples.

Sixth, to assuage concerns about the results being dependent on the choice of the matched difference-in-differences approach, I also evaluate the effects of the reform using a synthetic difference-in-differences design (Arkhangelsky et al. 2021). This approach builds a synthetic control by reweighting *unaffected* municipalities so that their pre-reform trends in tax revenues are approximately similar to those of the treated ones. Also in this case, the effects are overall similar to the matched difference-in-differences results. Figure A11 reports the evolution of tax revenues for *consolidated* and *synthetic unaffected* municipalities. The implied treatment effect is a 2.9% increase in tax revenues, statistically significant at the 5% level.[39] Moreover, when looking at differential effects based on exposure to the reform, this method delivers results very similar to the matched difference-in-differences.[40] *center* municipalities experience a 4.6% increase in tax revenues (Figure A11), while *periphery* municipalities experience a 0.9% increase in tax revenues, statistically indistinguishable from zero (Figure A11).

4.2. The effects of consolidation on tax audits

The increase in tax revenues in *consolidated* municipalities could be due to an increase in the amount of taxes recovered through auditing (*detection* channel) or to a reduction in evasion due to a behavioral response by taxpayers (*deterrence* channel). Due to the lack of geographically disaggregated data on auditing, I use data on the amount of credit assessed by tax audits in each (post-reform) jurisdiction and conduct the analysis at a more aggregate level. As discussed in Section 3.1, I cannot observe whether the assessed credit was actually paid by the taxpayer.

38. I discuss this proxy in greater detail in Section 5.2.

39. For this exercise, p-values are computed via bootstrap (1,000 replications).

40. Notice that when using this approach, I have to conduct two separate estimations: one comparing *center* and *unaffected* municipalities, and one comparing *periphery* and *unaffected* municipalities.

Therefore, I am unable to use this information to quantify the relative magnitude of the detection and deterrence channels. Yet, these data allow me to provide suggestive evidence on changes in the intensity of the detection channel.

Table 2 displays the impact of the reform on tax audits. Odd columns present results from Equation 9 without controls, while even columns include jurisdiction characteristics interacted with year fixed effects.[41] The coefficient in column 1 shows that the total tax credit assessed through auditing in *consolidated* jurisdictions increases by 34%, statistically significant at the 10% level. The coefficient becomes smaller (30%) and less precisely estimated once I include controls (column 2). The increase in the total amount of tax audits is driven by an increase of 25-32% in the average amount of an audit (columns 3 and 4), while there is no detectable effect on the number of audits carried out (columns 5 and 6), even if one should note that the point estimate is positive.[42] These results suggest that the increase in tax revenues is due, at least in part, to an increase in the intensity of tax enforcement and not only to taxpayers' behavioral response to a higher perceived probability of being audited. This is driven by more effective rather than more frequent auditing.

4.3. The effects of consolidation on personnel structure and costs

Having shown that the reform improves tax revenues, a natural question is whether it also changed the personnel structure and induced savings in personnel costs. To answer this, I compare *consolidated* and *unaffected* offices before and after the reform.[43]

Table A5 displays the impact of the reform on personnel structure and costs, reporting results from Equation 9. The point estimate in column 1 suggests that *consolidated* offices tend to have more tax agents after the reform, but the results are noisy and not statistically significant. Leveraging the granularity of the personnel data, I can break down the analysis by managerial level and look at frontline workers (column 2), middle managers (column 3), and top managers (column 4), and verify that personnel dynamics align with the setup of the model in Section 2.3.[44] One can observe that there is no statistically significant change in the number of frontline

41. I include the same characteristics as in the municipality-level analysis: 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 use the average among all the municipalities in a jurisdiction.

42. Dynamic coefficients are reported in Figure A12.

43. In the pre-reform period, I assign tax agents from closed offices to the expanded office to which the majority of its jurisdiction municipalities will be assigned.

44. I define frontline workers as those with no managerial position, middle managers as those with manager levels 2 to 4, and top managers as those with manager levels 5 to 7.

agents or middle managers. Instead, there is a precisely estimated 20% drop in the number of top managers; as there is usually one top manager per office, it is not surprising that having fewer offices reduces the total number of top managers. After the reform, these top managers tend to have a larger span of control, both in terms of supervised frontline agents (column 5) and in terms of supervised middle managers (column 6). Table A6 shows the consequences for personnel costs. Overall, there is no detectable effect on total (column 1), frontline (column 2), or middle manager (column 3) wage bills. There is a large (17%) reduction in the top manager wage bill (column 4), but the effect is noisily estimated.[45]

Summing up, these results suggest that the increase in tax revenues due to the reform does not come at the cost of higher personnel expenses; if anything, there are some savings in managerial costs.

5 — Mechanisms

The previous section documented that the reform led to growth in tax revenues—driven at least in part by more effective tax auditing—but also to amplified geographic inequality in tax enforcement. This section conducts several empirical exercises to understand the underlying mechanisms and, more broadly, to examine how the spatial organization of a tax authority shapes fiscal capacity, with the aim of informing policymakers considering similar reforms in other contexts.

The conceptual framework in Section 2.3 identifies three forces through which consolidation affects fiscal capacity: (i) the *managerial utilization channel*, (ii) the *distance channel*, and (iii) the *enforcement utilization channel*. The exercises below are organized around the empirical predictions associated with each channel.

5.1. The role of managerial utilization

Managerial resources. By concentrating agents and decision-making power over larger regions in fewer offices, the reform expands managers' span of control and geographic scope, potentially enlarging the set of solvable tax problems. The framework predicts that consolidation is particularly beneficial when the central office manager is of higher quality (P1).

To test this prediction, I examine whether higher manager quality leads to larger revenue gains from the reform. I classify high-quality managers using data on tax agents' careers and

45. Dynamic coefficients are reported in Figures A13 and A14.

conduct a heterogeneity analysis within the baseline difference-in-differences specification. I find that revenue gains in *consolidated* municipalities are larger where high-quality managers oversee the expanded office.

Following Minni (2024), I proxy manager quality with promotion speed. In this context, promotions reflect how higher-level managers assess a tax agent's performance and are not solely based on seniority. I define high-quality managers as those who reach work level 4 at a relatively young age.[46]

I compile a list of all managers overseeing local offices during the sample period.[47] To define and validate the high-quality measure, I consider all other tax agents deployed in local or lower-tier offices who reach work level 4 between 2013 and 2023.[48] Figure A15a shows the age distribution at promotion to work level 4. I classify as high-quality those managers promoted before age 42, corresponding to the bottom tercile of this distribution.[49]

The intuition is that faster progression up the managerial ladder signals higher performance, reflecting the tax authority's assessment of a manager's work. I validate this empirically by showing that high-quality status predicts future career success. While wage increases are limited in this context, deployment to regional or central headquarters serves as a promotion signal. Figure A15b shows that high-quality managers are more likely to be eventually deployed to headquarters.

I apply this measure to managers overseeing local offices in the reform year.[50] I then estimate the following heterogeneity specification:

46. I focus on work level 4 because it is the first level at which a tax agent can oversee a local office.

47. For each office-month-year, I identify the tax agent occupying the position of *"delegado(a)"* or *"delegado(a) adjunto"* with the highest work level.

48. This approach defines and validates the quality proxy on an out-of-sample group, avoiding overfitting.

49. Some managers overseeing local offices during the sample period were promoted to work level 4 before 2013, the start of the personnel data. For these managers, I manually retrieve their age at first promotion from the government Transparency Portal (Brazilian Federal government 2023).

50. Due to substantial managerial turnover during the reform, I focus on offices with a single manager throughout 2020. Fifty-three of the 67 post-reform jurisdictions meet this criterion. Figure A15c shows that the share of *center* municipalities exposed to a high-quality manager is higher than the corresponding share of *periphery* and *unaffected* municipalities.

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^H \left(\text{Consolidated}_i \times \text{High-quality Manager}_i \cdot D_t\right) + \sum_{t=-5}^{2} \delta_t^L \left(\text{Consolidated}_i \times \text{Low-quality Manager}_i \cdot D_t\right) + X_{it}\psi + \epsilon_{it} \quad (10)$$

Following the framework, I test whether consolidation effects are stronger when the local office manager during the reform period is of high quality ($\delta^H > \delta^L$).

Figure 6 presents the coefficients from the dynamic specification. After the reform, municipalities with low-quality managers see a 2.8% increase in tax revenues (significant at the 5% level). Consistent with the prediction, the effect is substantially larger—9.4%—in municipalities overseen by high-quality managers. A t-test rejects equality of the two effects (p-value $= 0.033$).[51]

In summary, this section documents that giving a larger span of control to talented managers leads to tax collection gains. This provides evidence on how a consolidated structure enables a better utilization of managerial talent.

Total staff and managerial congestion A second prediction of the framework (P2) suggests that the effects of consolidation, are attenuated whenever it leads to the office manager being congested. To test this hypothesis, I compute the total staff in a consolidated office by summing the staff of the center and periphery offices which merged together.[52] I then compare the evolution of tax revenues between consolidated municipalities with low (bottom two terciles) and high (top tercile) total staff.

Figure 7 displays the dynamic coefficients. Jurisdictions with a smaller total staff see a 5.1% increase in tax revenues (statistically significant at the 1% level), whereas jurisdictions with a larger total staff see a 1.3% increase (not statistically significant). A t-test of the difference yields a p-value of 0.055.[53] These results provide evidence consistent with the framework prediction that the gains from consolidation erode as the merged officer pool grows large enough to congest the pooled manager.

Unfortunately in this setting there is no variation that allows me pinning down at which

51. Column 1 of Table 3 reports the estimated coefficients for the pre-post specification.

52. I use the merging offices size rather than the actual size because one may be worried that there are endogenous relocation of personnel (e.g. more staff relocated to offices where tax enforcement is harder).

53. Column 2 of Table 3 reports the estimated coefficients for the pre-post specification.

office size the manager start being congested and thus informing policymakers about the optimal size of an office. However, this finding suggests that even if distance-related frictions were fully eliminated, a fully consolidated structure covering the entire country would be unlikely to be optimal.

5.2. The role of distance

The reform increases the average distance between tax offices and the municipalities they oversee. Figure A16a shows that on average, *periphery* municipalities end up 114 minutes farther away from their jurisdiction tax office after the reform (an 85% increase relative to the pre-reform average). The framework highlights that this increase in distance can be an important channel through which consolidation affects tax revenues.

While distance from administrative offices has been shown to matter for the ability of governments to implement policies (Mann 1984; Michalopoulos and Papaioannou 2014), it is less clear whether it matters for tax enforcement, especially in a context like Brazil where taxes are filed electronically, collected through the bank network, and technology is vastly used for tax auditing. In this context, distance may matter because it makes it harder to physically inspect taxpayer premises and collect local information. As a starting point, Figure A16b shows a negative correlation between travel distance and tax revenues at the municipality level. Furthermore, in Figure A17, I show that this correlation holds both before and after the reform and across the three groups of municipalities (based on how they are exposed to the reform).

In the following I conduct several empirical exercises to illustrate the relevance of the distance channel. First, I show that consolidation leads to larger aggregate revenue gains in jurisdictions where a larger share of municipalities do not experience an increase in distance frictions. Second, I leverage variation in the intensity of distance frictions to document how they shape geographic inequality in tax enforcement within consolidated jurisdictions.

Distance and aggregate tax revenues. The framework predicts that consolidation generates larger revenue gains when the tax base is more concentrated in *center* municipalities (P3), since a larger share of taxable problems then escapes the distance friction. To test this prediction, I classify consolidated jurisdictions by the share of pre-reform GDP concentrated in *center* municipalities, distinguishing between those in the top tercile of this distribution (High concentration tax base)

and the remainder (Low concentration tax base).[54] I then estimate the following specification in the matched sample:

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^{HC} \left(\text{Consolidated}_i \times \text{High concentration tax base}_i \cdot D_t\right) + \sum_{t=-5}^{2} \delta_t^{LC} \left(\text{Consolidated}_i \times \text{Low concentration tax base}_i \cdot D_t\right) + X_{it}\psi + \epsilon_{it} \quad (11)$$

Figure 8 reports point estimates and 95% confidence intervals. Consistent with the prediction, revenue gains in *consolidated* municipalities are larger in jurisdictions where the tax base is more concentrated in the center. In these jurisdictions, the reform raised tax revenues by 6.7%, compared with 2.2% in jurisdictions with low tax base concentration—an effect that is not statistically significant at conventional levels. A t-test rejects equality of the two effects (p-value $= 0.035$).[55]

Distance and spatial inequality in tax revenues. To test the predictions on distance frictions and spatial inequality, I focus on *consolidated* jurisdictions and exploit variation in the intensity of distance frictions among *periphery* municipalities induced by the reform.

A natural approach would be to use variation in post-reform distance from the central office. However, endogenous jurisdiction assignments after the reform could confound this measure—for instance, municipalities with declining tax potential may be assigned to a farther office. To address this concern, I leverage baseline variation in the distance between each municipality and the nearest other tax office; this distance becomes relevant only if and when the office serving a municipality closes. Because it uses baseline distances, this approach also addresses the concern that municipalities experiencing many nearby closings—perhaps due to declining regional tax potential—mechanically end up farther from their new office.

Figure A18 illustrates how this variable is constructed. Each municipality (stylized towns in the figure) belongs at baseline to a tax jurisdiction served by the corresponding office (colored dots). For each municipality, I compute the distance from the nearest tax office in a different jurisdiction but within the same fiscal region, $\text{Distance}_{i(0)}^{i \to \bar{r}}$ (dashed lines). After the reform, this distance becomes relevant for municipalities initially served by closed offices (crossed dots).

54. I consider the distribution across *consolidated* jurisdictions only, as the equivalent share for non-consolidated jurisdictions would be mechanically equal to 1. The second tercile is 0.65.

55. Column 3 of Table 3 reports the estimated coefficients for the pre-post specification.

For municipality D, the nearest other office lies in a different fiscal region (dashed-dotted line), at distance $\text{Distance}_{i(0)}^{i\to\hat{r}}$. Since municipalities cannot be assigned to offices in a different fiscal region, $\text{Distance}_{i(0)}^{i\to\bar{r}}$ excludes that distance. However, cases like municipality D allow me to control for time-varying effects of distance from the nearest office in *any* fiscal region—a proxy for market access. Since fiscal region boundaries matter for tax enforcement but not for goods trade, this allows me to isolate the effect of distance from the tax office from other effects of geographic remoteness.

