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 enforcement and managerial 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 allocation of limited resources (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 consolidated 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 *core* 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. *Core* 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 (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 driving the reform's effects. The objective is to understand why consolidation increased tax revenues on average and why it led to a divergence in tax enforcement within consolidated jurisdictions. I first explore whether the increase in revenues is larger in jurisdictions where the reform allowed more efficient utilization of enforcement and managerial resources. If more efficient utilization involves reallocating staff effort from *periphery* to *core* municipalities, this mechanism can explain both the average increase and the divergence. Second, I test whether the divergence is also due to the reform making tax enforcement harder in *periphery* municipalities. In particular, I examine whether increased distance between tax offices and *periphery* municipalities weakens tax agents' ability to monitor these areas.

I show that the effects of consolidation are stronger in jurisdictions where it enables more efficient utilization of staff resources. After the reform, expanded offices have larger staff and can decide how to allocate staff effort over a wider region, which includes both *core* municipalities and the *periphery* municipalities that merged in after the reform. This suggests that consolidation may be particularly beneficial in jurisdictions where the tax base is concentrated in *core* municipalities, as it allows reallocating staff effort toward areas with a larger tax base, thus improving the targeting of enforcement resources. To test this hypothesis, I use average GDP in the years before the reform as a proxy for the tax base and show that the increase in tax revenues is stronger in jurisdictions in which the tax base is concentrated in *core* municipalities. This suggests that the organizational choice of how to deploy agents across the territory and the geographical scope of decision-making power of each local office can improve the utilization of limited enforcement resources (Bergeron et al. 2022; Kapon et al. 2024).

Because the reform also increased the staff-to-population ratio in consolidated jurisdictions, I also study how staff intensity relates to tax revenues. I leverage yearly variation over time in the number of tax agents deployed across local offices as well as changes in jurisdiction boundaries due to the reform. This enables me to estimate the relationship between staff ratio and tax revenues

while controlling for both municipality and year fixed effects.[4] The estimates imply a positive and sizeable relationship, suggesting that the choice of where to deploy staff by itself matters for tax revenues and could be a channel explaining the effects of the reform (Basri et al. 2021).

To further explore the possibility that consolidation leads to more efficient utilization of resources, I focus on the role of local office managers. By reducing the number of office managers, a consolidated structure can be an opportunity to retain only the most talented ones and give them a larger span of control. To take a first step in testing this hypothesis, I investigate whether higher manager quality leads to larger tax collection gains from the reform (Fenizia 2022). Using detailed data on tax agents' careers, I identify high-quality managers overseeing local offices during the reform period. Specifically, I build on Minni (2024) and define high-quality managers as those promoted to manager at a relatively younger age. I find that tax revenue increases are greater in *consolidated* municipalities overseen by offices with high-quality managers in charge.

To understand whether the divergence in revenues within consolidated jurisdictions is due only to reoptimization in the allocation of effort or also to new frictions that make tax enforcement harder, I study the role of distance. After the reform, *periphery* municipalities end up being served by a new office that is typically farther away (on average, the new office is 114 minutes farther than the previous one, an 85% increase relative to the pre-reform average). To isolate the effect of distance from the tax office from other confounding factors due to geographical remoteness, I leverage the variation in distance at baseline between a municipality and the nearest *other* office that could serve it, combined with the closing of offices, and control for a proxy of market access.[5] I find that *periphery* municipalities that end up one hour farther from the new jurisdiction tax office experience a 0.8% slower growth in tax collection compared to *core* 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 be. Therefore, one might expect stronger effects of distance on tax revenues in areas where local information is more valuable. As local information is more valuable in settings with less third-party reporting, I test this hypothesis by leveraging geographic variation in the formality rate of workers (Jensen 2022). Heterogeneity with respect to this measure shows that the negative effects of distance on tax revenues are larger where the formality rate is lower and local information is

4. As the staff-to-population ratio changes every year, a difference-in-differences approach leveraging only the variation from the reform is not suitable to explore this channel.

5. I illustrate the advantages and limitations of this approach in detail in Section 5.2.

thus more valuable. This suggests that distance acts as a friction that prevents tax agents from gathering local information. To further support this mechanism, I geocode data on work-related trips carried out by tax agents and use those carried out by agents in the audit department as a proxy for physical inspections of taxpayer premises. I find suggestive evidence that municipalities farther away receive fewer inspections.

Taken together, these findings document that a consolidated structure increases overall revenues but exacerbates geographic inequality in tax enforcement. Moreover, by uncovering some forces underlying the trade-off between a more or less consolidated structure, this paper can offer guidance to policymakers interested in implementing a similar reform in other contexts. For instance, a consolidated structure may lead to larger revenue gains in contexts characterized by geographically concentrated economic activity, high heterogeneity in managerial talent, high levels of formality, and limited geographical frictions. More broadly, the results underscore the importance of the spatial organization of a bureaucracy for its effectiveness.

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).[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.[7] 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 a tax authority.[8]

Second, the results of this paper relate to a vast literature studying state capacity through the

6. 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).

7. In doing so, I speak to the literature on the value of local information in governance across various domains (Duflo et al. 2018; Dal Bó et al. 2021), as well as to the literature studying the incentives, constraints, and consequences of creating and updating administrative cadasters of taxpayers (Casaburi and Troiano 2016; Gadenne 2017; Christensen and Garfias 2021; Bowles 2024; Martínez 2023; Ferraz et al. 2024).

8. 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).

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; Fenizia 2022; Caria et al. 2022; Best et al. 2023; 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 and 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.[9] By highlighting that this dimension can impact how the organization targets staff effort, I complement 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).[10]

Furthermore, I connect to the related strand of the literature examining the effects of administrative reforms in historical settings (Aneja and Xu 2024; 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.[11]

The rest of the paper is organized as follows: Section 2 describes the institutional setting, the reform of the tax authority, and introduces the data sources. Section 3 lays out the research design and empirical strategy. Section 4 presents evidence on the reform's impact on tax revenues. Section 5 explores the mechanisms that contribute to these effects. Section 6 concludes.

9. 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).

10. 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.

11. 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).

2 — Context and Data

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.[12] 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.[13]

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 fiscal regions.[14] 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

12. 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 2.3 for more details.

13. 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.

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

municipalities and is overseen by a local office (yellow dots in the figure).[15] As of January 2020, there are 94 tax jurisdictions.

Tax enforcement responsibilities are shared between central units in Brasília, regional headquarters, and local offices.[16] 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.[17] 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).

The reform occurred at the end of July 2020.[18] 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

15. 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.

16. 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.

17. 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.

18. 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.

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.[19] 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 *core* of the new jurisdictions: these are the municipalities whose baseline jurisdiction office was expanded by the reform. The map in Figure 2b shows which municipalities belong to each group. [20] Table A2b shows that *periphery* municipalities have a smaller population and labor force and a lower level of economic activity than *core* municipalities.

19. Of the remaining tax agents, 3% were deployed to another local office and 5% to another tier of the tax authority.
20. I also report a residual share (*Other*) for observations for which the sector was not reported.

2.3. 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.[21] 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.[22] 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.[23] 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).[24]

Tax agents I collect data on the payroll of federal civil servants from the Transparency Portal

21. 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).

22. 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.

23. 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.

24. 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 January 1.

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.[25] 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.[26] 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,[27] 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 2023), aggregated at the municipality-year level.

25. 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.

26. 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.

27. 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.

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.[28] This leaves me with a sample of 5,415 municipalities in the period 2013–2023.

3 — 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 (e.g., the amount of taxes recovered after auditing given a level of tax evasion) and the indirect (e.g., the deterrent effect of audits on the level of tax evasion) effects of tax enforcement (Allingham and Sandmo 1972).[29]

$$Y_i = \tau_i \bar{Y}_i \tag{1}$$

It follows from Equation 1 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 after the reform if the two groups are on parallel trends in the evolution of their tax base $\bar{Y}$.

28. 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.

29. 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.

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.[30]

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.[31]

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.[32]

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

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

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). D_t is an indicator for each period between 2016 ($t = -5$) and 2023 ($t = 2$).[33] Consolidated_i

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

31. 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.

32. 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 *core* and *periphery* municipalities after matching (Table A3b).

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

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;[34] 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.

$$y_{it} = \alpha_i + \alpha_t + \beta \text{Consolidated}_i \cdot Post_t + X_{it}\psi + \epsilon_{it} \tag{3}$$

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

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^C (\text{Core}_i \cdot D_t) + \sum_{t=-5}^{2} \delta_t^P (\text{Periphery}_i \cdot D_t) + X_{it}\psi + \epsilon_{it} \tag{4}$$

$$y_{it} = \alpha_i + \alpha_t + \beta_1 \text{Core}_i \cdot Post_t + \beta_2 \text{Periphery}_i \cdot Post_t + X_{it}\psi + \epsilon_{it} \tag{5}$$

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 (Core_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 (1), parallel trends in the tax base and in tax revenues would imply parallel trends in tax enforcement.

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

34. 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.

dynamic coefficients from Equation (2) 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 *core*, *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:

$$y_{jt} = \alpha_j + \alpha_t + \rho \text{Consolidated}_j \times \text{Post}_t + X_{jt}\psi + \epsilon_{jt} \tag{6}$$

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 3. 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 2. 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.[35]

35. 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 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%).

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 5. The results are reported in Table 1. The coefficients in column 3 show that tax revenues in *core* 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 *core* 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 4 for both *core* (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, *core* 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.[36] 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).

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

36. The direction of distributional consequences is not obvious ex ante. On the one hand, *core* municipalities tend to be richer than *periphery* municipalities, suggesting that stronger tax enforcement in *core* 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 *core* 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.

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 *core* 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.[37] 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.

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

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.[38] Moreover, when looking at differential effects based on exposure to the reform, this method delivers results very similar to the matched difference-in-differences.[39] *Core* 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 2.3, I cannot observe whether the assessed credit was actually paid by the taxpayer. 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 6 without controls, while even columns include jurisdiction characteristics interacted with year fixed effects.[40] 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

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

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

40. 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.

(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.[41] 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.[42]

Table A4 displays the impact of the reform on personnel structure and costs, reporting results from Equation 6. 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).[43] One can observe that there is no statistically significant change in the number of frontline 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 A5 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.[44]

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.

41. Dynamic coefficients are reported in Figure A12.

42. 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.

43. 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.

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

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. In this section, I conduct several empirical exercises to understand the underlying mechanisms and, more broadly, to understand how the spatial organization of a tax authority shapes fiscal capacity, informing policymakers considering similar reforms in different contexts.

I start by exploring the benefits of concentrating tax agents and decision-making over larger regions in fewer offices and how this may explain the overall increase in revenues. I focus on the possibility that the reform allowed more efficient utilization of enforcement and managerial resources. Because the reform also changed the staff-to-population ratio across jurisdictions, I also study how staff intensity relates to tax revenues. If the benefits arise, at least in part, from reallocating staff effort from *periphery* to *core* municipalities, these mechanisms can explain both the average increase in revenues and the divergence.

I then investigate whether the divergence is also due to the reform making tax enforcement harder in *periphery* municipalities. In particular, I examine the costs of increased distance between tax offices and the *periphery* municipalities they oversee.

5.1. The benefits of consolidation

Improved targeting of staff effort The reform concentrates tax agents and decision-making power in fewer offices. Consolidated offices have larger staff and can decide how to allocate staff effort over a wider region, which includes both *core* municipalities and the *periphery* municipalities that merged in after the reform.[45] This suggests that consolidation may be particularly beneficial in jurisdictions where the tax base is concentrated in *core* municipalities. In these jurisdictions, consolidated offices optimizing the utilization of the newly pooled workforce are able to reallocate staff effort toward areas with a larger tax base, thus improving the targeting of enforcement resources.

To test this hypothesis, I classify consolidated jurisdictions based on the share of their pre-reform GDP that is concentrated in *core* municipalities. Specifically, I distinguish between jurisdictions in which this share is in the top tercile of the distribution (High concentration tax base) and

45. As described in Section 2.1, the selection of cases to audit happens at the regional level and is not done by the local offices. However, the offices are responsible for allocating staff effort during the audit stage.

those in which it is not (Low concentration tax base).[46] I then estimate the following specification in the matched sample:

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^{HC} \left(\text{Consolidated}_i \times \text{High concentration tax base}_i \cdot D_t\right) + \sum_{t=-5}^{2} \delta_t^{LC} \left(\text{Consolidated}_i \times \text{Low concentration tax base}_i \cdot D_t\right) + X_{it}\psi + \epsilon_{it} \quad (7)$$

Figure 6 reports the point estimates and the 95% confidence intervals. In line with the hypothesis of better targeting of enforcement resources, the estimates suggest that the increase in tax revenues for *consolidated* municipalities is higher in jurisdictions in which the tax base is more concentrated in *core* municipalities. In these jurisdictions, the reform led to a 6.7% increase in tax revenues, whereas in jurisdictions where the tax base is not concentrated, the reform led to a 2.2% increase (not statistically significant at conventional levels). A t-test rejects the hypothesis that the effect is the same in the two groups (p-value = 0.035).[47]

These findings suggest that the organizational choice of how to deploy agents across local offices and the geographical scope of decision-making power of each local office can improve the utilization of limited enforcement resources (Bergeron et al. 2022; Kapon et al. 2024; Facchetti 2025).

Managerial resources Public sector managers can influence the productivity of the offices they oversee (Fenizia 2022) through supervision, mentoring, and the allocation of tax agents (Minni 2024; Sen 2024). As managers vary in quality, a consolidated structure can be an opportunity to retain only the most talented managers and give them a larger span of control. As a first step to test this hypothesis, in this section I examine whether higher manager quality leads to larger tax collection gains from the reform.[48] After classifying high-quality managers using data on

46. I consider the distribution of *consolidated* jurisdictions (for non-consolidated jurisdictions, the equivalent of this share would be mechanically 1). The second tercile is 0.65.

47. Columns 1 and 2 of Table 3 report the estimated coefficients.

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

tax agents' careers, I conduct a heterogeneity analysis for the baseline difference-in-differences specification. I find that the increase in tax revenues in *consolidated* municipalities is stronger where high-quality managers oversee the expanded office.

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

I compile a list of all managers overseeing local offices during the sample period.[49] These are the managers I classify. To define and validate the high-quality measure, I consider all the other tax agents deployed in local or lower-tier offices who reach work level 4 between 2013 and 2023.[50] Figure A15a shows the age distribution at promotion to work level 4. I classify high-quality managers as those promoted at an age in the bottom tercile of this distribution, meaning before age 42.[51]

The intuition behind this measure is that faster progression up the managerial ladder indicates higher performance, reflecting the tax authority's valuation of the manager's work. I validate this empirically by showing that high-quality status correlates with future personal success. While wage increases are limited in this context, being deployed to regional or central headquarters can be seen as a promotion. Figure A15b shows that high-quality managers are more likely to eventually be deployed to headquarters.

I apply this measure to the managers overseeing local offices in the year of the reform.[52] I then conduct the following heterogeneity analysis:

49. To do this, for each office-month-year I identify the tax agent occupying the position of either *"delegado(a)"* or *"delegado(a) adjunto"* and with the highest work level.

50. This approach allows me to define and validate the quality proxy on an out-of-sample group and to avoid overfitting issues.

51. 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 search their profiles in the government Transparency Portal (Brazilian Federal government 2023) and retrieve their age at first promotion.

52. Due to substantial managerial turnover during the reform, I focus on offices with only one manager throughout the reform year, 2020. Fifty-three of the 67 sample post-reform jurisdictions meet this criterion.

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^H \left(\text{Consolidated}_i \times \text{High-quality Manager}_i \cdot D_t\right) + \sum_{t=-5}^{2} \delta_t^L \left(\text{Consolidated}_i \times \text{Low-quality Manager}_i \cdot D_t\right) + X_{it}\psi + \epsilon_{it} \quad (8)$$

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

Figure 7 presents the coefficients from the dynamic specification. After the reform, municipalities with low-quality managers see a small (2.2%) but precisely estimated increase in tax revenues. In contrast, the effect is much larger (9%) in municipalities overseen by high-quality managers. A t-test rejects the hypothesis that the effect is the same in the two groups (p-value = 0.033). [53]

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

Staff ratio The reform relocates tax agents across offices. Figure A16a displays the evolution over time of the average staff ratio, the number of tax staff per 1,000 inhabitants in the jurisdiction, and shows that after the reform, *consolidated* jurisdictions tend to have a higher staff ratio than *unaffected* jurisdictions. As it has been documented that countries with more tax agents per capita tend to collect more tax revenues (Okunogbe and Tourek 2024; Jensen and Weigel 2024), it is natural to ask whether this relocation of tax agents is one channel through which the reform increased tax revenues. Indeed, consistent with the cross-country evidence, Figure A16b shows that the positive correlation between staff ratio and tax revenues also holds at the municipality level within Brazil. To further explore this mechanism, I estimate a regression including both municipality and year fixed effects:[54]

$$y_{it} = \alpha_i + \alpha_t + \gamma_T \text{Staff ratio}_{it} + \epsilon_{it} \quad (9)$$

Results, reported in Table 4, confirm the positive relationship between staff ratio and tax

53. Columns 3 and 4 of Table 3 report the estimated coefficients.

54. As staff per capita changes every year, and not only because of the reform, a difference-in-differences approach is not suitable to explore this channel.

revenues. In column 1, I use the whole matched sample; the coefficient is large and positive but noisily estimated. One reason for the large confidence interval may be that *periphery* municipalities experience a change in staff ratio at the same time as they experience an increase in distance from the tax office, which may confound the relationship (see Section 5.2).[55] Therefore, in column 2, I exclude *periphery* municipalities; the coefficient becomes larger and more precisely estimated. In column 3, I further restrict the sample by focusing on *core* municipalities only. As these municipalities are all experiencing consolidation, I can estimate the effect of staff intensity in a group in which every municipality is experiencing the effects of the reform but not a change in distance from the office. In columns 4 to 6, I repeat the same analysis but use an indicator *High staff ratio* for being in the top tercile of the distribution of staff ratio as the explanatory variable. The coefficients are positive and precisely estimated in all the specifications. In Table A6, I repeat the exercise but control for a set of municipality characteristics interacted with year fixed effects.[56] All the coefficients remain large and positive, but in some specifications they become noisily estimated and not statistically significant at conventional levels.

This exercise, despite not being causal, suggests that the increase in staff ratio in *consolidated* jurisdictions is one channel through which the reform increased tax revenues. More broadly, these results point to the relevance of the choice of where to deploy staff for tax capacity (Basri et al. 2021).

5.2. The costs of consolidation: increased distance from tax offices

The reform increases the average distance between tax offices and the municipalities they oversee. Figure A17a shows that on average, *periphery* municipalities end up 114 minutes farther away from their jurisdiction tax office after the reform (a 85% increase relative to the pre-reform average).

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. Moreover, tax agents may be more knowledgeable about the municipalities near their homes, and one may think that they tend to live close to the office

55. Consistent with this, the coefficient becomes larger and precisely estimated once I control for travel distance from the office; see Table A7.

56. I include the same controls as in the even columns in Table 1.

where they are located. Finally, taxpayers may perceive that the probability of detection if they evade is lower if they are farther from the tax office and thus may evade more.

As a starting point, Figure A17b shows a negative correlation between travel distance and tax revenues at the municipality level. Furthermore, in Figure A18, 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 leverage variation due to the consolidation reform to quantify the role of distance in explaining the divergence in tax enforcement within consolidated jurisdictions.

Empirical strategy To study how the distance between a tax office and different municipalities impacts tax enforcement, I restrict my analysis to *consolidated* jurisdictions only. All the municipalities in this sample experience the change in the concentration of tax agents and decision-making power due to the reform, but only *periphery* municipalities experience the change in distance from the tax office.