Figure A19 displays the correlation between distance from the nearest office in a different jurisdiction but same fiscal region (horizontal axis) and distance from the actual jurisdiction office (vertical axis). The left panel shows a clear positive correlation before the reform, both for municipalities whose jurisdiction office closes in 2020 (red dots) and for those whose office remains open (blue and gray dots). The right panel shows that this relationship steepens after the reform for *periphery* municipalities only.

I estimate the following specification:

$$y_{it} = \alpha_i + \alpha_{rt} + \sum_{t=-5}^{2} \delta_t \left(\text{Periphery}_i \times \text{Distance}_{i(0)}^{i\to\bar{r}} \cdot D_t\right) + \iota\, \text{Distance}_{i(0)}^{i\to\hat{r}} \times \text{Year}_t + \epsilon_{it} \quad (12)$$

where all variables are as in Equation 8; $\text{Distance}_{i(0)}^{i\to\bar{r}}$ is the baseline distance from the nearest tax office in a different jurisdiction but same fiscal region. The coefficients of interest are the δ_t. Interacting $\text{Distance}_{i(0)}^{i\to\hat{r}}$—the baseline distance from the nearest office in any fiscal region—with year fixed effects flexibly controls for time-varying effects of market access.

As an alternative, I estimate a difference-in-differences specification comparing *periphery* and *center* municipalities, interacting the post-reform indicator with indicators for being above (High Distance) or below (Low Distance) the second tercile of the distribution of baseline distance from the nearest other office in the same fiscal region:

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^H \left(\text{Periphery High Distance}_i \cdot D_t\right) + \sum_{t=-5}^{2} \delta_t^L \left(\text{Periphery Low Distance}_i \cdot D_t\right) + \iota \, \text{Distance}_{i(0)}^{i \to \hat{r}} \times \text{Year}_t + \epsilon_{it} \quad (13)$$

Figure 9 displays the dynamic coefficients from Equation 12. There is no evidence of differential pre-trends. The estimates imply that the average increase in travel distance leads to 0.75% slower revenue growth in *periphery* municipalities relative to *center* municipalities.[56] Figure A20 displays the results from the high/low distance specification (Equation 13). Municipalities farther from the nearest other tax office experience a 7.3% smaller change in revenues relative to *center* municipalities, compared with 2.5% for municipalities closer to the nearest other office—a difference that is not statistically significant. A t-test rejects equality of the two effects (p-value $= 0.007$). These estimates are consistent with the framework prediction (P4): the revenue divergence between *center* and *periphery* municipalities is larger where *periphery* municipalities end up farther from the new tax office.

One mechanism through which distance can impede tax enforcement is by limiting agents' ability to physically inspect taxpayers' premises. I proxy for such inspections using data on tax agents' work-related trips, restricting to trips from local offices to municipalities within their jurisdiction made by agents in the auditing department. I estimate Equation 12 with a Poisson model, using the number of trips received at the municipality-year level as the dependent variable. Figure A21 reports the dynamic coefficients. There is no evidence of differential pre-trends. After the reform, the number of trips declines in the first two years,[57] though the coefficient in the third year is smaller and indistinguishable from zero.

The role of local information. The results above show that distance contributes to the divergence in tax enforcement. Given that taxes are filed electronically, collected through the bank network, and that technology is widely used for auditing, this finding is not obvious ex ante. While agents' ability to detect evasion using hard data should be unaffected by location, their ability

56. Distance from the jurisdiction office increases on average by 85% for *periphery* municipalities after the reform. Column 5 of Table 3 reports the estimated coefficients for the pre-post specification.

57. The implied average effect is a 14.9% reduction; a t-test rejects that the effect equals zero at the 10% level (p-value $= 0.09$).

to gather local information may not be. If distance matters because it impedes the gathering of local information, one would expect larger effects where local information is more valuable. The framework captures this through the friction parameter $\pi(D)$ and predicts that spatial divergence should be larger when third-party reporting is limited (P5). Third-party reporting, by generating an information trail, makes tax enforcement easier (Pomeranz 2015; Jensen 2022), implying that local information is less valuable where formality is higher. I therefore classify municipalities by the extent of baseline third-party reporting, using the formality rate from the 2010 population census. Municipalities in the top tercile of the formality rate distribution are classified as having *high* third-party reporting; the remainder as having *low* third-party reporting.

Figure 10 displays dynamic coefficients from Equation 12, allowing differential effects by third-party reporting status. Consistent with the framework, the effect of distance is concentrated in municipalities with *low* third-party reporting, where the average increase in distance leads to a 0.9% decline in revenues. In municipalities with *high* third-party reporting, the effect is close to zero and statistically insignificant. A t-test rejects equality of the two effects (p-value $= 0.03$).[58] This confirms the framework's prediction: distance frictions are more consequential where local information is more valuable for tax enforcement

5.3. The role of enforcement resources utilization

The framework highlights a third channel through which consolidation affects tax revenues: by pooling enforcement effort and drawing cases from the combined tax base, consolidation reallocates effort across locations. When enforcement effort was already concentrated in the *center* before the reform ($\alpha > s$), this reallocation is negative and attenuates the consolidation gain. Conversely, when *center* municipalities were under-served ($\alpha < s$), the reallocation is positive and amplifies the gains. The framework therefore predicts that consolidation benefits are larger when the *center* office had fewer agents relative to its tax base before the reform (P6).

I define the staff-GDP mismatch as the difference between the *center*'s share of total jurisdiction staff and its share of total jurisdiction GDP, so that a higher mismatch indicates that the *center* was under-staffed relative to its tax base ($\alpha < s$). Figure 11 shows that jurisdictions with a high pre-reform mismatch (above the second tercile) experience a 6.3% increase in tax revenues, compared with 2.7% for low-mismatch jurisdictions. While the difference is not statistically significant at conventional levels (p-value $= 0.15$), this exercise provides suggestive evidence in favour of the

58. Column 6 of Table 3 reports the estimated coefficients for the pre-post specification.

framework prediction: jurisdictions where the pre-reform staff allocation was more misaligned with the tax base experience larger consolidation gains.[59]

6 — Conclusion

Understanding which administrative reforms can make states more effective is of growing interest to scholars and policymakers. In this paper, I focus on a key choice faced by tax authorities: how to structure their spatial organization. Thanks to novel disaggregated data on the Brazilian federal tax authority's spatial organization and tax collection, I study a system-wide reform that consolidated half of the existing local offices. While several countries have undergone similar reforms, empirical evidence on their effects has been lacking.

The results reveal that on average, tax revenues increase in consolidated regions. The gains in revenues due to the reform would be sufficient to fund Bolsa Família, the main social assistance program in Brazil, for almost one million additional households.

This paper has also documented that the reform led to divergence in tax enforcement between the *center* and *periphery* of consolidated regions. This finding is relevant for policymakers whose objective may be not only to maximize tax revenues but also to consider equity in tax enforcement intensity across different regions (Bachas et al. 2024). Beyond distributional concerns, stark differences in tax enforcement intensity may incentivize firms to relocate to areas where it is easier to evade taxes and lead to distortions in the allocation of economic activity and even to an erosion of the tax base in the long term (Fajgelbaum et al. 2019; Dix-Carneiro et al. 2021). Moreover, regional inequality in tax enforcement could have downstream consequences for political accountability and participation (Weigel 2020; Montenbruck 2023). Future work should shed light on these broader impacts.

By uncovering some forces underlying the trade-off between a more or less consolidated structure, this paper offers guidance to policymakers interested in implementing similar reforms in other contexts. It suggests that a consolidated structure may lead to larger revenue gains in contexts characterized by scarce managerial talent, geographically concentrated economic activity, high levels of formality, and limited geographical frictions.

More broadly, this paper shows that the effectiveness of a bureaucracy can be affected by its spatial organization and uncovers a trade-off between leveraging scarce managerial talent

59. The lack of statistical precision likely reflects limited variation in the staff-GDP mismatch measure. Column 4 of Table 3 reports the estimated coefficients for the pre-post specification.

and maintaining proximity to local information; this trade-off is particularly relevant wherever enforcement depends on soft information. 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 (Assunção et al. 2023) and with delivering social assistance programs (Muralidharan et al. 2023; Banerjee et al. 2024). Investigating how the effects of different organizational choices are shaped by the available monitoring and enforcement technologies is an interesting area for future research.

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Tables

Table 1: Effects of the reform on tax revenues

	Tot. Tax (log)			
	(1)	(2)	(3)	(4)
Consolidated × Post	0.033**	0.033***		
	(0.014)	(0.012)		
Periphery × Post			0.011	0.010
			(0.016)	(0.014)
Center × Post			0.049***	0.052***
			(0.018)	(0.015)
Municipality	✓	✓	✓	✓
Year	✓	✓	✓	✓
Controls		Yes		Yes
P-value: Center = Periphery			0.052	0.011
R^2	0.98	0.98	0.98	0.98
Clusters	136	136	136	136
Observations	54,450	54,450	54,450	54,450
Dep. Var. Mean	64,080	64,080	64,080	64,080

Notes. The table presents estimates from Equation 6 (columns 1 and 2) and from Equation 8 (columns 3 and 4). Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. *Post* indicator equal to 1 for years after the reform (2021-2023). *Consolidated* indicator equal to 1 for municipalities part of consolidated jurisdiction. *Periphery* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Center* 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 year. *Controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log)—all from 2010 census and interacted with year fixed effects. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the mesoregion level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 2: Effects of the reform on tax audits

	Audit amount (log)		Audit avg. amount (log)		N. audits (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Consolidated × Post	0.293*	0.265	0.279**	0.229	0.014	0.035
	(0.164)	(0.163)	(0.136)	(0.148)	(0.117)	(0.107)
2021 Tax Jur.	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
Controls		Yes		Yes		Yes
P-value:	0.074	0.104	0.041	0.120	0.903	0.741
R^2	0.74	0.78	0.46	0.51	0.86	0.88
Clusters	67	67	67	67	67	67
Observations	737	737	737	737	737	737
Dep. Var. Mean	937,209	937,209	8,490	8,490	118	118

Notes. The table presents estimates from Equation 9. Observations are at the post-reform tax jurisdiction-year level. The dependent variable are: log of total amount of audit posted (columns 1 and 2), log of average amount of an audit (columns 3 and 4), and log of number of audits carried out (columns 5 and 6). *Post* indicator equal to 1 for years after the reform (2021-2023). *Consolidated* indicator equal to 1 for consolidated jurisdictions. *Fixed effects:* 2021 tax jurisdiction and year. *Controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log)—all from 2010 census, averaged across the municipalities in the jurisdiction, and interacted with year fixed effects. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the post-reform tax jurisdiction level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

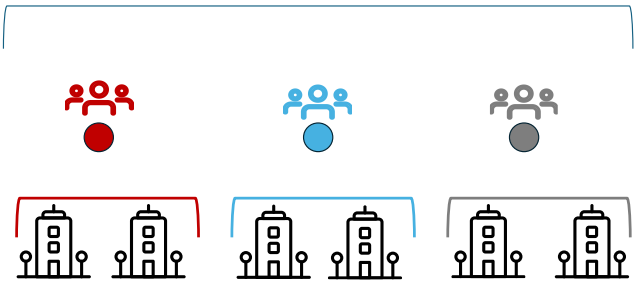
Table 3: Mechanisms: overview tests

	Manager	Total Staff	Concentration	Mismatch	Distance	
	Tot. Tax (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Consolidated High × Post	0.060**	-0.018	0.047**	0.059***		
	(0.027)	(0.016)	(0.019)	(0.021)		
Consolidated Low × Post	0.030	0.061***	0.025	0.021		
	(0.019)	(0.016)	(0.016)	(0.015)		
Periphery Log Distance other office (pre) × Post					-0.007**	
					(0.003)	
Periphery Log Distance other office (pre) High Formality × Post						-0.003
						(0.004)
Periphery Log Distance other office (pre) Low Formality × Post						-0.009**
						(0.004)
Municipality	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
P-value: High = Low	0.26	0.31	0.000013	0.062		0.16
R^2	0.98	0.98	0.98	0.98	0.98	0.98
Clusters	114	136	136	136	69	69
Observations	42,713	54,450	54,450	54,450	26,433	26,433
Dep. Var. Mean	64,080	64,080	64,080	64,080	64,080	64,080

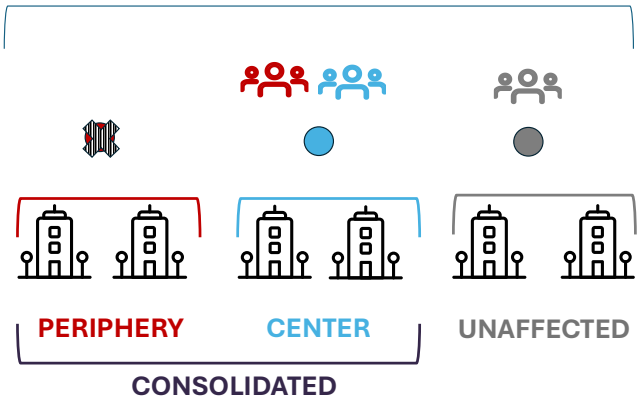
Notes. The table presents estimates from Equation 6 (columns 1 to 4) and from a static version of Equation 12 (columns 5 and 6). Column 1 reports the effects of the reform separately for municipalities in jurisdictions with *high* and *low* quality managers in charge at the time of the reform. Column 2 reports the effects of the reform separately for municipalities in jurisdictions with *high* and *low* total pre-reform staff. Column 3 reports the effects of the reform separately for municipalities in jurisdictions with *high* and *low* concentration of the tax base. Column 4 reports the effects of the reform separately for municipalities in jurisdictions with *high* and *low* pre-reform mismatch between staff deployment and tax base. Column 5 reports the effect of the distance at baseline from the nearest tax office in the same jurisdiction. Column 6 interacts distance with indicators for being a municipality with high or low formality rate. The sample in column 1 excludes the 14 jurisdictions which experience managerial turnover in the year of the reform. In columns 5 and 6 the sample include only municipalities in consolidated jurisdictions and the specifications control for a proxy of market access: the distance at baseline from the nearest tax office in another jurisdiction in any fiscal region. See Section 5 for definition of the variables. *Fixed effects:* municipality and year. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figures