One may be concerned about endogenous jurisdiction assignments after the reform affecting distance. For instance, municipalities with declining tax potential could be assigned to an office farther away. To address this concern, I leverage the variation at baseline in distance between a municipality and the nearest other tax office; this distance becomes relevant only if and after the tax office serving a municipality closes. Because it uses baseline distances, this approach also addresses the concern that municipalities tend to experience a larger increase in distance if many closings happen in a region (e.g., because a region has a declining tax potential).

Figure A19 provides an illustration of how this variable is defined. Each municipality (stylized towns in the figure) is part, at baseline, of a tax jurisdiction and is served by the corresponding office (colored dots in the figure). For each municipality, I compute the distance from the nearest tax office in another tax jurisdiction but within the same fiscal region, $Distance_{i(0)}^{i \to \bar{r}}$ (dashed lines in the figure). After the reform, this distance becomes more relevant for municipalities initially served by closed offices (the crossed dots).

Moreover, notice that for municipality D, the nearest other office would be located in a different fiscal region (see dashed-dotted line) at a distance $\text{Distance}_{i(0)}^{i \to \hat{r}}$. Since municipalities cannot be assigned to an office in a different fiscal region, the variable $Distance_{i(0)}^{i \to \bar{r}}$ will not consider that distance. However, cases like municipality D allow me to control for time-varying effects of distance from the nearest other office in *any* fiscal region. This can be interpreted as a proxy

for market access: fiscal region boundaries do not matter for goods trade, but they do matter for tax enforcement. This allows me to isolate the effect of distance from the tax office from other possible effects of geographic remoteness.

The correlation between the distance from the nearest office in a different tax jurisdiction but same fiscal region (horizontal axis) and the distance from the actual jurisdiction tax office (vertical axis) can be visualized in Figure A20. The left panel represents the correlation before the reform. There is a clear positive correlation both for municipalities whose jurisdiction tax office closes in 2020 (red dots) and for those whose office remains open (blue and gray dots). However, the right panel displays that this relationship becomes steeper after the reform for *periphery* municipalities only.

I estimate the following specification:

$$y_{it} = \alpha_i + \alpha_{rt} + \sum_{t=-5}^{2} \delta_t \left(\text{Periphery}_i \times \text{Distance}_{i(0)}^{i \to \bar{r}} \cdot D_t \right) + \iota \text{Distance}_{i(0)}^{i \to \hat{r}} \times \text{Year}_t + \epsilon_{it} \quad (10)$$

where everything is as in Equation 5; $\text{Distance}_{i(0)}^{i \to \bar{r}}$ is the distance at baseline from the nearest tax office in another jurisdiction but same fiscal region. The coefficients of interest are the δ_t. The inclusion of the distance at baseline from the nearest tax office in another jurisdiction in any fiscal region, $\text{Distance}_{i(0)}^{i \to \hat{r}}$, interacted with year fixed effects allows me to control flexibly for time-varying effects of market access.

As an alternative strategy, I also estimate a difference-in-differences specification comparing *periphery* and *core* municipalities and interacting the post-reform indicator with an indicator for being above ($HighDistance_i = 1$) or below ($LowDistance_i = 1$) the second tercile of the distribution of baseline distance from the nearest other tax office in the same fiscal region.

$$y_{it} = \alpha_i + \alpha_t + \sum_{t=-5}^{2} \delta_t^H \left(\text{Periphery High Distance}_i \cdot D_t \right) + \sum_{t=-5}^{2} \delta_t^L \left(\text{Periphery Low Distance}_i \cdot D_t \right) + \iota \text{Distance}_{i(0)}^{i \to \hat{r}} \times \text{Year}_t + \epsilon_{it} \quad (11)$$

Effects on tax revenues I display the dynamic coefficients from Equation 10 in Figure 8. One can appreciate the absence of evidence of differential pre-trends. The effect is negative and implies that for *periphery* municipalities the average increase in travel distance leads to a 0.75% slower growth in revenues compared to *core* municipalities.[57] As an alternative strategy, I also estimate the specification with indicators for being at high or low distance from the tax office (Equation 11). Results are displayed in Figure A21. Municipalities at a greater distance from the nearest other tax office experience a 7.3% smaller change in tax revenues compared to *core* municipalities. For municipalities that are closer to the nearest other tax office, the difference is 2.5%, and it is not statistically significant. A t-test rejects the hypothesis that the two effects are the same (p-value = 0.007). Overall these estimates suggest that the divergence in tax revenues between *core* and *periphery* municipalities is driven by municipalities that end up farther from the new tax office.[58]

Why does distance matter? So far, the results in this section have shown that distance can be an explanation for the divergence in tax enforcement. Given that in this context taxes are filed electronically, collected through the bank network, and technology is vastly used for tax auditing, this result is not obvious ex ante. While tax agents' ability to detect evasion using technology and hard data should be unaffected by their location, their ability to gather local information may be. If a reason why distance matters is that it makes it harder to gather local information, one would expect a larger effect in areas where local information is more valuable. There is ample evidence on how third-party reporting, by generating an information trail, makes tax enforcement easier (Pomeranz 2015; Jensen 2022). This suggests that local information is less valuable in areas with more third-party reporting. I thus classify municipalities based on the extent of third-party reporting at baseline. In particular, I use information on the formality rate from the 2010 population census. I classify municipalities in the top tercile of the distribution of formality rate as having *high* third-party reporting and the others as having *low* third-party reporting.

Figure 9 displays the dynamic coefficients from estimating Equation 10 but allowing differential effects for municipalities with *high* and *low* third-party reporting. 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 tax revenues. In municipalities with *high* third-party reporting,

57. For *periphery* municipalities distance from their jurisdiction office increases on average by 85% after the reform.

58. Column 1 of Table 5 report the coefficients.

the effect is close to zero and not statistically significant. A t-test rejects the hypothesis that the two effects are the same (p-value = 0.03).[59] This evidence suggests that distance matters because it hinders the ability of tax agents to gather local information.

Distance and tax agents' visits One way in which tax agents can collect local information is by physically inspecting taxpayers' premises. While I do not have data on these inspections, I use information on tax agents' work-related trips as a proxy. In particular, I consider only trips from local offices to municipalities in their jurisdictions. Moreover, I focus on trips carried out by tax agents working in the auditing department. I then estimate the specification in Equation 10 with a Poisson model using the number of trips received at the municipality-year level as the dependent variable.

Figure 10 reports the dynamic coefficients. There is no evidence of differential pre-trends. After the reform, we observe a decline in the number of trips in the first two years.[60] However, the coefficient in the third year is smaller and indistinguishable from zero.[61]

Summing up, this section has shown that increased travel distance between municipalities and local offices is a relevant channel explaining the divergence between *periphery* and *core* municipalities. The effects are stronger in areas where gathering local information is more valuable for tax enforcement. The results on the number of tax agents' visits, while noisily estimated, provide suggestive evidence that distance acts as a friction making it harder for tax agents to visit *periphery* municipalities and gather local information.

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.

59. Columns 2 and 3 of Table 5 report the coefficients.

60. The implied average effect is a 14.9% reduction in the number of trips, a t-test rejects that the effect is equal to 0 at the 10% level ($p - value = 0.09$).

61. Column 4 of Table 5 report the coefficients.

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 core 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 (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 geographically concentrated economic activity, high heterogeneity in managerial talent, high levels of formality, and limited geographical frictions.

This study opens several avenues for future research. First, having provided evidence on the importance of how tax agents are deployed across the territory, the natural next step is to quantify the returns to higher staff-to-population ratios for tax enforcement. This key parameter may allow progress toward estimating a tax enforcement production function and provide guidance on the optimal allocation of tax agents across the territory. Second, the results on managerial quality call for further investigation of how to select talented managers and what these managers do differently.

More broadly, this paper shows that the effectiveness of a bureaucracy can be affected by its spatial organization. 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)
Core × Post			0.049***	0.052***
			(0.018)	(0.015)
Municipality	✓	✓	✓	✓
Year	✓	✓	✓	✓
Controls		Yes		Yes
P-value: Core = 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 3 (columns 1 and 2) and from Equation 5 (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. *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 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 6. 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: Mechanism: concentration tax base and managerial quality

	Tot. Tax (log)			
	Concentration tax base		Quality manager	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Consolidated × t = -5	0.023	-0.003	0.051**	-0.011
	(0.020)	(0.019)	(0.020)	(0.022)
Consolidated × t = -4	0.034*	-0.009	0.026	0.004
	(0.018)	(0.015)	(0.023)	(0.016)
Consolidated × t = -3	-0.007	-0.014	-0.004	0.001
	(0.015)	(0.014)	(0.019)	(0.015)
Consolidated × t = -2	0.028***	0.008	0.014	0.018
	(0.010)	(0.011)	(0.013)	(0.013)
Consolidated × t = 0	0.039**	0.012	0.062***	0.017
	(0.015)	(0.011)	(0.020)	(0.012)
Consolidated × t = 1	0.065**	0.025	0.108***	0.030**
	(0.026)	(0.015)	(0.030)	(0.015)
Consolidated × t = 2	0.092***	0.028	0.100***	0.037**
	(0.025)	(0.019)	(0.036)	(0.017)
Municipality	✓	✓	✓	✓
Year	✓	✓	✓	✓
P-value: High = Low	0.035		0.033	
R^2	0.983	0.98	0.98	0.98
Observations	54,450	54,450	42,713	42,713
Clusters	136	136	114	114
Dep. Var. Mean	64,080	64,080	57,618	57,618