Figure 1 — Illustration of how the reform affects different municipalities



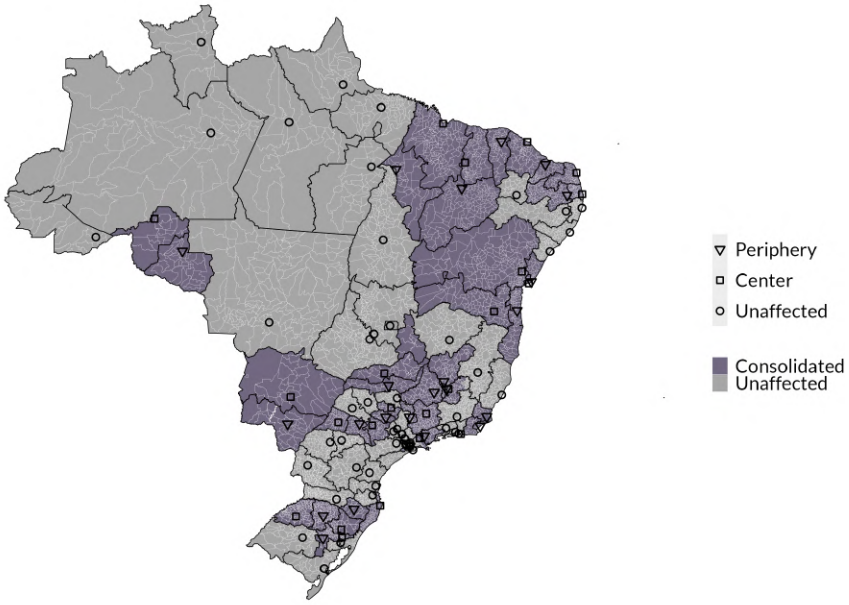
(a) Baseline



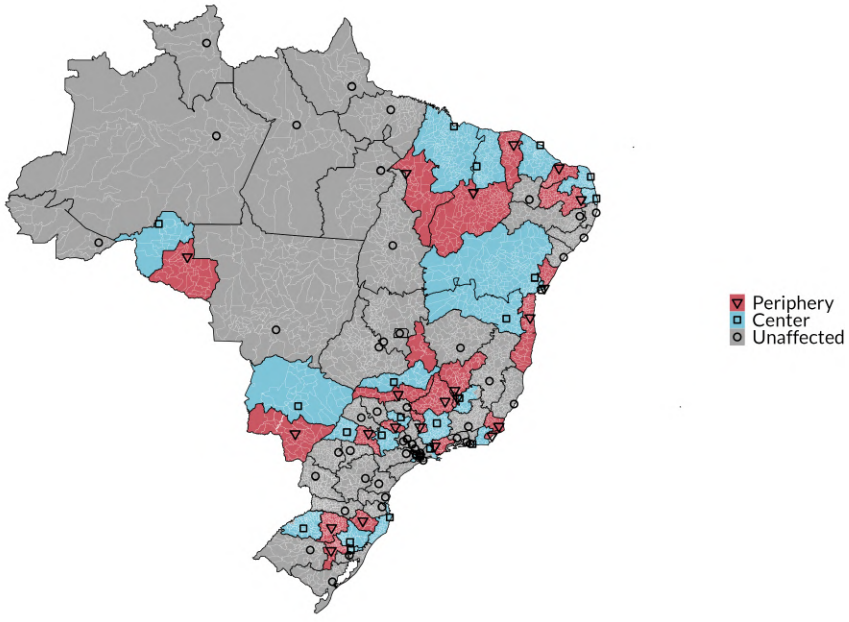
(b) Post reform

Notes. Schematic illustration of how the reform affects different municipalities. The top-left panel describes the baseline situation, with three offices (dots) serving two municipalities (stylized towns) each. The top-right panel describes the post-reform situations, with only two offices still open. *Consolidated* municipalities are part of jurisdictions affected by the reform, whereas *unaffected* municipalities are not. Within consolidated jurisdictions, we can distinguish between*center* and *periphery* municipalities. *Center* municipalities are the ones whose tax offices absorbs municipalities and tax agents from the closing offices. *Periphery* municipalities are the ones whose jurisdiction tax office closes in 2020.

Figure 2 — Map of municipalities by exposure to the reform



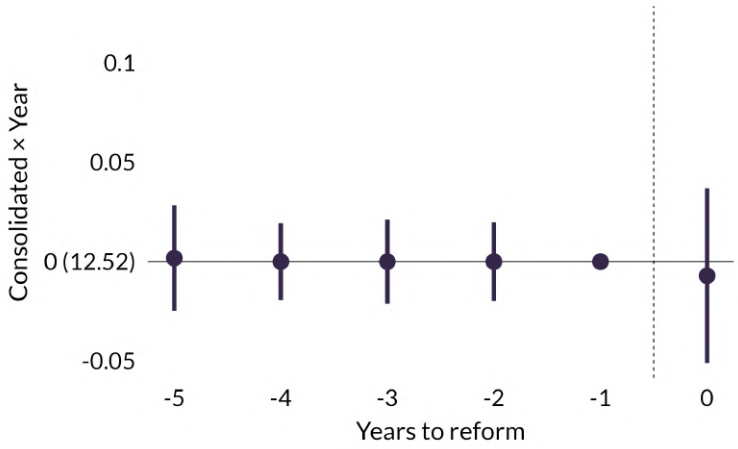
(a) *Consolidated* and *unaffected*



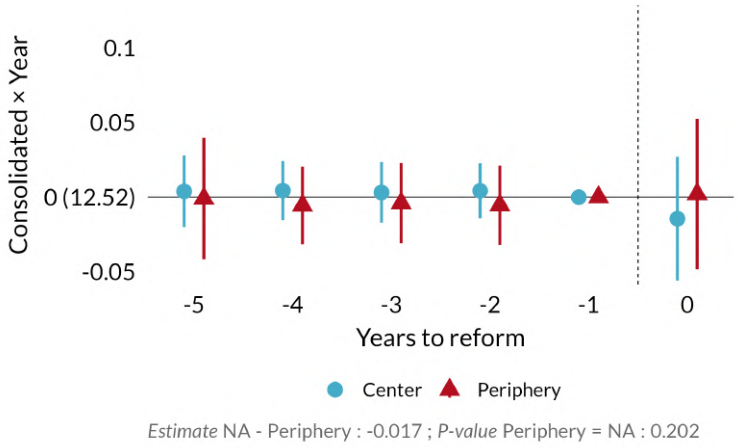
(b) *Core, periphery* and *unaffected*

Notes. These figures display how different municipalities are exposed to the reform. The top panel reports *consolidated* (purple) and *unaffected* (gray) municipalities. The bottom panel further partitions the *consolidated* group into *center* (blue) and *periphery* (red) municipalities. Thick black borders indicate the boundaries of pre-reform tax jurisdictions, and black shapes the corresponding local offices. Offices closed by the reform are marked with triangles, the ones expanded by the reform are marked with squares, and the ones unaffected are markers with dots. Thin white lines indicate the boundaries of municipalities.

Figure 3 — Matched treated and control are on similar trends in economic activity



(a) *Consolidated* and *unaffected*

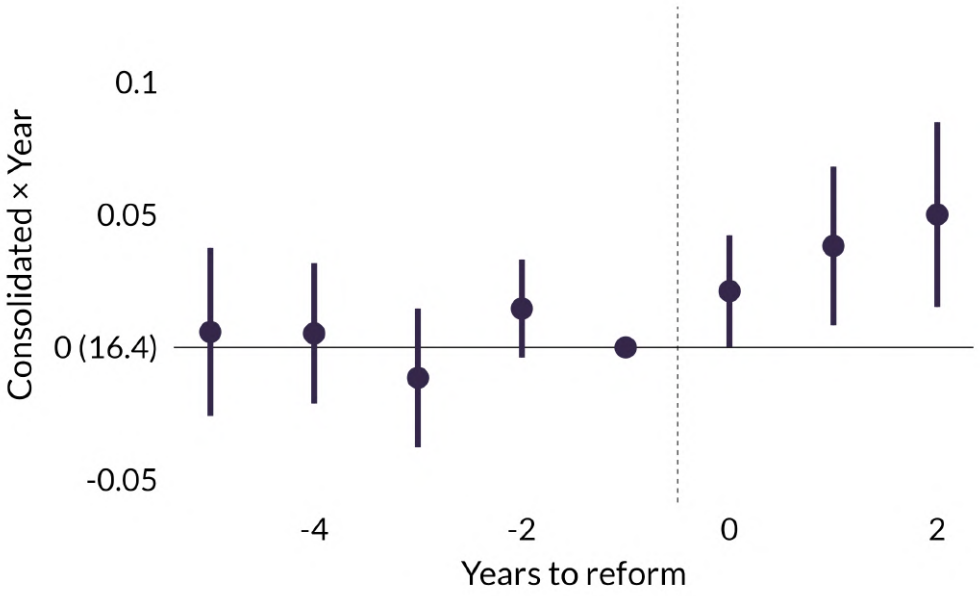


Estimate NA - Periphery : -0.017 ; *P-value* Periphery = NA : 0.202

(b) *Core, periphery* and *unaffected*

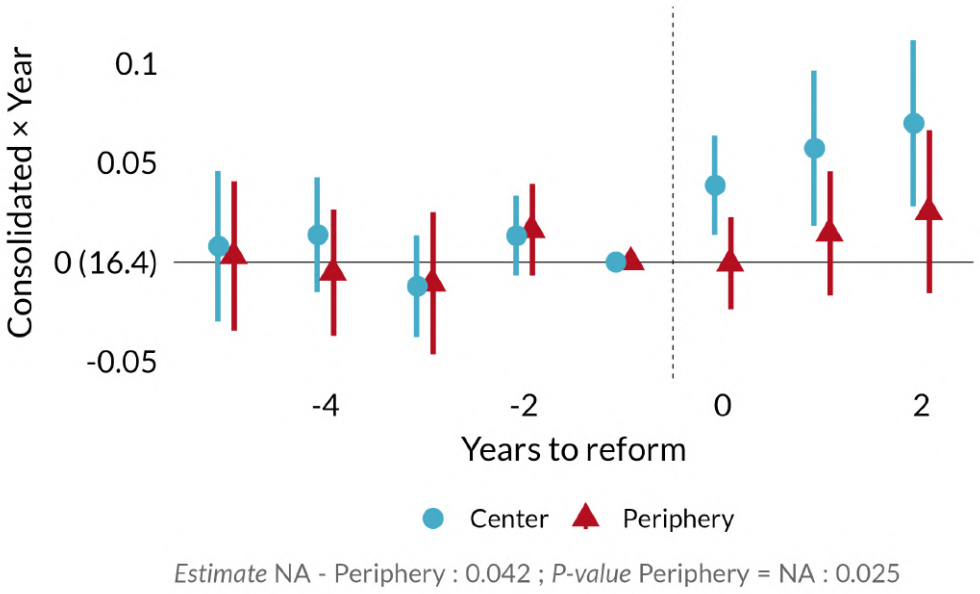
Notes. The figure presents estimates from Equation 5 (top panel) and from Equation 7 (bottom panel). Observations are at the municipality-year level. The dependent variable is the log of GDP (available up to 2021). The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Error bars represent 95% confidence intervals.

Figure 4 — Effect of consolidation on tax revenues



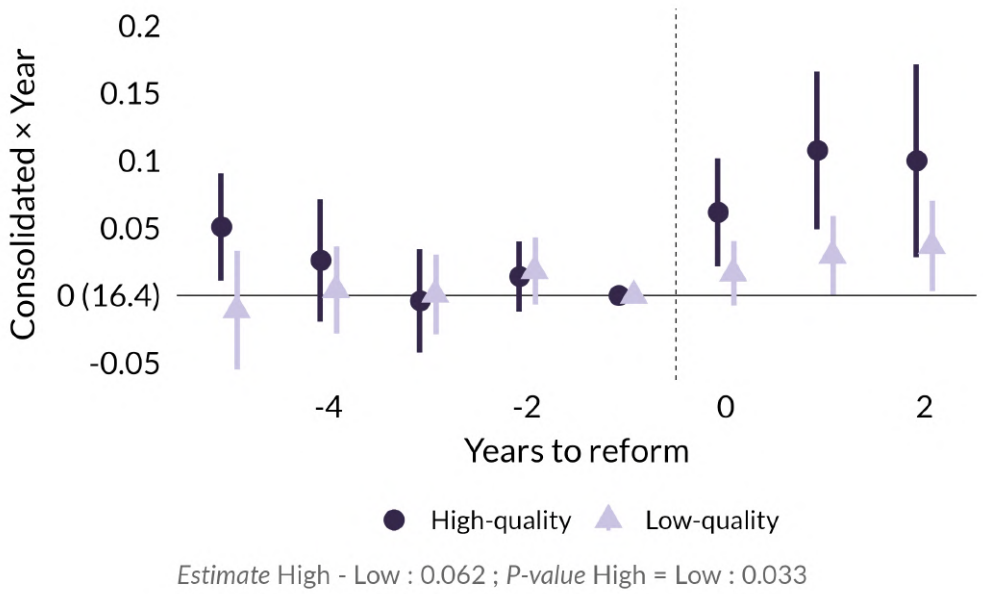
Notes. The figure presents estimates from Equation 5. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 5 — Effect of consolidation on tax revenues by differential exposure to reform



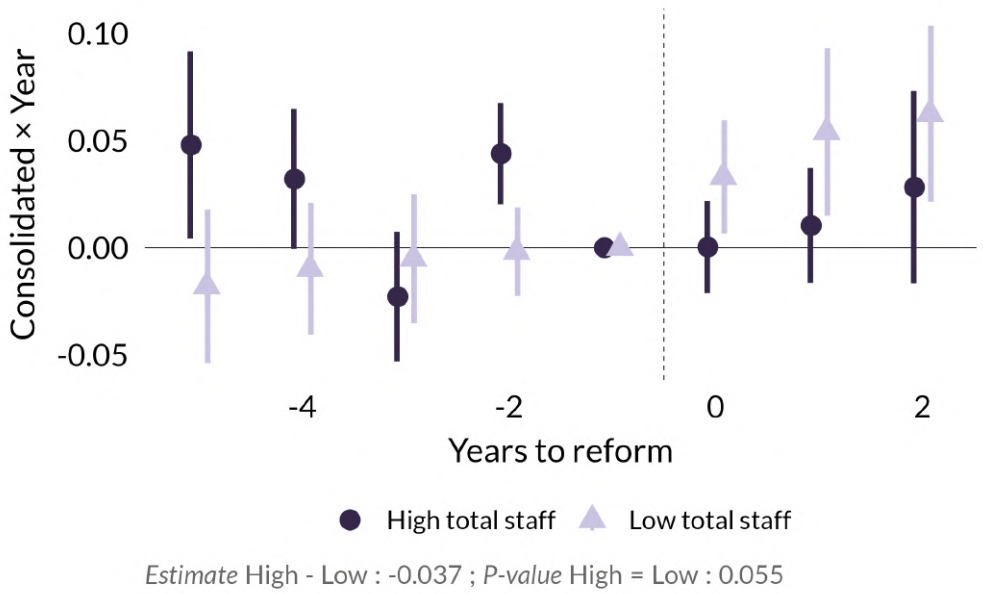
Notes. The figure presents estimates from Equation 7. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *periphery* (red triangles) and *center* (blue dots) municipalities. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. The difference between the post-reform coefficients and a p-value from a t-test on the difference being 0 is reported at the bottom of the figure. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 6 — Mechanism: managerial quality



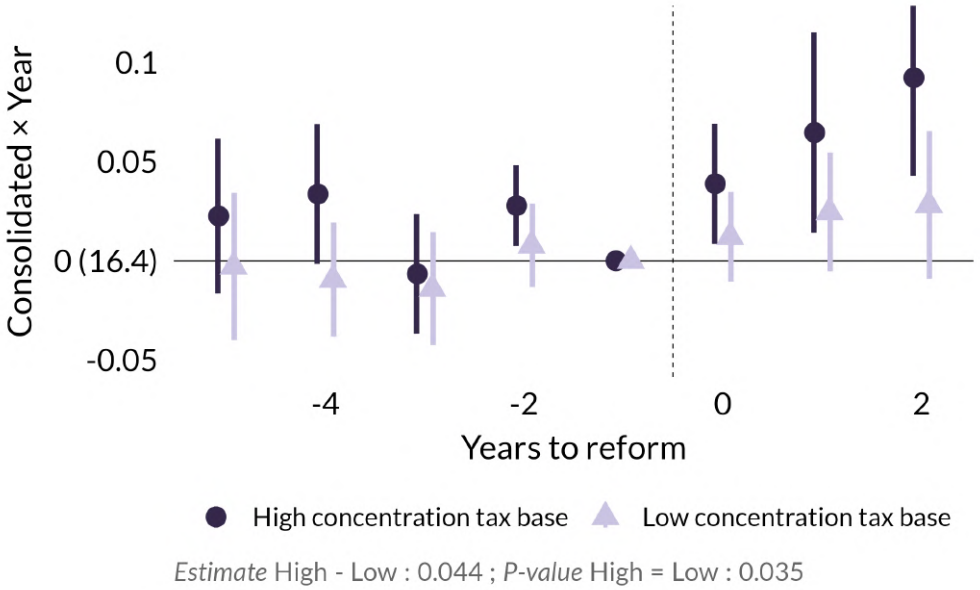
Notes. The figure presents estimates from Equation 10. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) quality managers in charge at the time of the reform. Managerial quality is defined based on age of first promotion to a managerial position. See Section 5.1 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. The sample excludes the 14 jurisdictions which experience managerial turnover in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 7 — Mechanism: congestion



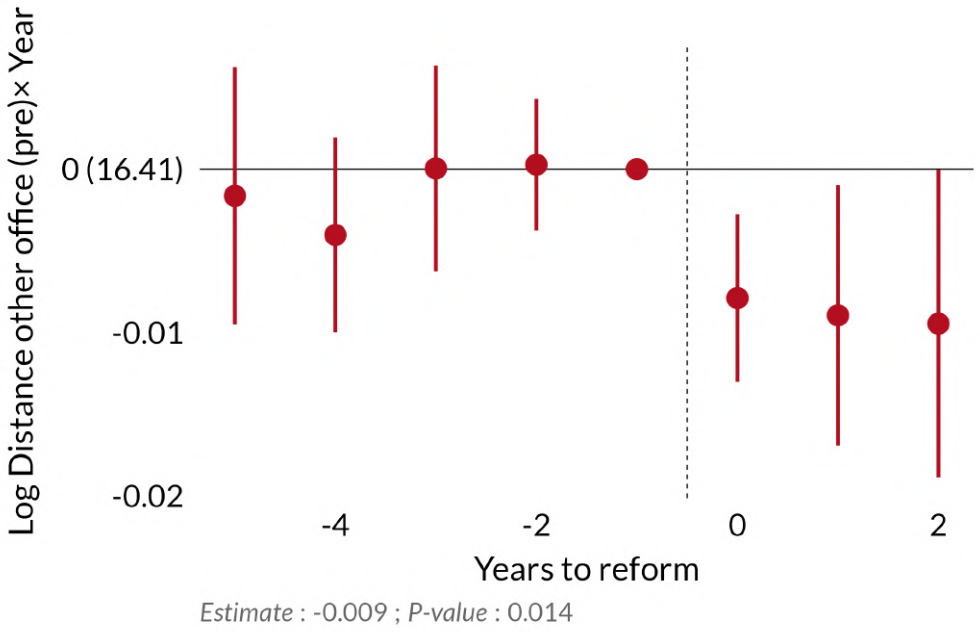
Notes. The figure presents estimates from Equation 5 separately for offices with low and high total staff size. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) pre-reform total staff. *High* total staff indicates that the share is in the top tercile of the distribution. See Section 5.1 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. The sample excludes the 14 jurisdictions which experience managerial turnover in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 8 — Mechanism: concentration tax base



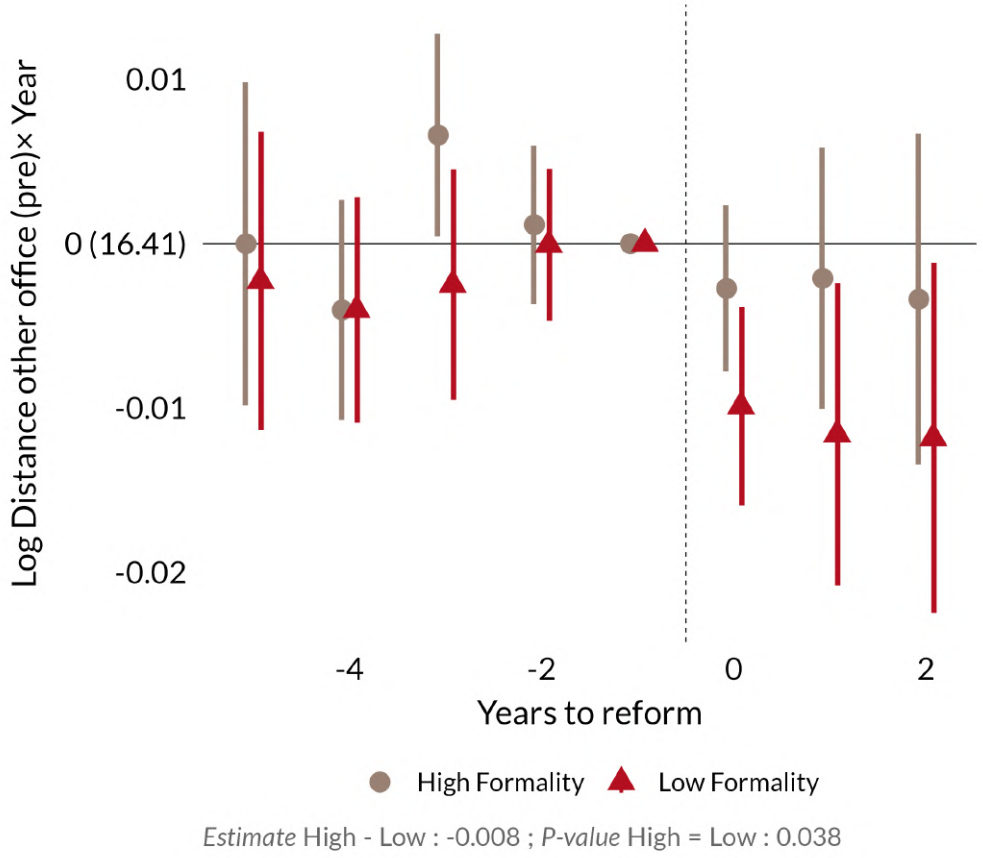
Notes. The figure presents estimates from Equation 11. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) concentration of the tax base. Concentration is measured as the share of the (pre-reform) GDP in the jurisdiction that comes from*center* municipalities. *High* concentration indicates that the share is in the top tercile of the distribution. See Section 5.2 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 9 — Mechanism: distance from office and spatial inequality in revenues



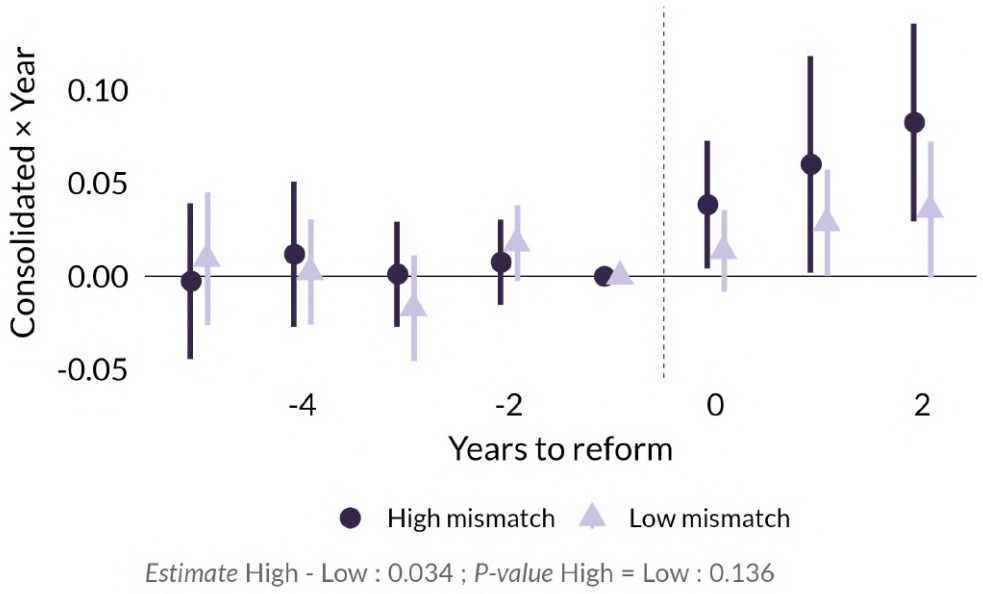
Notes. The figure presents estimates from Equation 12. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The sample include only municipalities in consolidated jurisdictions. I report estimates for the interaction between distance from the nearest other tax office in the same fiscal region before the reform and being a *periphery* municipality. The specification controls for a proxy of market access, the distance at baseline from the nearest tax office in another jurisdiction in any fiscal region. See Section 5.2 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 10 — Mechanism: distance from office, prevalence of third party reporting and spatial inequality in revenues



Notes. The figure presents estimates from Equation 12 augmented by interacting the treatment variables with indicators for high and low formality rate. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The sample include only municipalities in consolidated jurisdictions. I report estimates for the interaction between distance from the nearest other tax office in the same fiscal region before the reform and being a *periphery* municipality, interacted with indicators for municipalities with a *high* (dots) and *low* (triangles) formal rate. The specification controls for a proxy of market access, the distance at baseline from the nearest tax office in another jurisdiction in any fiscal region. See Section 5.2 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 11 — Mechanism: mismatch staff and tax base



Notes. The figure presents estimates from Equation 11. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) mismatch before the reform. Mismatch is measured as the difference between the share of the (pre-reform) GDP and of the total staff in the jurisdiction that comes from*center* municipalities. *High* mismatch indicates that the share is in the top tercile of the distribution. See Section 5.3 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

A — Appendix: Additional Tables and Figures

A.1. Additional tables

Table A1: Characteristics offices affected by the reform

	Periphery		Center	
	(1)	(2)	(3)	(4)
Constant	0.063	0.840**	0.376***	0.227
	(0.040)	(0.300)	(0.102)	(0.334)
Distance nearest office	-0.023*	-0.025	-0.021**	-0.032***
	(0.011)	(0.015)	(0.007)	(0.010)
< 100 inspectors	0.402***	0.340***	-0.270**	-0.268**
	(0.060)	(0.064)	(0.091)	(0.084)
Distance regional HQ		0.002		0.003**
		(0.002)		(0.001)
Avg. experience staff		-0.041**		0.009
		(0.015)		(0.018)
Nearest office has < 100 inspector			0.195***	0.179**
			(0.058)	(0.063)
Dep. Var. Mean	0.26	0.26	0.26	0.26
R^2	0.20	0.25	0.17	0.18
Clusters	10	10	10	10
Observations	91	91	91	91

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 nearest office:* distance (in minutes) from the nearest local office in the fiscal region. *Distance from reg. HQ:* distance (in minutes) from the regional headquarters of the fiscal region. *< 100 inspectors:* indicator equal to 1 if the number of tax agents deployed in the office in 2020 is less than 100. *Nearest office has < 100 inspector:* indicator equal to 1 if the number of tax agents deployed in the nearest office local office in the fiscal region in 2020 is less than 100. *Avg. experience staff:* average number of years staff in the offices have worked for tax authority. Sample does not include offices in Brasília, São Paulo, and Rio de Janeiro. Standard errors are clustered at the fiscal region level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A2: Characteristics of municipalities by exposure to reform