Notes. The table presents estimates from Equation 7 (columns 1 and 2) and from Equation 8 (columns 3 and 4). Columns 1 and 2 report the effects of the reform separately for municipalities in jurisdictions with *high* (column 1) and *low* (column 2) concentration of the tax base. Columns 2 and 4 report the effects of the reform separately for municipalities in jurisdictions with *high* (column 3) and *low* (column 4) quality managers in charge at the time of the reform. The sample in columns 3 and 4 excludes the 14 jurisdictions which experience managerial turnover in the year of the reform. See Section 5.1 for definition of the variables. *Consolidated* indicator equal to 1 for municipalities part of consolidated jurisdiction. *High concentration tax base*: municipality in consolidated jurisdiction with share of pre-reform GDP that is concentrated in *core* municipalities in the top tercile of the distribution. *Low concentration tax base*: municipality in consolidated jurisdiction with share of pre-reform GDP that is concentrated in *core* municipalities in the two bottom terciles of the distribution. *High quality manager*: municipality in consolidated jurisdiction overseen by a manager first promoted to a managerial position before age 42. *Low quality manager*: municipality in consolidated jurisdiction with manager overseen by a manager first promoted to a managerial position after age 42. *Fixed effects:* municipality and year. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 4: Mechanism: staff ratio

	Tot. Tax (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Staff ratio	0.538	0.860*	1.536**			
	(0.377)	(0.518)	(0.713)			
High Staff ratio				0.035***	0.056***	0.051***
				(0.012)	(0.012)	(0.017)
Municipality	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
R^2	0.98	0.98	0.98	0.98	0.98	0.98
Clusters	136	127	54	136	127	54
Observations	54,450	42,735	14,718	54,450	42,735	14,718
Dep. Var. Mean	64,080	64,080	64,080	64,080	64,080	64,080

Notes. The table presents estimates from Equation 9. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The explanatory variables are: staff ratio, defined as number of tax agents per 1,000 inhabitants (columns 1 to 3) and an indicator for staff ratio being in the top tercile of the distribution (columns 4 to 6). Columns 1 and 4 include the whole matched sample. Columns 2 and 5 only *core* and *unaffected* municipalities. Columns 3 and 6 include only *core* municipalities. *Fixed effects:* municipality and year. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the mesoregion level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

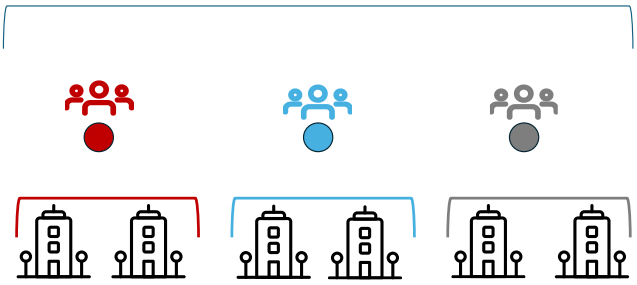
Table 5: Mechanism: distance from office

	Tot. Tax (log)			N. Audit Inspections
		High-Formality	Low-Formality	
	(1)	(2)	(3)	(4)
Periphery Distance other office pre (log) × t = -5	-0.002	-0.000	-0.002	-0.103
	(0.004)	(0.005)	(0.005)	(0.090)
Periphery Distance other office pre (log) × t = -4	-0.004	-0.004	-0.004	-0.016
	(0.003)	(0.003)	(0.003)	(0.106)
Periphery Distance other office pre (log) × t = -3	0.000	0.007**	-0.002	-0.048
	(0.003)	(0.003)	(0.004)	(0.087)
Periphery Distance other office pre (log) × t = -2	0.000	0.001	-0.000	-0.038
	(0.002)	(0.002)	(0.002)	(0.070)
Periphery Distance other office pre (log) × t = 0	-0.008***	-0.003	-0.010***	-0.157
	(0.003)	(0.003)	(0.003)	(0.099)
Periphery Distance other office pre (log) × t = 1	-0.009***	-0.002	-0.012**	-0.166*
	(0.004)	(0.004)	(0.005)	(0.100)
Periphery Distance other office pre (log) × t = 2	-0.009***	-0.003	-0.012**	-0.047
	(0.005)	(0.005)	(0.005)	(0.118)
Municipality	✓	✓	✓	✓
Year	✓	✓	✓	✓
Control Mkt Access	Yes	Yes	Yes	Yes
P-value: High = Low		0.038		
R^2	0.984	0.98	0.98	NA
Observations	26,433	26,433	26,433	7,350
Clusters	69	69	69	64
Dep. Var. Mean	56,403	56,403	56,403	0.3

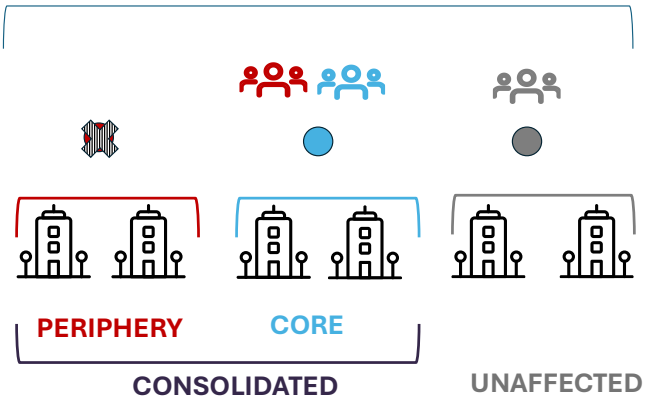
Notes. The table presents estimates from Equation 10. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues (columns 1, 2, and 3) and number of physical tax inspections (column 4). The sample include only municipalities in consolidated jurisdictions. In columns 2 and 3 the specification is augmented by interacting distance with indicators for being a municipality with high (column 2) or low (column 3) formality rate; the coefficients are estimated in the same regression but are reported separately for easier visualization. The coefficients in column 4 is estimated with a Poisson model. All 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 5.2 for definition of the variables. *Periphery* indicator equal to 1 for municipalities whose jurisdiction tax office closes in 2020. *Log Distance other office (pre):* Log distance from the nearest other tax office in the same fiscal region before the reform (in minutes). *High Formality*: municipality with formality rate in employment in the top tercile of the distribution. *Low Formality*: municipality with formality rate in employment in the two bottom terciles of the distribution. *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



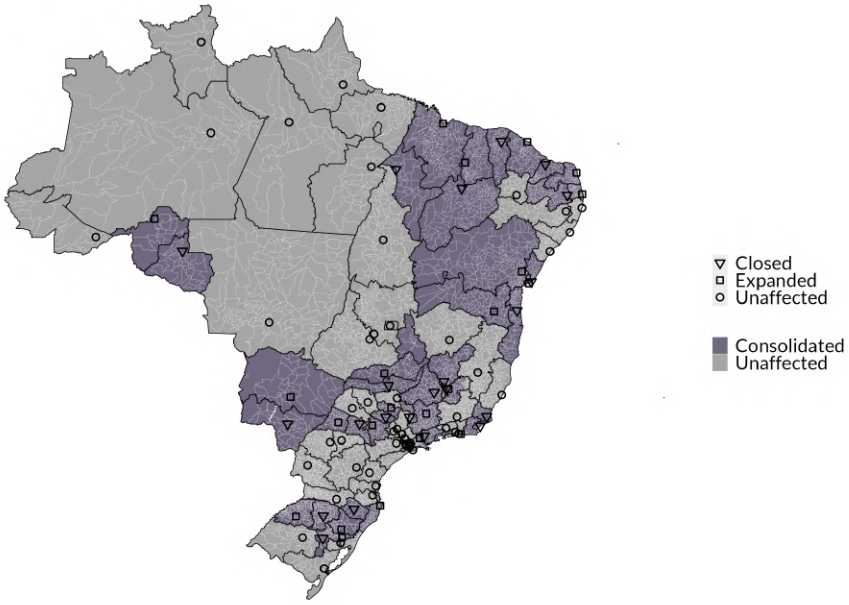
(a) Baseline



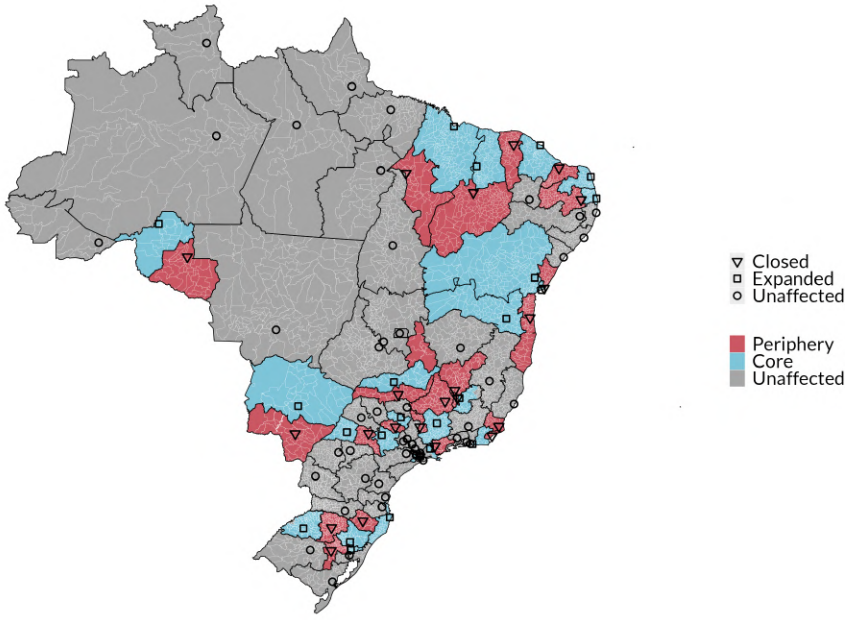
(b) Post reform

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 *core* and *periphery* municipalities. *Core* 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