(a) *Consolidated* vs. *unaffected* municipalities

	Unaffected (N=2851)		Consolidated (N=2712)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Primary share (2010)	0.44	0.30	0.51	0.33	0.07***	0.01
Secondary share (2010)	0.20	0.10	0.20	0.10	0.00*	0.00
Tertiary share (2010)	0.51	0.12	0.52	0.13	0.00	0.00
Other share (2010)	0.05	0.04	0.04	0.03	-0.01***	0.00
Formal share (2010)	0.55	0.16	0.51	0.18	-0.04***	0.00
Population (2010)	38920.05	264235.09	29422.84	105942.99	-9497.21*	5350.54
Area	2108.30	7627.42	954.30	2148.75	-1154.00***	148.69
Nightlights	1723.09	9505.44	1251.97	3772.97	-471.12**	192.20
GDP	1407843.56	15362483.84	831698.40	3863030.25	-576145.16*	297123.93
Distance tax office (minutes)	261.18	1045.40	147.21	110.66	-113.97***	19.72
Distance other tax office (minutes)	335.39	1060.41	192.26	111.56	-143.13***	20.00
Federal tax collection (log)	16.24	1.76	16.00	1.73	-0.23***	0.05

(b) *Core* vs. *periphery* municipalities

	Center (N=1502)		Periphery (N=1210)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Primary share (2010)	0.51	0.32	0.52	0.35	0.01	0.08
Secondary share (2010)	0.20	0.10	0.20	0.09	-0.01	0.02
Tertiary share (2010)	0.51	0.12	0.52	0.13	0.01	0.02
Other share (2010)	0.04	0.03	0.04	0.04	0.00	0.00
Formal share (2010)	0.50	0.18	0.52	0.17	0.02	0.05
Population (2010)	36025.06	135217.06	21227.36	48445.92	-14797.70**	5800.59
Area	1003.17	2583.91	893.64	1434.37	-109.53	287.62
Nightlights	1486.49	4545.79	960.86	2472.33	-525.63*	266.24
GDP	989703.93	4632633.99	635562.61	2597766.77	-354141.32	247333.86
Distance tax office (minutes)	155.81	118.23	136.53	99.49	-19.27	26.87
Distance other tax office (minutes)	192.52	108.52	191.94	115.27	-0.58	31.64
Federal tax collection (log)	16.10	1.80	15.88	1.63	-0.21	0.33

Notes. Observations are at the municipality level. Each table reports mean and standard deviation for the two municipality groups in year 2016, as well as the differences between group means. *Consolidated* indicator equal to 1 for municipalities part of consolidated jurisdiction. *Periphery* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Core* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office that was closed in 2020. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Characteristics of municipalities by exposure to reform, after matching

(a) *Consolidated* vs. *unaffected* municipalities

	Unaffected (N=2547)		Consolidated (N=2403)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Primary share (2010)	0.47	0.31	0.51	0.32	0.05***	0.01
Secondary share (2010)	0.20	0.10	0.20	0.10	0.00	0.00
Tertiary share (2010)	0.51	0.12	0.51	0.12	0.00	0.00
Other share (2010)	0.05	0.04	0.04	0.03	-0.01***	0.00
Formal share (2010)	0.54	0.17	0.51	0.18	-0.03***	0.01
Population (2010)	21705.80	41612.71	22721.92	37687.26	1016.13	1076.90
Area	1943.07	7589.76	959.21	2101.46	-983.85***	165.72
Nightlights	946.16	2053.32	976.16	1877.19	30.00	53.64
GDP	517627.03	1300127.55	516105.46	1254351.96	-1521.57	34514.56
Distance tax office (minutes)	292.82	1186.38	147.37	110.45	-145.45***	26.43
Distance other tax office (minutes)	360.67	1199.56	193.77	111.01	-166.90***	26.75
Federal tax collection (log)	15.97	1.55	15.99	1.60	0.02	0.05

(b) *Core* vs. *periphery* municipalities

	Center (N=1338)		Periphery (N=1065)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Primary share (2010)	0.52	0.32	0.51	0.33	-0.01	0.07
Secondary share (2010)	0.20	0.10	0.19	0.09	-0.01	0.02
Tertiary share (2010)	0.51	0.12	0.52	0.13	0.01	0.02
Other share (2010)	0.04	0.03	0.04	0.04	0.00	0.00
Formal share (2010)	0.50	0.18	0.52	0.17	0.02	0.05
Population (2010)	24748.96	38865.50	20175.29	36009.24	-4573.67	3087.74
Area	991.40	2489.71	918.78	1475.63	-72.62	285.93
Nightlights	1061.54	1942.58	868.90	1786.79	-192.63	165.12
GDP	543596.73	1294624.62	481567.14	1201557.43	-62029.58	109278.74
Distance tax office (minutes)	157.63	118.05	134.48	98.63	-23.16	26.79
Distance other tax office (minutes)	195.60	108.81	191.47	113.72	-4.13	30.97
Federal tax collection (log)	16.03	1.64	15.93	1.55	-0.10	0.30

Notes. Observations are at the municipality level. Each table reports mean and standard deviation for the two municipality groups in year 2016, as well as the differences between group means. The sample is restricted to municipalities in the common support of the matching variables (baseline approach) and the mean is computed using the matching weights. *Consolidated* indicator equal to 1 for municipalities part of consolidated jurisdiction. *Periphery* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Core* indicator equal to 1 for municipalities whose jurisdiction tax office absorbs municipalities previously served by an office that was closed in 2020. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: Effects of the reform on tax revenues, by tax type

	SS Tax (log) (1)	Inc. Tax (log) (2)	Other Tax (log) (3)	SS Tax (log) (4)	Inc. Tax (log) (5)	Other Tax (log) (6)
Consolidated × Post	0.040**	0.022	0.014			
	(0.019)	(0.026)	(0.016)			
Periphery × Post				0.010	0.025	0.015
				(0.020)	(0.030)	(0.020)
Core × Post				0.063**	0.020	0.012
				(0.024)	(0.027)	(0.018)
Municipality	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
P-value: Core = Periphery				0.042	0.82	0.86
R^2	0.98	0.97	0.98	0.98	0.97	0.98
Clusters	136	136	136	136	136	136
Observations	54,450	54,439	54,438	54,450	54,439	54,438
Dep. Var. Mean	27,462	12,500	24,129	27,462	12,500	24,129

Notes. The table presents estimates from Equation 6 (columns 1 to 3) and from Equation 8 (columns 4 to 6). Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues from social security contributions (columns 1 and 4), income tax (columns 2 and 5), other taxes and contributions (columns 3 and 6). *Post* indicator equal to 1 for years after the reform (2021-2023). *Consolidated* indicator equal to 1 for municipalities part of consolidated jurisdiction. *Periphery* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Core* 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 year. *Controls:* area (log), population (log), agricultural, secondary, and tertiary share of employment, income (log)—all from 2010 census and interacted with year fixed effects. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the mesoregion level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A5: Effects of the reform on personnel structure

	N. Agents (log) (1)	N. Frontline (log) (2)	N. Middle managers (log) (3)	N. Top managers (log) (4)	Frontline / Top managers (5)	Middle managers / Top managers (6)
Consolidated × Post	0.026	0.010	0.093	-0.205*	69.184*	10.127**
	(0.036)	(0.041)	(0.069)	(0.110)	(41.450)	(4.207)
2021 Tax Jur.	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
R^2	0.98	0.97	0.93	0.93	0.83	0.78
Clusters	67	67	67	67	67	67
Observations	737	737	737	718	718	718
Dep. Var. Mean	4.8	4.6	2.9	0.15	154.8	19.8

Notes. The table presents estimates from Equation 9. Observations are at the post-reform tax jurisdiction-year level. The dependent variable are: total number of tax agents (column 1), number of frontline agents (column 2), number of middle managers (column 3), number of top managers (column 4), number of frontline agents per top manager (column 5), number of middle managers per top manager (column 6). *Post* indicator equal to 1 for years after the reform (2021-2023). *Consolidated* indicator equal to 1 for consolidated jurisdictions. *Fixed effects:* jurisdiction and year. Standard errors are clustered at the post-reform tax jurisdiction level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

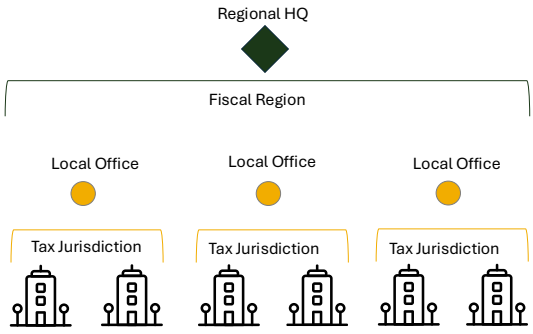
Table A6: Effects of the reform on personnel costs

	Tot. Wages (log)	Frontline Wages (log)	Middle manager Wages (log)	Top manager Wages (log)
	(1)	(2)	(3)	(4)
Consolidated × Post	0.020	0.007	-0.037	-0.174
	(0.042)	(0.048)	(0.123)	(0.113)
2021 Tax Jur.	✓	✓	✓	✓
Year	✓	✓	✓	✓
R^2	0.98	0.97	0.88	0.93
Clusters	67	67	67	67
Observations	737	737	737	717
Dep. Var. Mean	17.1	16.9	15.7	13.0

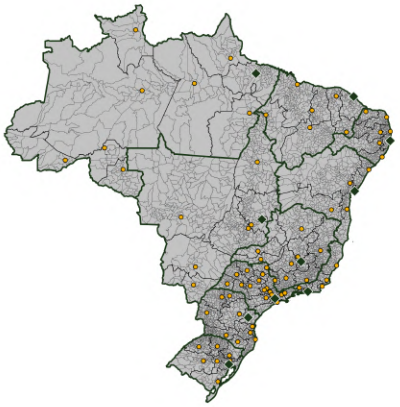
Notes. The table presents estimates from Equation 9. Observations are at the post-reform tax jurisdiction-year level. The dependent variable is the wage bill for: all tax agents (column 1), frontline agents (column 2), middle managers (column 3), top managers (column 4). *Post* indicator equal to 1 for years after the reform (2021-2023). *Consolidated* indicator equal to 1 for consolidated jurisdictions. *Fixed effects:* jurisdiction and year. Standard errors are clustered at the post-reform tax jurisdiction level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

A.2. Additional figures

Figure A1 — The spatial organization of the federal tax authority



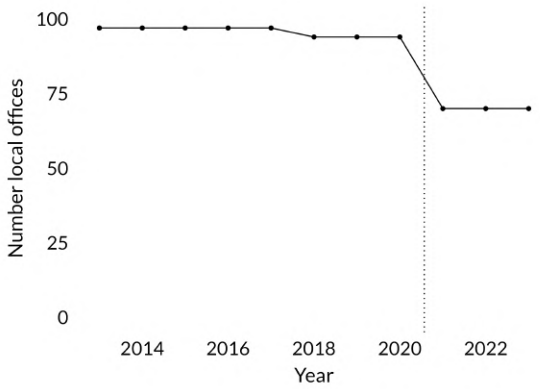
(a) Schematic representation



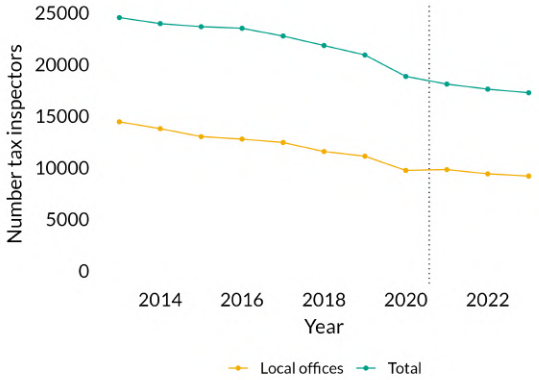
(b) Map of offices and tax jurisdictions before the reform

Notes. Panel (a) illustrates the spatial organization of the tax authority schematically. There are 10 fiscal regions. Each region is managed by a regional headquarters (green diamond). A fiscal region may span several states. Each fiscal region is further partitioned in tax jurisdictions. As of the beginning of 2020 there are 94 tax jurisdictions. Each jurisdiction is managed by a local office (yellow dots) and spans multiple municipalities (stylized towns at bottom) Panel (b) shows the locations of regional headquarters and local tax offices operating in Brazil as of the beginning of 2020. The borders of the fiscal regions are indicated by green think lines and their regional headquarters are marked with green diamonds. The borders of the tax jurisdictions are indicated with black lines. Local offices are marked with yellow dots. Thin white lines indicate the boundaries of the 5,563 municipalities.