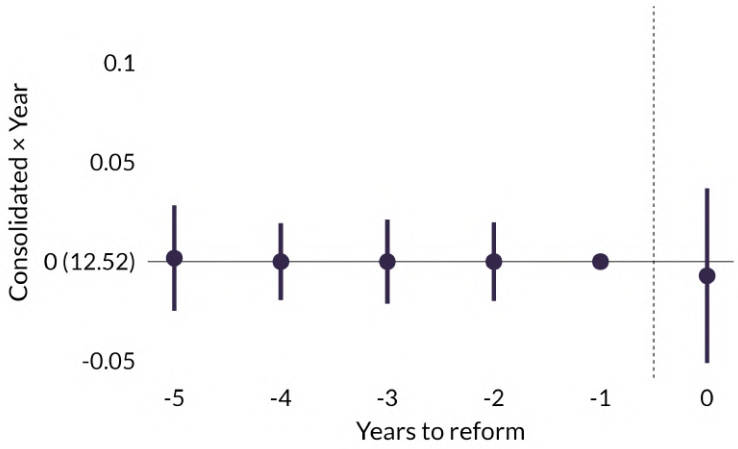
(a) *Consolidated* and *unaffected*



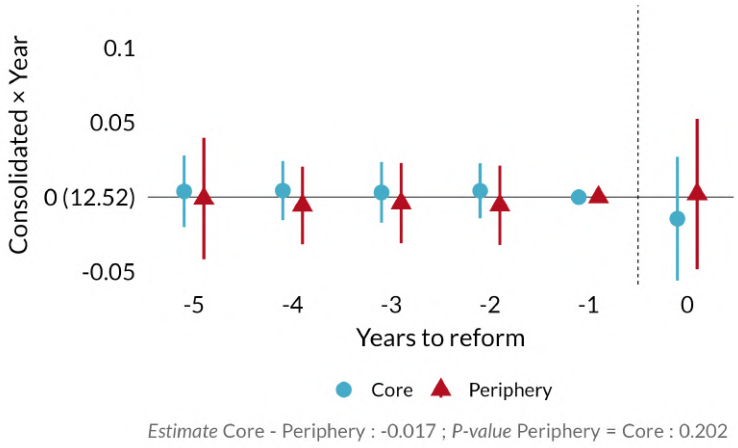
(b) *Core, periphery* and *unaffected*

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 *core* (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*

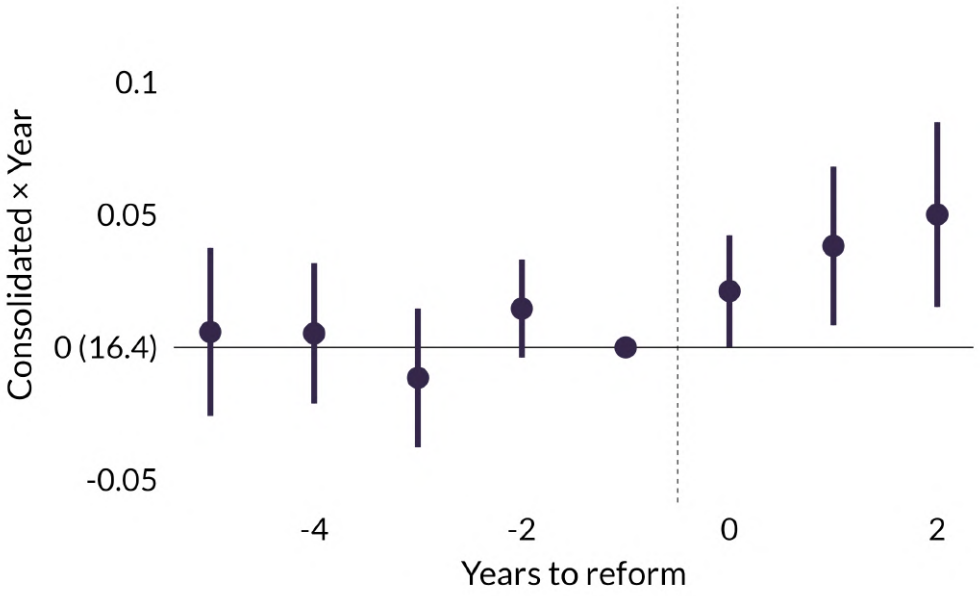


Estimate Core - Periphery : -0.017 ; *P-value* Periphery = Core : 0.202

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

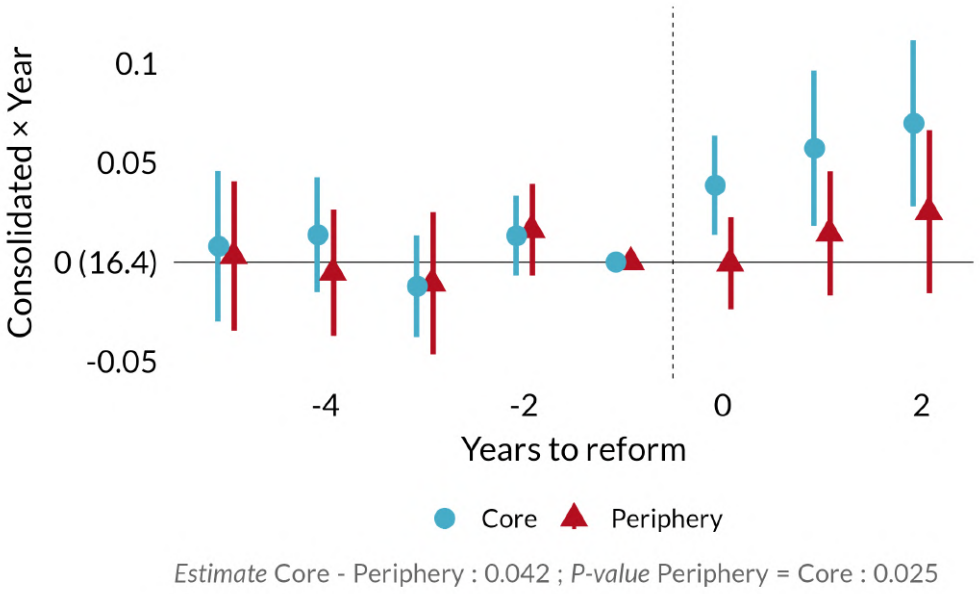
Notes. The figure presents estimates from Equation 2 (top panel) and from Equation 4 (bottom panel). Observations are at the municipality-year level. The dependent variable is the log of GDP (available up to 2021). The coefficient for the year of the reform is normalized to 0. The parenthetical label 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



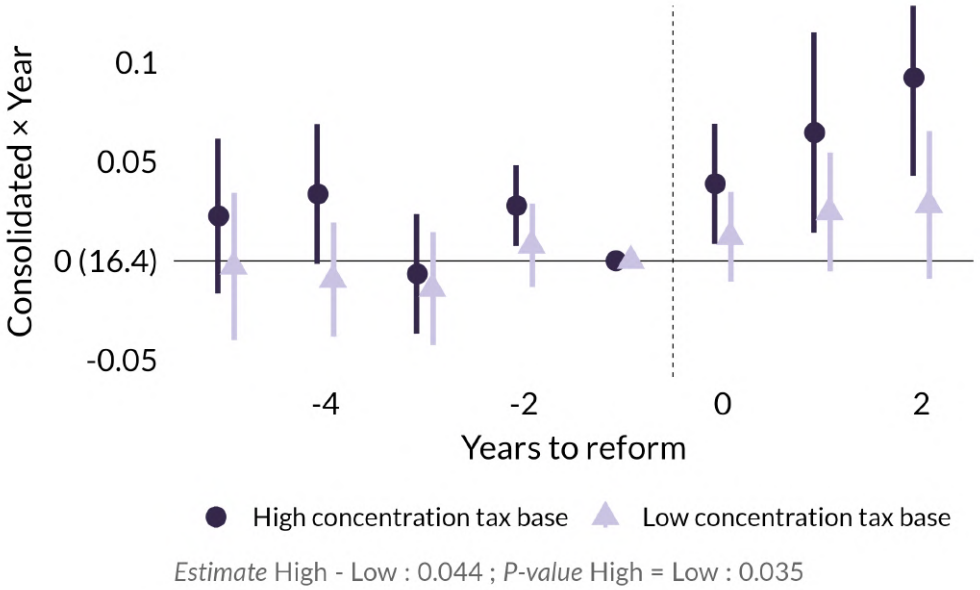
Notes. The figure presents estimates from Equation 2. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The coefficient for the year of the reform is normalized to 0. The parenthetical label reports the sample average of the dependent variable 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



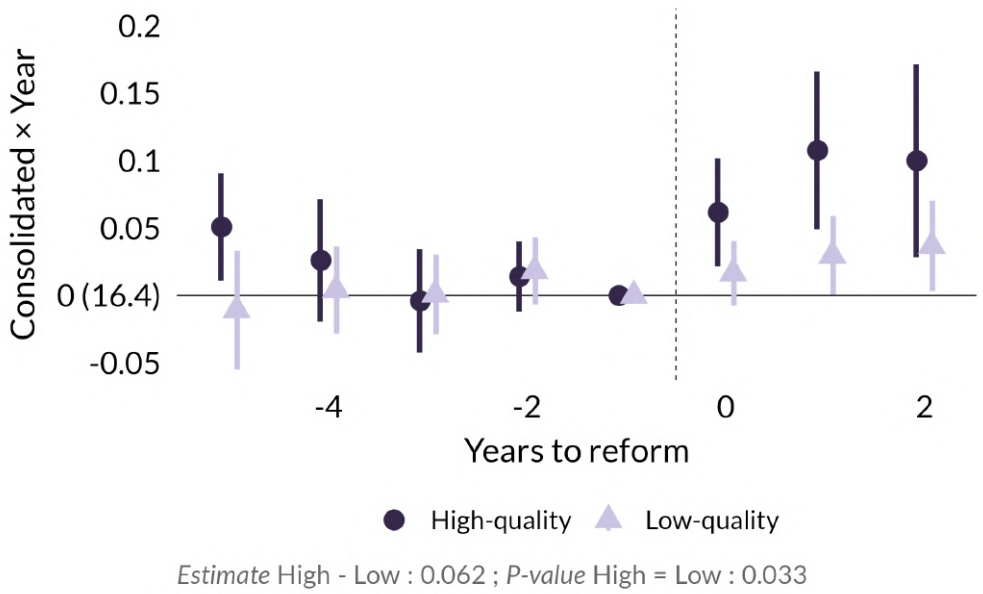
Notes. The figure presents estimates from Equation 4. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *periphery* (red triangles) and *core* (blue dots) municipalities. The coefficient for the year of the reform is normalized 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: concentration tax base



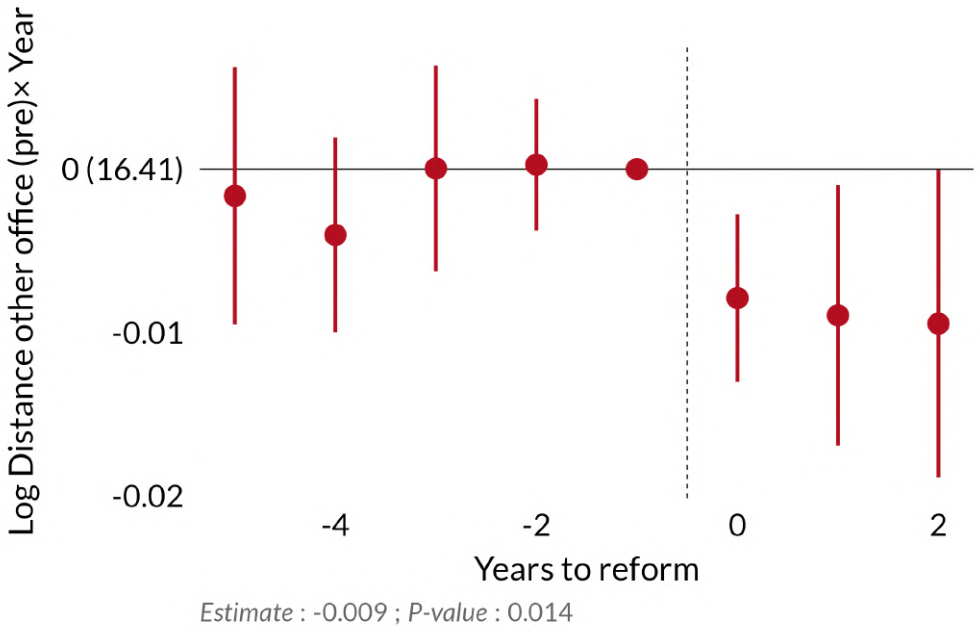
Notes. The figure presents estimates from Equation 7. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) concentration of the tax base. Concentration is measured as the share of the (pre-reform) GDP in the jurisdiction that comes from *core* municipalities. *High* concentration 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. *Fixed effects:* municipality and year. Standard errors are clustered at the mesoregion level. Error bars represent 95% confidence intervals.

Figure 7 — Mechanism: managerial quality



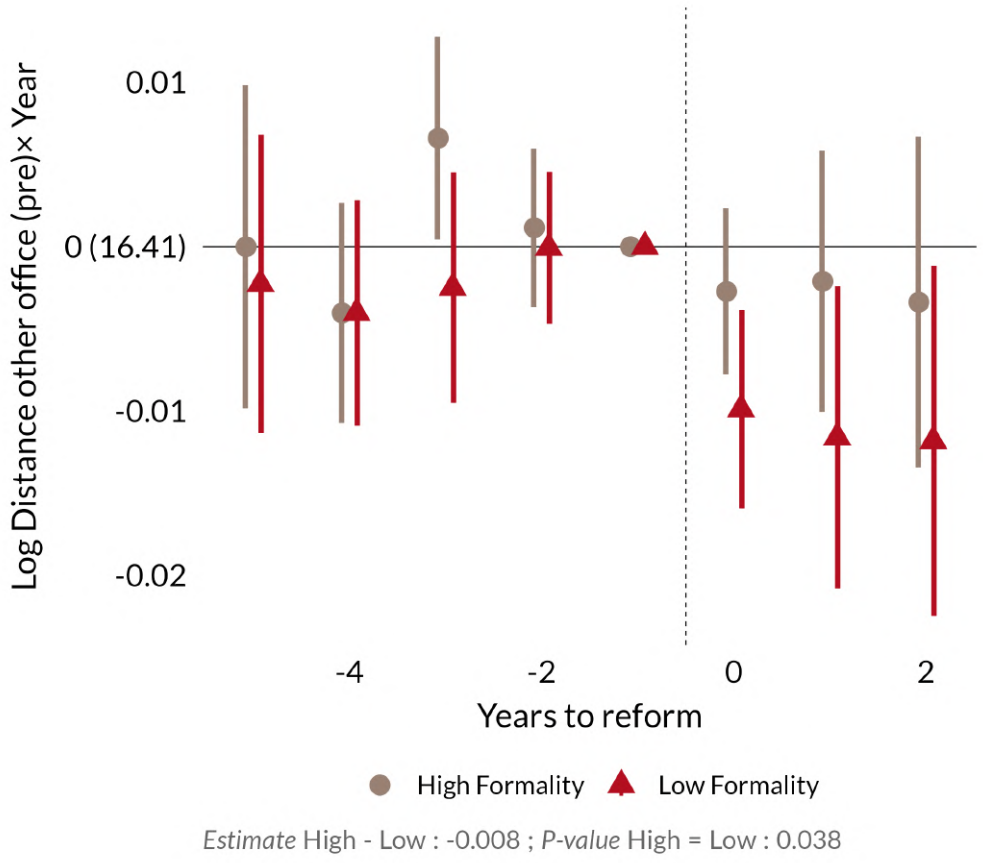
Notes. The figure presents estimates from Equation 8. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. I report estimates for *consolidated* municipalities in jurisdictions with *high* (dots) and *low* (triangles) quality managers in charge at 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 8 — Mechanism: distance from office and revenues



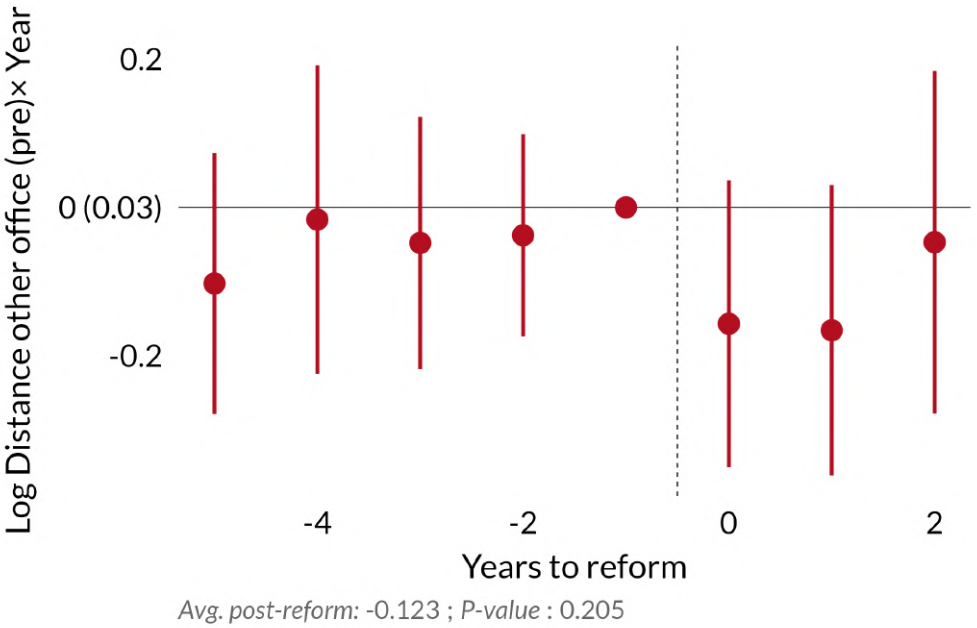
Notes. The figure presents estimates from Equation 10. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The sample include only municipalities in consolidated jurisdictions. I report estimates for the interaction between distance from the nearest 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 9 — Mechanism: distance from office and revenues by prevalence of third party reporting



Notes. The figure presents estimates from Equation 10 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 10 — Mechanism: distance from office and tax inspections



Notes. The figure presents coefficients from Equation 10 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.

A — Appendix: Additional Tables and Figures

A.1. Additional tables

Table A1: Characteristics offices affected by the reform

	Closing		Expanded	
	(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

	Core (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

	Core (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 personnel structure

	N. Agents (log)	N. Frontline (log)	N. Middle managers (log)	N. Top managers (log)	Frontline / Top managers	Middle managers / Top managers
	(1)	(2)	(3)	(4)	(5)	(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 6. 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 A5: 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 6. 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. $^{*}\ p < 0.1$; $^{**}\ p < 0.05$; $^{***}\ p < 0.01$

Table A6: Effects of staff ratio on tax revenues: additional controls

	Tot. Tax (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Staff ratio	0.104	0.418	1.236*			
	(0.353)	(0.468)	(0.673)			
High Staff ratio				0.020*	0.036***	0.041***
				(0.011)	(0.010)	(0.015)
Municipality	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.98	0.98	0.98	0.98	0.98	0.98
Clusters	136	127	54	136	127	54
Observations	54,450	42,735	14,718	54,450	42,735	14,718
Dep. Var. Mean	64,080	64,080	64,080	64,080	64,080	64,080

Notes. The table presents estimates from Equation 9. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The explanatory variables are: staff ratio, defined as number of tax agents per 1,000 inhabitants (columns 1 to 3) and an indicator for staff ratio being in the top tercile of the distribution (columns 4 to 6). Columns 1 and 4 include the whole matched sample. Columns 2 and 5 exclude *periphery* municipalities. Columns 3 and 6 include only *core* municipalities. *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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

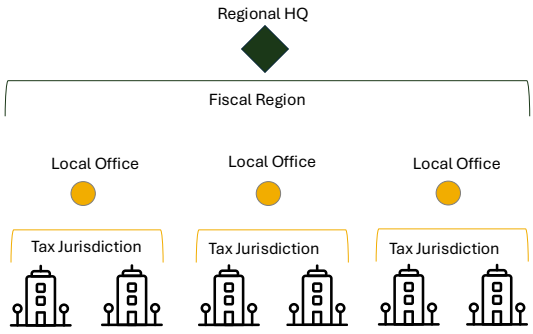
Table A7: Effects of staff ratio on tax revenues, controlling for distance from office