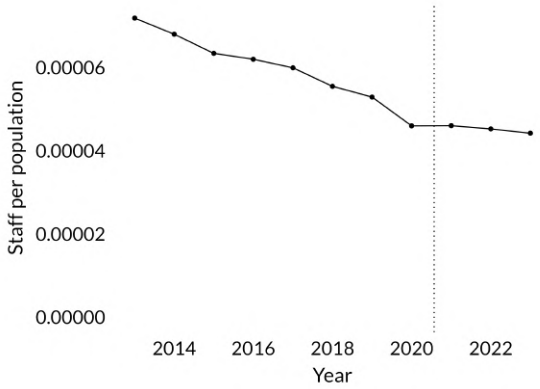
Figure A2 — The evolution of the tax authority



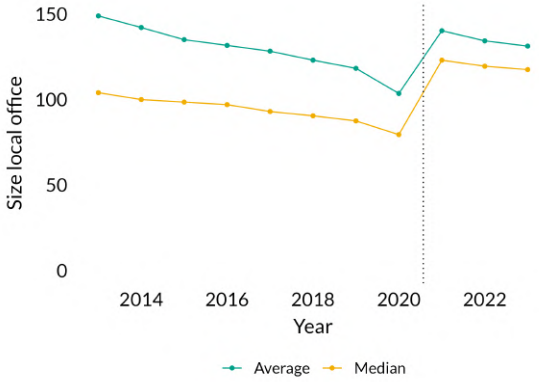
(a) Number of local tax offices



(b) Number of tax agents



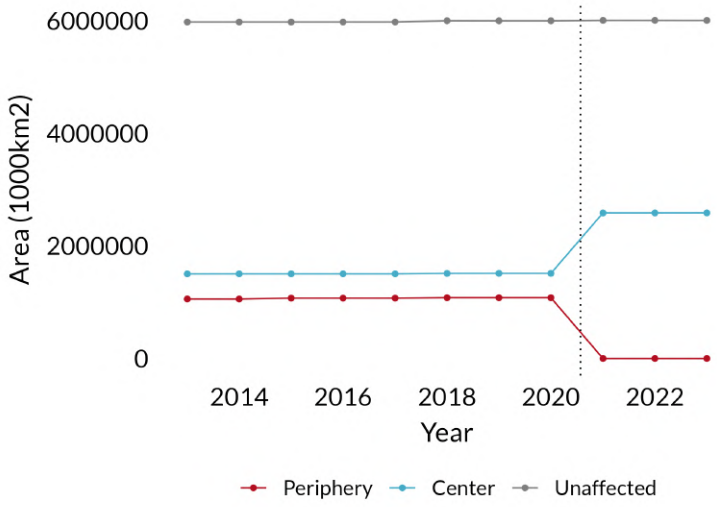
(c) Staff ratio



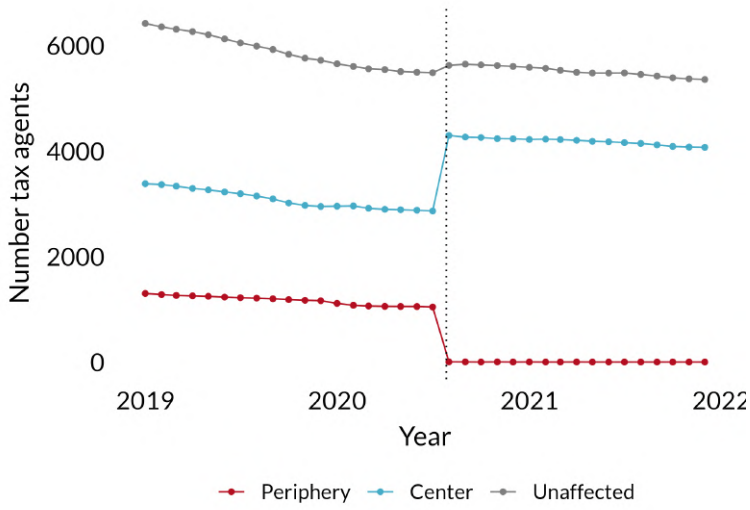
(d) Size of local tax offices

Notes. These graphs describe the evolution of the tax authority over time. The 2020 reform shuts 24 local offices (top-left panel). The number of tax agents decline over time (top-right panel). This is leads to a decline in the number of tax agents in local office per thousands inhabitants (bottom-left panel). At the same time, the average and median size of local offices also declines before bouncing back after the reform (bottom-right panel).

Figure A3 — The reform



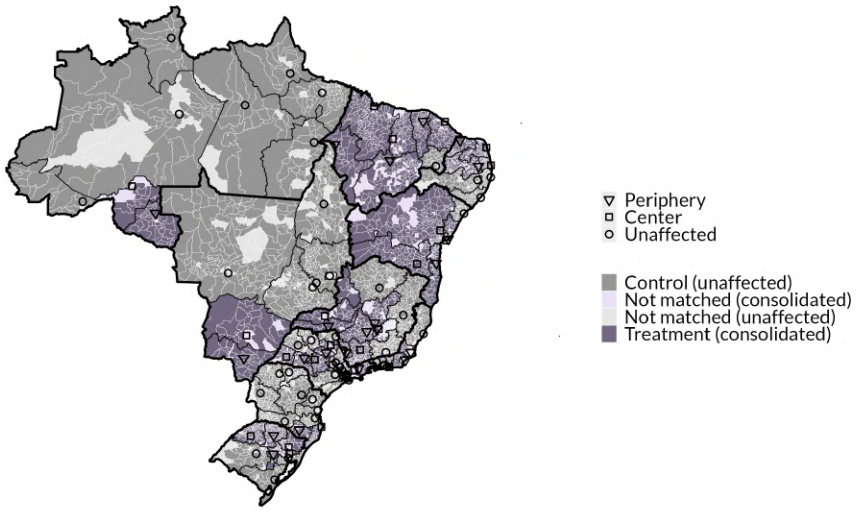
(a) Municipalities served by closing offices are assigned to expanded offices



(b) 92% tax agents from closing offices are reallocated to expanded offices

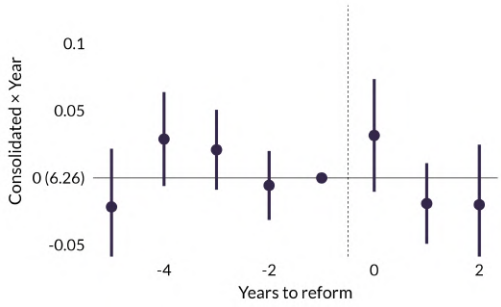
Notes. These graphs illustrate the reform. Municipalities are assigned to the jurisdiction of the expanded offices. The reform happens on July 27, 2020. From the month of August, 92% of tax agents from closing offices are assigned to the expanded offices

Figure A4 — Map of municipalities by matching status and exposure to the reform

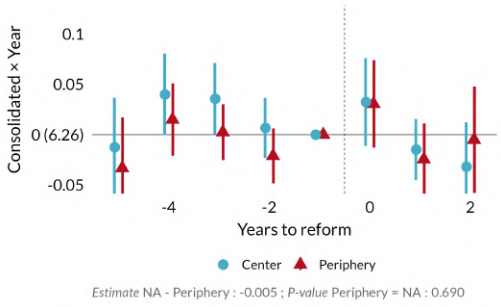


Notes. These figures display which municipalities are matched and how they are exposed to the reform. Treatment municipalities are the matched *consolidated* (dark purple), control municipalities are the matched *unaffected* (dark gray). I also report non-matched *consolidated* (light purple) and non-matched *unaffected* (light gray) municipalities. Thick black borders indicate the boundaries of pre-reform tax jurisdictions, and black shapes the corresponding local offices. Offices closed by the reform are marked with triangles, the ones expanded by the reform are marked with squares, and the ones unaffected are markers with dots. Thin white lines indicate the boundaries of municipalities.

Figure A5 — Matched treated and control are on similar trends in economic activity (nightlights)



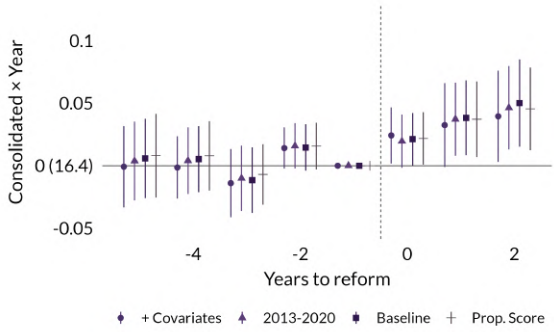
(a) *Consolidated* and *unaffected*



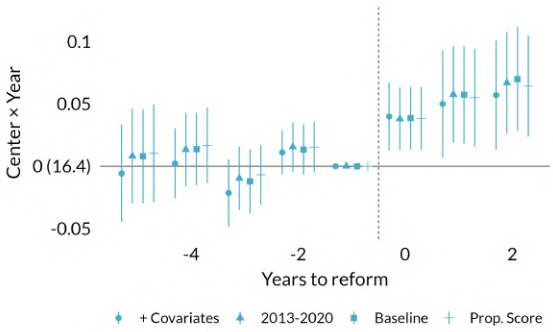
(b) *Core, periphery* and *unaffected*

Notes. The figure presents estimates from Equation 5 (left panel) and from Equation 7 (right panel). Observations are at the municipality-year level. The dependent variable is the log of nightlights. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Error bars represent 95% confidence intervals.

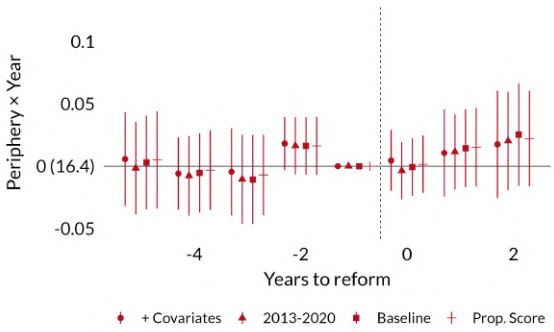
Figure A6 — Effect of consolidation on tax revenues: alternative matching procedure



(a) Consolidated



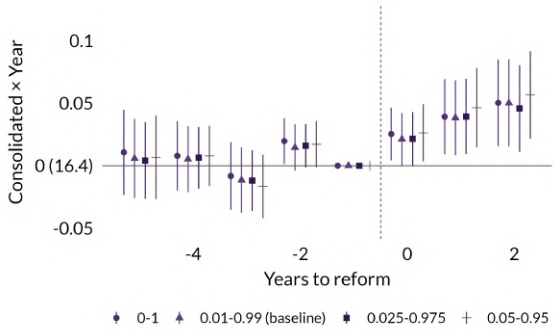
(b) Core



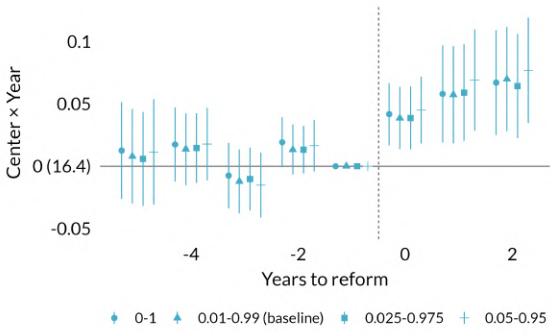
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c). Coefficients in panels (b) and (c) are estimated in the same regression but reported separately to make visualization easier. The dependent variable is the log of federal tax revenues. Observations are at the municipality-year level. I report results using different matching procedures: matching on trajectory of GDP in the year 2016-2020 (baseline), matching on trajectory of GDP in the year 2016-2020 and a set of municipality covariates in 2010 (area, population, primary, secondary, and tertiary share of employment, income), matching on trajectory of GDP in the year 2013-2020, matching on trajectory of GDP in the year 2016-2020 but using propensity score (estimated via logit) and inverse probability weighting. Standard errors are clustered at the mesoregion level. See Section 4.1.1 for more details.

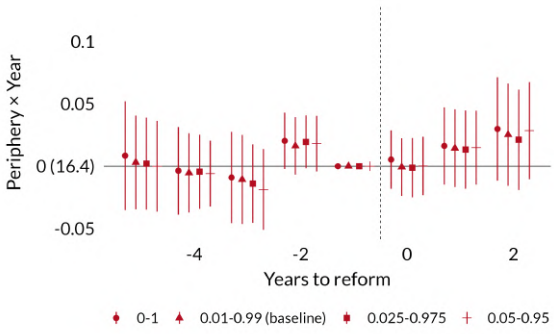
Figure A7 — Effect of consolidation on tax revenues: alternative common support before matching



(a) Consolidated



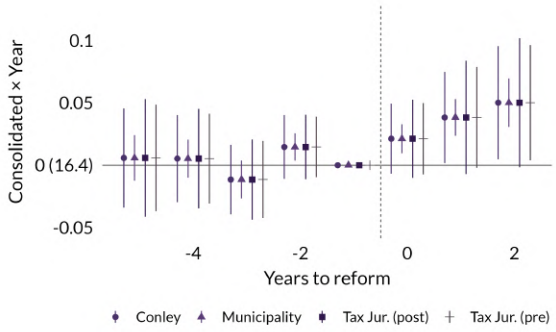
(b) Core



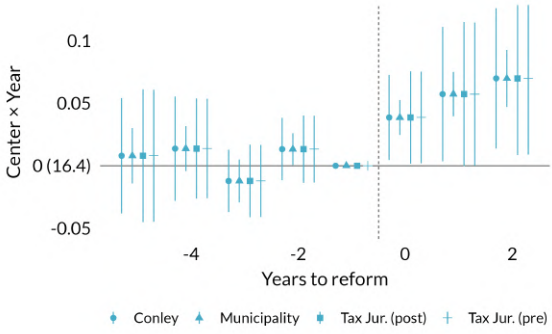
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c). Coefficients in panels (b) and (c) are estimated in the same regression but reported separately to make visualization easier. The dependent variable is the log of federal tax revenues. Observations are at the municipality-year level. I report results imposing alternative common support restrictions before matching: trimming 1% (top 1% and bottom 1%) of the matching variables' distribution (0.01-9.99, baseline), trimming top and bottom 2.5% (0.025-0.975), trimming top and bottom 5% (0.05-0.95), and not trimming (0-1). Standard errors are clustered at the mesoregion level. See Section 4.1.1 for more details.

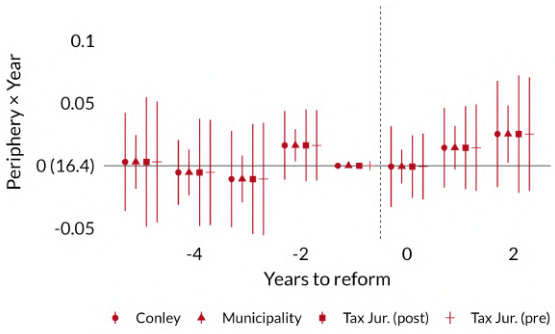
Figure A8 — Effect of consolidation on tax revenues: alternative inference



(a) Consolidated



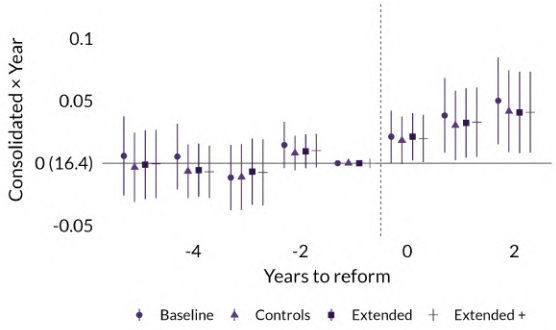
(b) Core



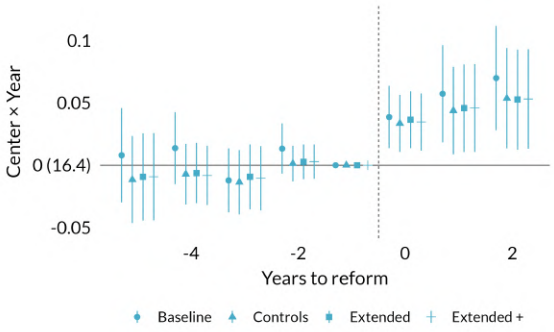
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c). Coefficients in panels (b) and (c) are estimated in the same regression but reported separately to make visualization easier. The dependent variable is the log of federal tax revenues. Observations are at the municipality-year level. I report alternative inference procedures: clustering at the *municipality* level, clustering at the *pre-reform tax jurisdiction* level, clustering at the *post-reform tax jurisdiction* level, and standard errors robust to spatial correlation using Conley 1999 standard errors with a 1000km cutoff. See Section 4.1.1 for more details.