	Tot. Tax (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Staff ratio	0.704*	0.863*	1.565**			
	(0.397)	(0.515)	(0.709)			
High Staff ratio				0.039***	0.056***	0.051***
				(0.012)	(0.012)	(0.017)
Municipality	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓
R^2	0.98	0.98	0.98	0.98	0.98	0.98
Clusters	136	127	54	136	127	54
Observations	54,450	42,735	14,718	54,450	42,735	14,718
Dep. Var. Mean	64,080	64,080	64,080	64,080	64,080	64,080

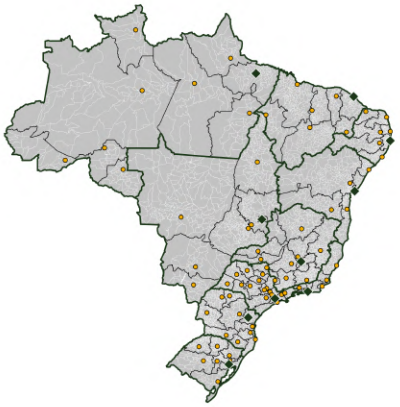
Notes. The table presents estimates from Equation 9. Observations are at the municipality-year level. The dependent variable is the log of federal tax revenues. The explanatory variables are: staff ratio, defined as number of tax agents per 1,000 inhabitants (columns 1 to 3) and an indicator for staff ratio being in the top tercile of the distribution (columns 4 to 6). Columns 1 and 4 include the whole matched sample. Columns 2 and 5 exclude *periphery* municipalities. Columns 2 and 5 include only *core* municipalities. All the regressions control for the (log) travel distance from the jurisdiction office. *Fixed effects:* municipality and year. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the mesoregion level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

A.2. Additional figures

Figure A1 — The spatial organization of the federal tax authority



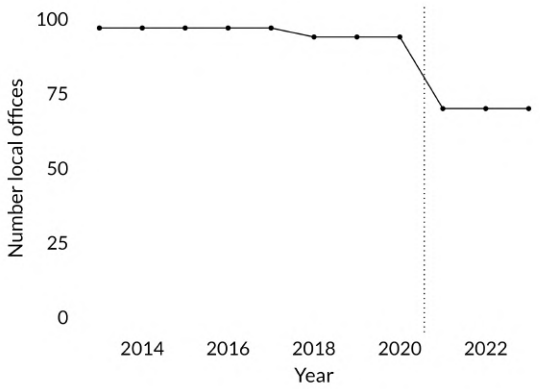
(a) Schematic representation



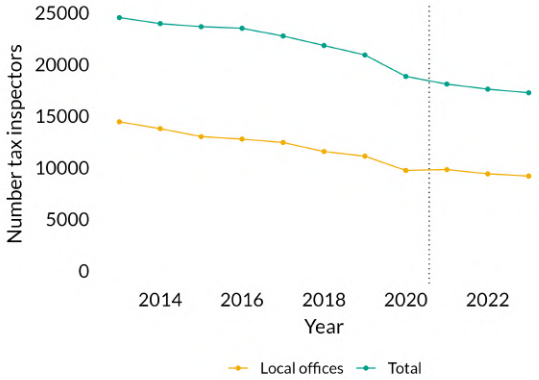
(b) Map of offices and tax jurisdictions before the reform

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.

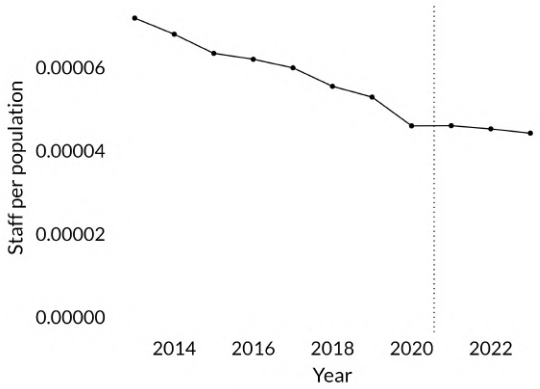
Figure A2 — The evolution of the tax authority



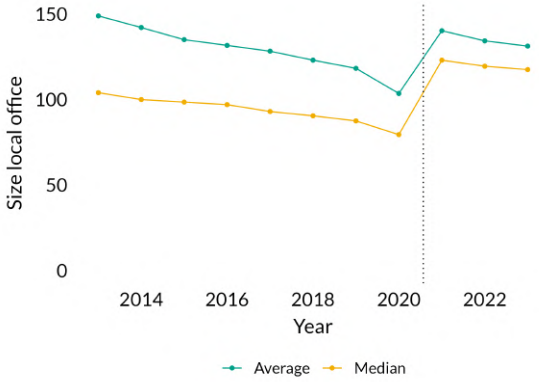
(a) Number of local tax offices



(b) Number of tax agents



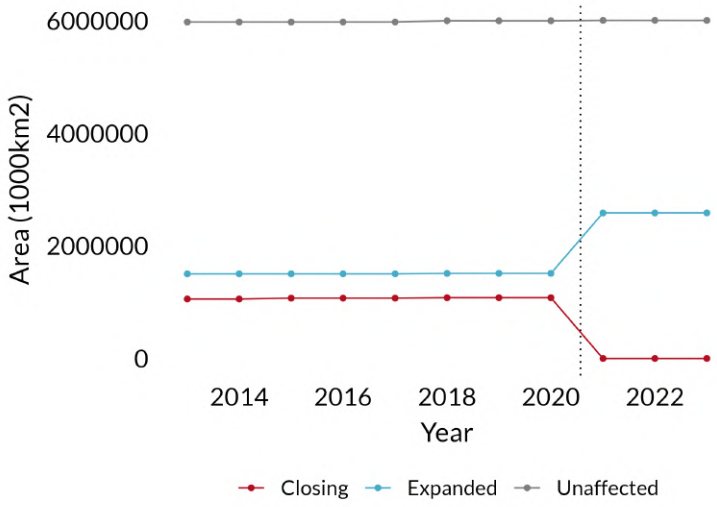
(c) Staff ratio



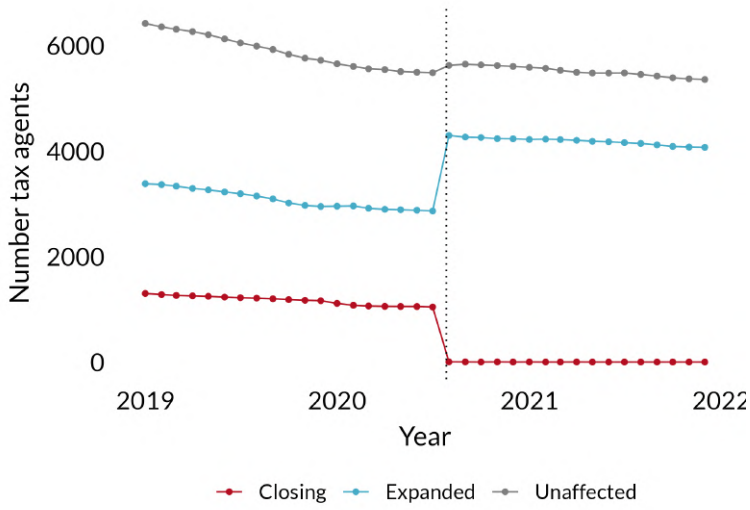
(d) Size of local tax offices

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



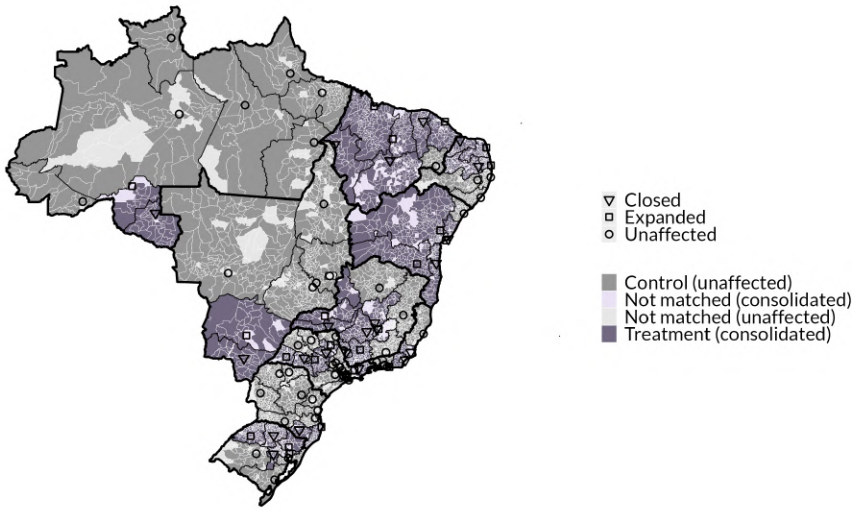
(a) Municipalities served by closing offices are assigned to expanded offices



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

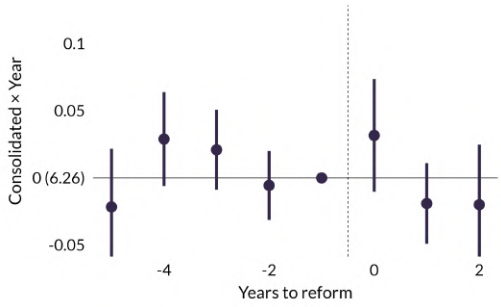
Notes. These graphs illustrate the reform. Municipalities are assigned to the jurisdiction of the expanded offices. The reform happens on July 27, 2020. From the month of August, 92% of tax agents from closing offices are assigned to the expanded offices

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

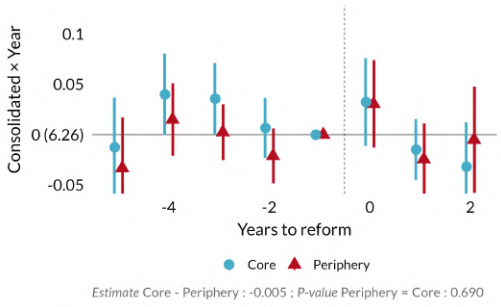


Notes. These figures display which municipalities are matched and how they are exposed to the reform. Treatment municipalities are the matched *consolidated* (dark purple), control municipalities are the matched *unaffected* (dark gray). I also report non-matched *consolidated* (light purple) and non-matched *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)



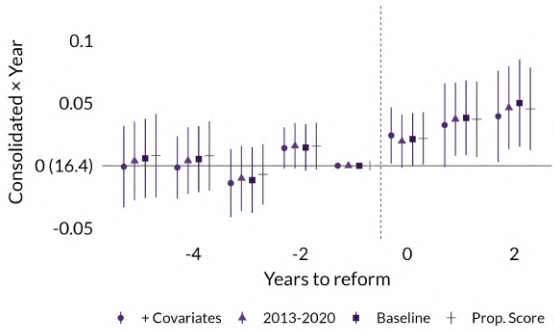
(a) *Consolidated* and *unaffected*



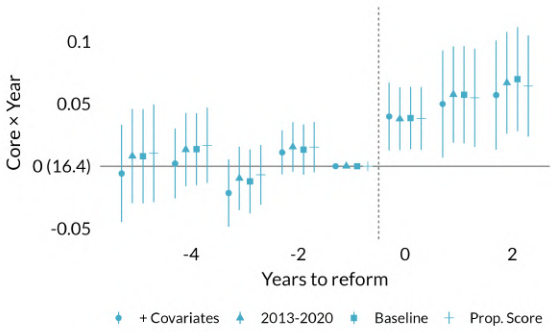
(b) *Core, periphery* and *unaffected*

Notes. The figure presents estimates from Equation 2 (left panel) and from Equation 4 (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.