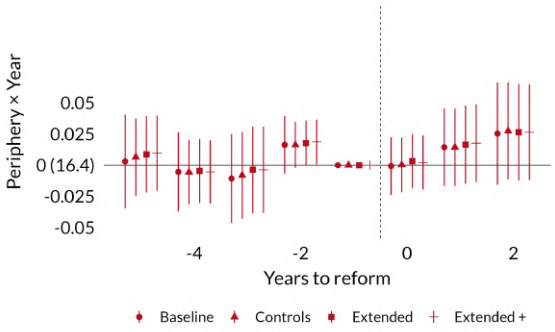
Figure A9 — Effect of consolidation on tax revenues: controls



(a) Consolidated



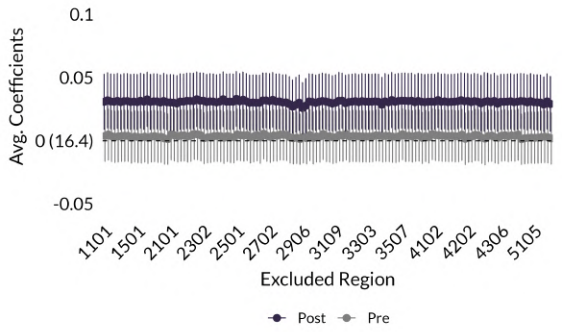
(b) Core



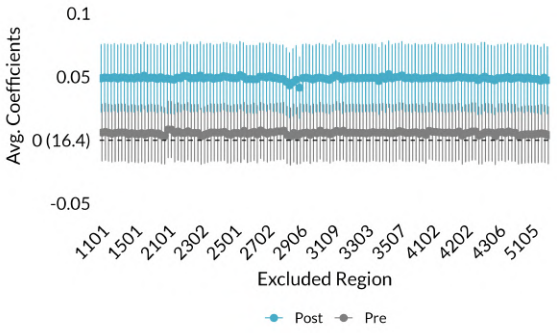
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c). Coefficients in panels (b) and (c) are estimated in the same regression but reported separately to make visualization easier. The dependent variable is the log of federal tax revenues. Observations are at the municipality-year level. *Baseline:* no controls. *Controls:* area (log), population (log), primary, secondary, and tertiary employment shares, and income (log)—all from the 2010 census and interacted with year fixed effects. *Extended:* all the above plus travel from the nearest office in any fiscal region (proxy market access) and formality rate from the 2010 census, interacted with year fixed effects. *Extended +:* all the above plus (log) nightlight in a municipality-year. Standard errors are clustered at the mesoregion level. See Section 4.1.1 for more details.

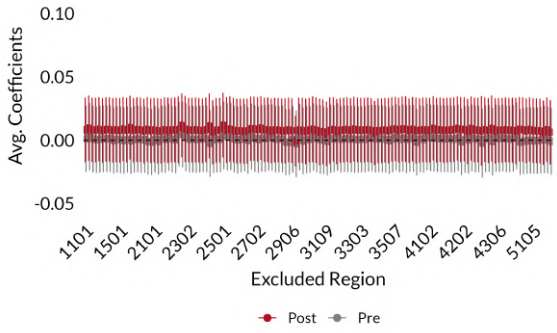
Figure A10 — Effect of consolidation on tax revenues: leave one out



(a) Consolidated



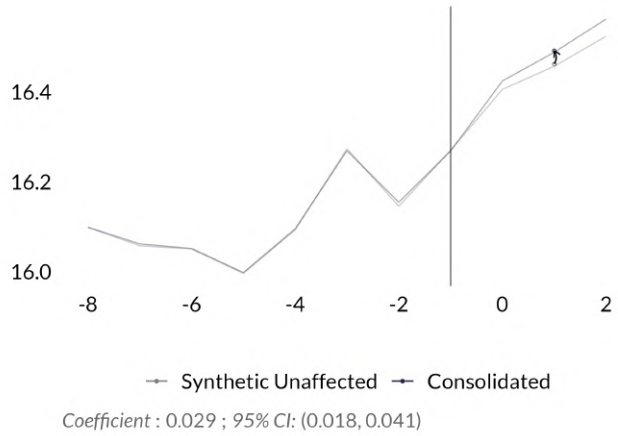
(b) Core



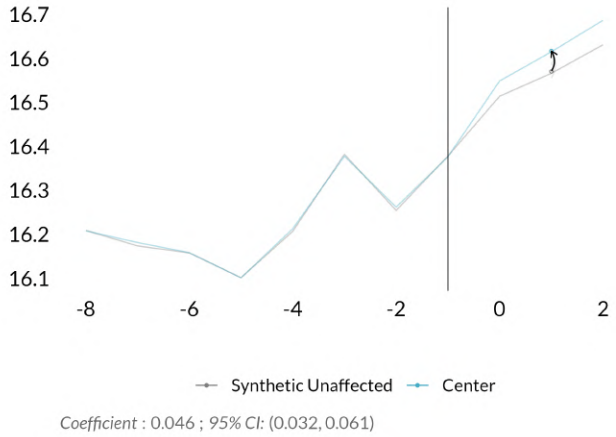
(c) Periphery

Notes. The plot reports the average and the 95% confidence interval for the coefficients in the pre-period (gray dots) and in the post-period (colored dots) for the interaction between year and (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c). Each pair of coefficients is estimated by leaving out one of the 136 mesoregions in the sample at a time. Coefficients in panels (b) and (c) are estimated in the same regression but reported separately to make visualization easier. The dependent variable is the log of federal tax revenues. Observations are at the municipality-year level. Standard errors are clustered at the mesoregion level. See Section 4.1.1 for more details.

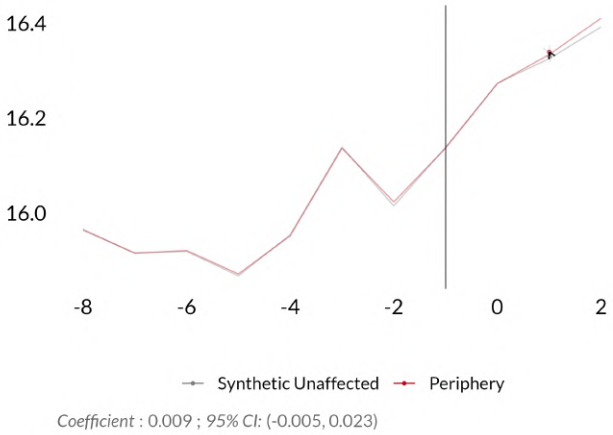
Figure A11 — Effect of consolidation on tax revenues: synthetic DID



(a) Consolidated



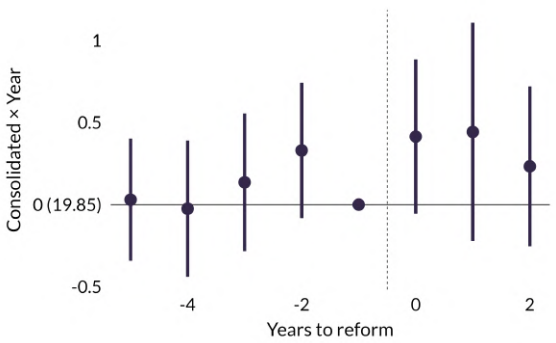
(b) Core



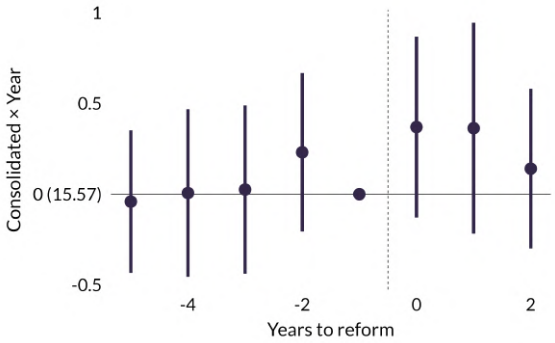
(c) Periphery

Notes. The plot reports the evolution of (log) tax revenues for (i) *consolidated* (panel a), (ii) *center* (panel b), and (iii) *periphery* (panel c), and the synthetic unaffected built using the approach in Arkhangelsky et al. (2021). The three estimates are estimated separately. The black arrow display the estimated effect of the policy; the estimated effect is also reported at the bottom of the figure. The gray arrows display the 95% confidence interval for the synthetic unaffected. Observations are at the municipality-year level. P-values are computed via bootstrap (1,000 replications).

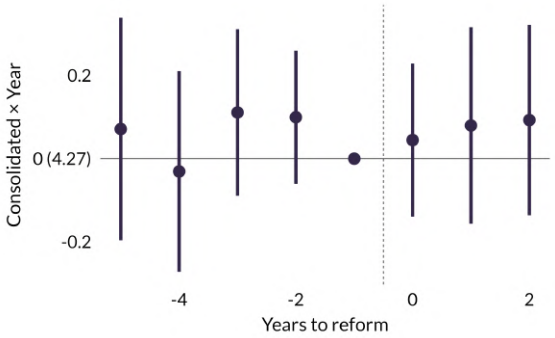
Figure A12 — Effect of consolidation on tax audits



(a) Tot. Amount Audit



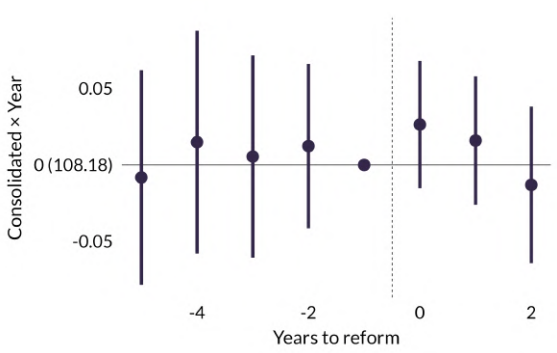
(b) Avg. Amount Audit



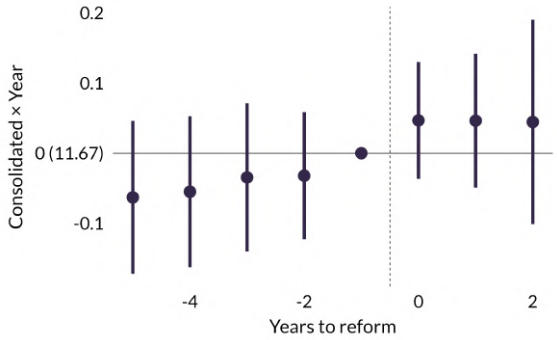
(c) N. audits

Notes. The plots report estimated coefficients and 95% confidence intervals for the interaction between year and *consolidated*. Observations are at the post-reform tax jurisdiction-year level. The dependent variable is the log of total amount of tax audit (panel a), the log of average amount of an audit (panel b), and the log of number of audits carried out (panel c). *Fixed effects:* jurisdiction and year. Standard errors are clustered at the tax jurisdiction level.

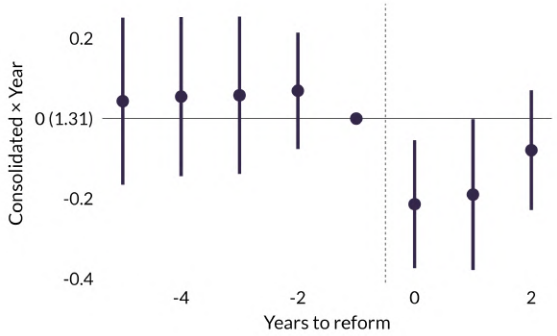
Figure A13 — Effect of consolidation on size personnel



(a) Frontline



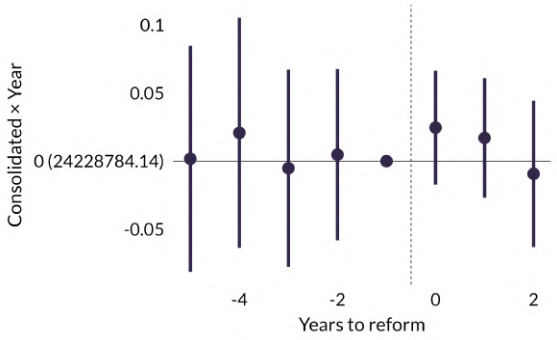
(b) Middle managers



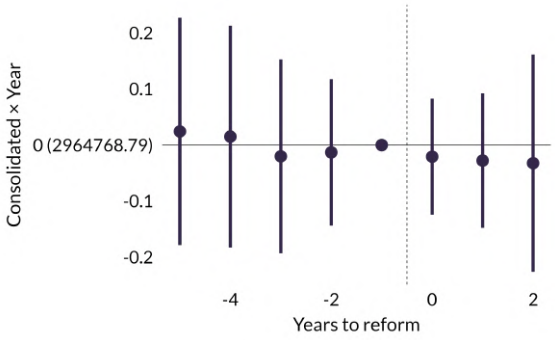
(c) Top managers

Notes. The plots report estimated coefficients and 95% confidence intervals for the interaction between year and *consolidated*. Observations are at the post-reform tax jurisdiction-year level. The dependent variables are the (log) number of frontline agents (panel a), middle-managers (panel b), and top managers (panel c). *Fixed effects:* jurisdiction and year. Standard errors are clustered at the tax jurisdiction level.

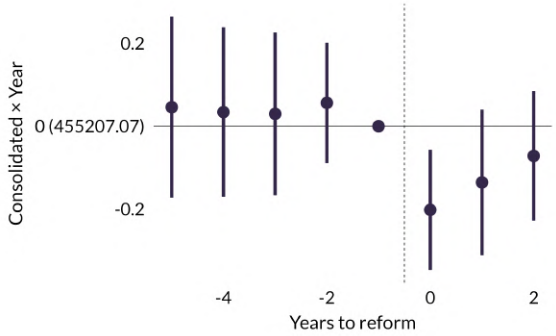
Figure A14 — Effect of consolidation on personnel cost



(a) Frontline



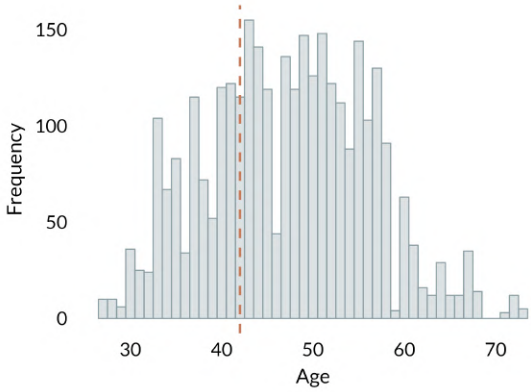
(b) Middle managers



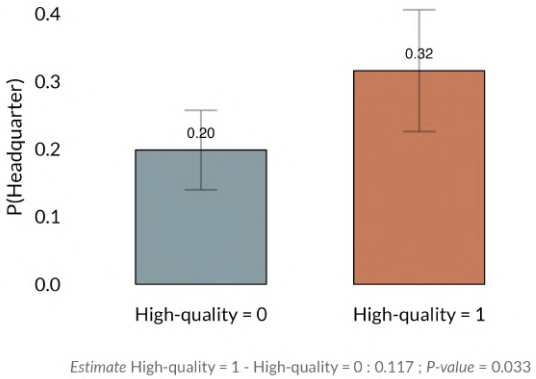
(c) Top managers

Notes. The plots report estimated coefficients and 95% confidence intervals for the interaction between year and *consolidated*. Observations are at the post-reform tax jurisdiction-year level. The dependent variables are the (log) wage bill for frontline agents (panel a), middle-managers (panel b), and top managers (panel c). *Fixed effects:* jurisdiction and year. Standard errors are clustered at the tax jurisdiction level.

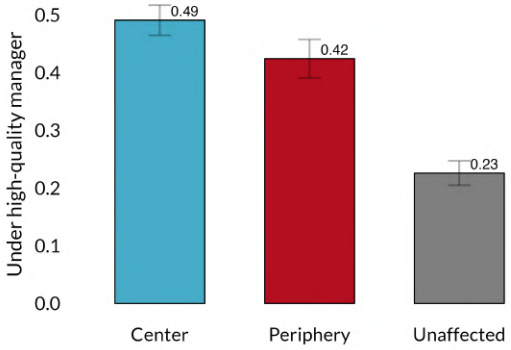
Figure A15 — Definition and validation high-quality manager measure



(a) Age at first promotion



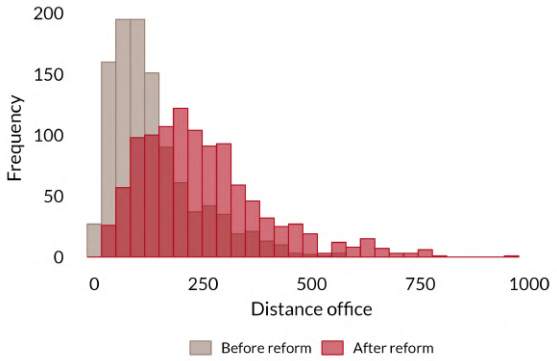
(b) Validation measure



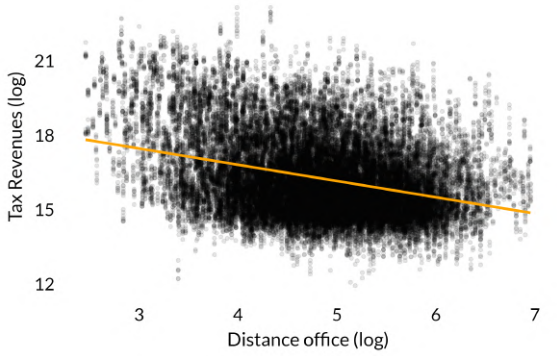
(c) Exposure to high-quality managers

Notes. This figure displays the definition and validation of the high-quality manager measure. The top left panel displays the distribution of ages at which tax agents are promoted to manager level 4. This managerial level is the first at which tax agents could oversee a local office. The vertical dashed line represents the 1st tercile in the distribution, 42 years old. I define *High-quality*$= 1$ for those tax agents who were younger than 42 at first promotion, and $= 0$ otherwise. To validate the measure, the top right panel displays the probability that a tax agent gets deployed to either the central or the regional headquarters at some point in their career, separately for the two groups. The estimated difference and a p-value for the difference being equal to 0 is reported at the bottom of the figure. The sample in both panels is restricted to tax agents who were deployed in a low-tier or local office at the moment of first promotion and excludes all managers who oversaw a local office in the period 2016-2023. The latter exclusion allows me to define and validate the quality proxy on an out-of-sample group and then apply it to the managers active in 2016-2023. The bottom panel displays the share of municipalities overseen by a high-quality manager in 2019 by exposure to the reform.

Figure A16 — Consolidation, distance from tax office, and tax revenues



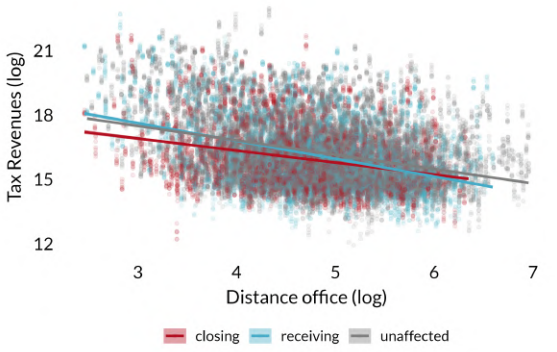
(a) Distance from tax office for *periphery* municipalities



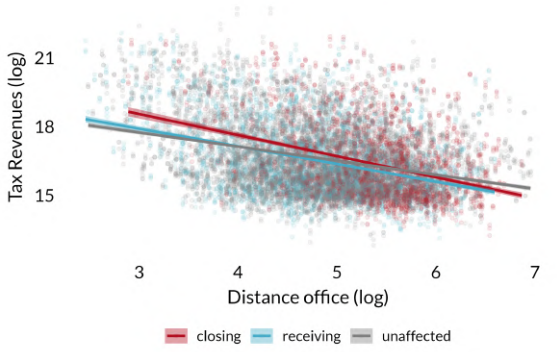
(b) Distance from tax office and tax revenues

Notes. The left panel shows the distribution of the travel distance (in minutes) from the jurisdiction tax office for *periphery* municipalities, before and after the reform. The right panel reports the relationship between tax revenues and the travel distance (in minutes) from the jurisdiction tax office. Each dot is a municipality-year observation. I also report a linear fit (solid line) with 95% confidence interval (shaded area).

Figure A17 — Distance from tax office and tax revenues by exposure to the reform



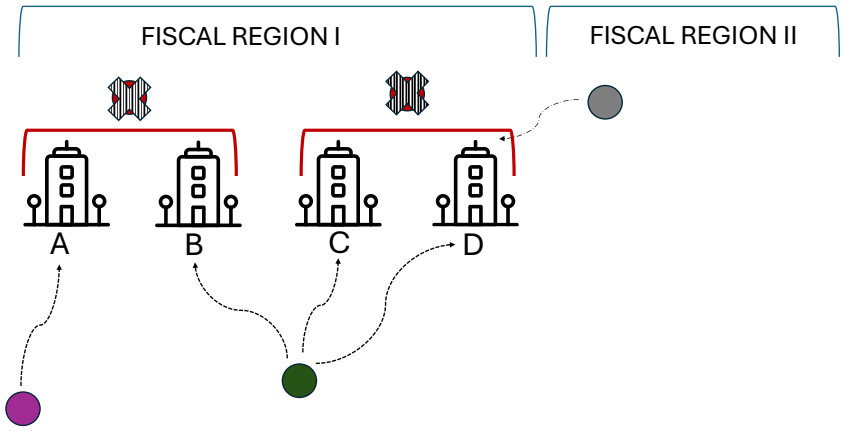
(a) Pre-reform



(b) Post-reform

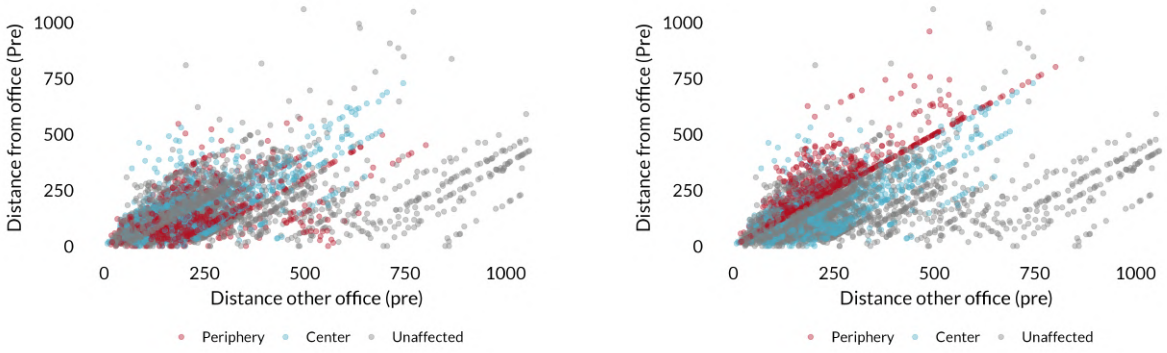
Notes. This figure reports the relationship between tax revenues and the travel distance (in minutes) from the jurisdiction tax office. Each dot is a municipality-year observation. It also shows the best linear fit (solid line) with 95% confidence interval (shaded area) separately for the three groups of municipalities based on their exposure to the reform. The left panel reports data for the pre-reform period, whereas the right panel reports data for the post-reform period,

Figure A18 — Empirical strategy for distance: schematic illustration



Notes. Schematic illustration of the variation underlying the 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 approach leverages the distance (dashed arrow) from the nearest office (at baseline) 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 nearest 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 variable is the one from the nearest office in the same fiscal region (green dot).

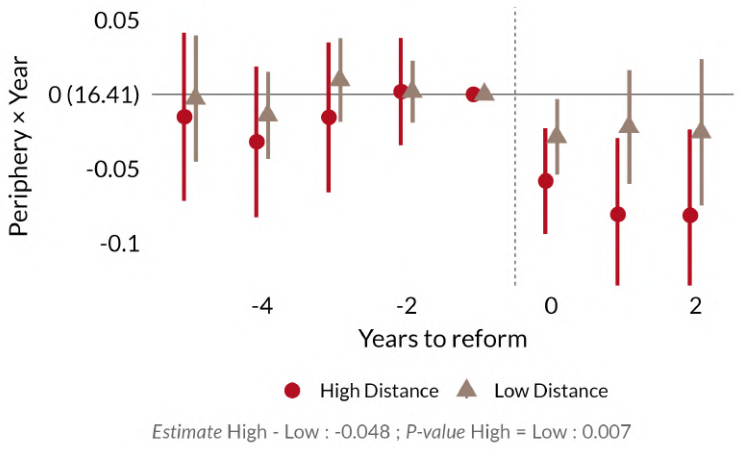
Figure A19 — Empirical strategy for distance



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

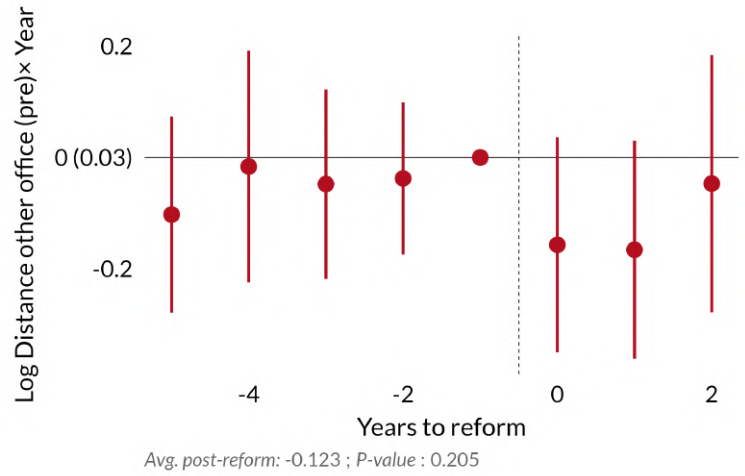
Notes. Observations are at the municipality level. The variable on the horizontal axis is the distance from the nearest office in a different tax jurisdiction but in the same fiscal region (at baseline). The variable on the vertical axis is the distance from the jurisdiction tax office, before (left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

Figure A20 — Mechanism: distance from office and spatial inequality in tax revenues



Notes. The figure presents estimates from Equation 13. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The sample include only municipalities in consolidated jurisdictions. I report estimates for *periphery* municipalities at *high distance* (dots) and *low distance* (triangles) from the nearest other tax office before the reform. See Section 5 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure A21 — Mechanism: distance from office and spatial inequality in tax inspections



Notes. The figure presents coefficients from Equation 12 estimated using a Poisson model. Observations are at the municipality-year level. The dependent variable is the number of physical tax inspections, proxied with the number of visits by tax agents in the auditing department. The sample include only municipalities in consolidated jurisdictions. I report estimates for the interaction between distance from the nearest other tax office in the same fiscal region before the reform and being a *periphery* municipality. The specification controls for a proxy of market access, the distance at baseline from the nearest tax office in another jurisdiction in any fiscal region. See Section 5.2 for definition of the variables. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable in the year of the reform. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.