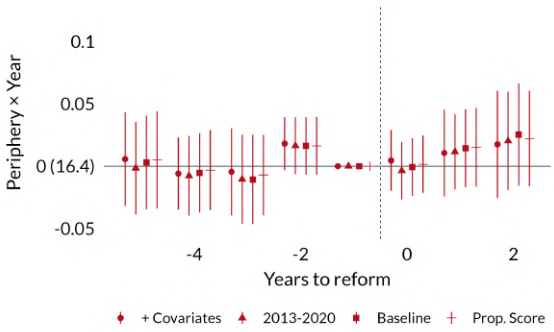
Figure A6 — Effect of consolidation on tax revenues: alternative matching procedure



(a) Consolidated



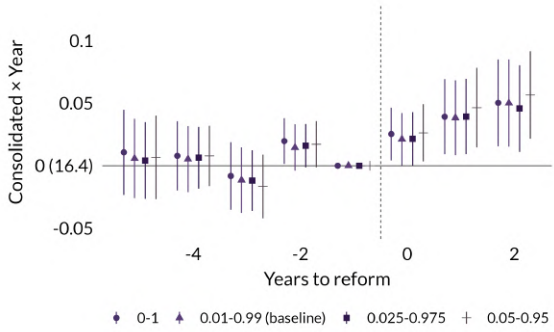
(b) Core



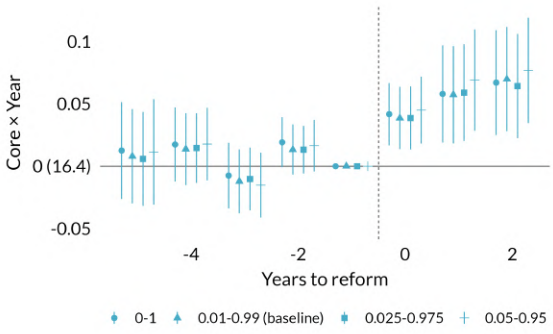
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *core* (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, agricultural, 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.

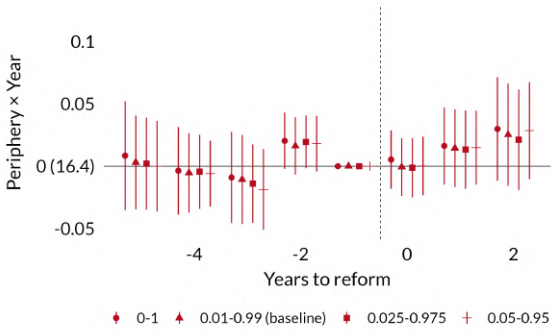
Figure A7 — Effect of consolidation on tax revenues: alternative common support before matching



(a) Consolidated



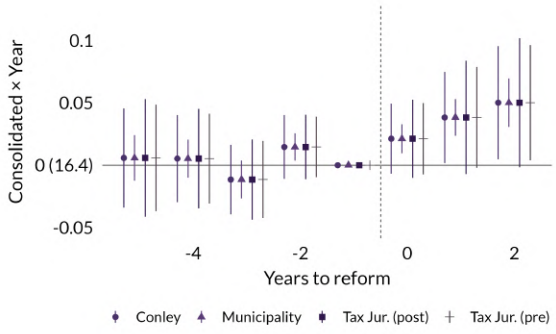
(b) Core



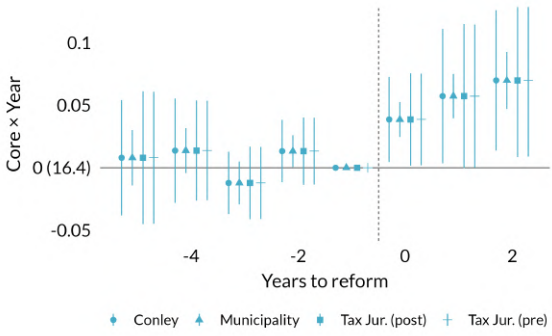
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *core* (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.

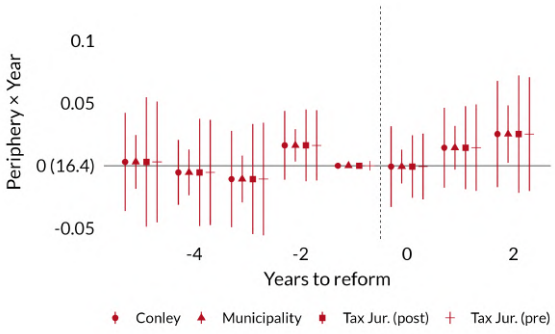
Figure A8 — Effect of consolidation on tax revenues: alternative inference



(a) Consolidated



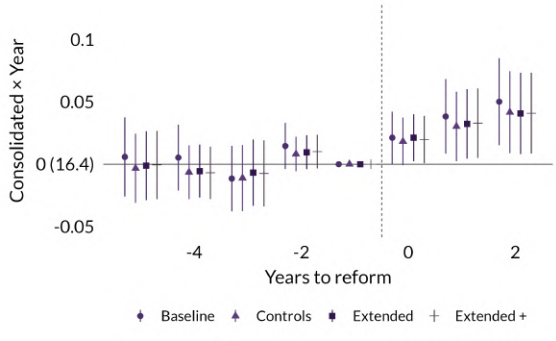
(b) Core



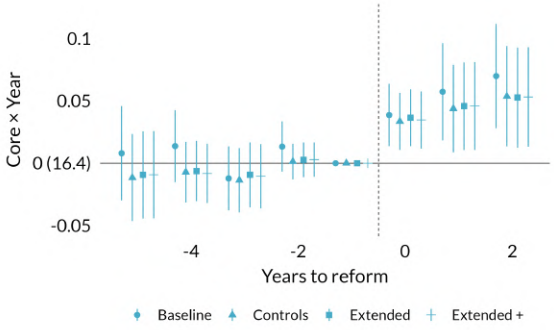
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *core* (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.

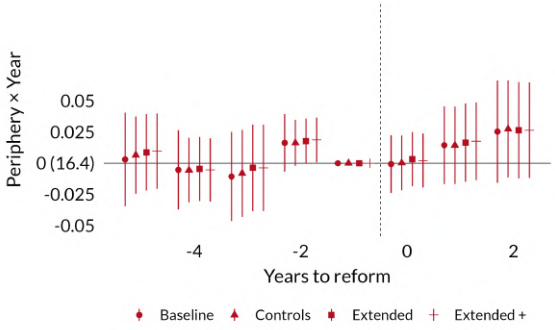
Figure A9 — Effect of consolidation on tax revenues: controls



(a) Consolidated



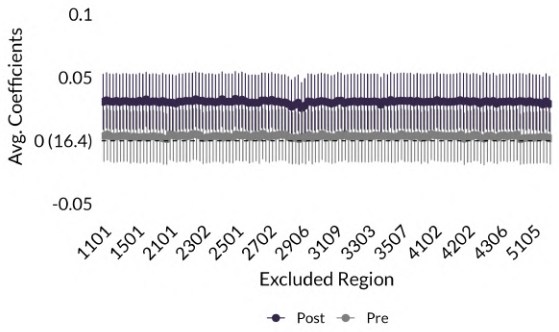
(b) Core



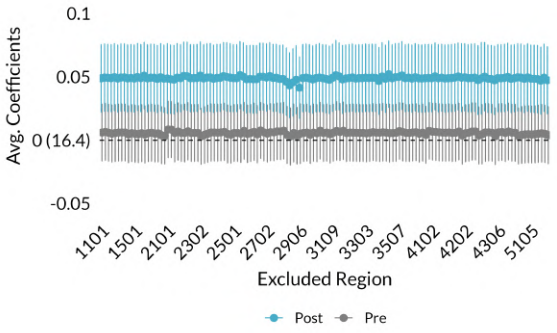
(c) Periphery

Notes. The plot reports the estimated coefficients and the 95% confidence interval for the interaction between year and (i) *consolidated* (panel a), (ii) *core* (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), agricultural, 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 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.

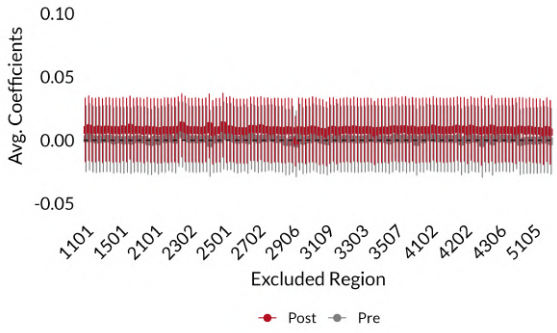
Figure A10 — Effect of consolidation on tax revenues: leave one out



(a) Consolidated



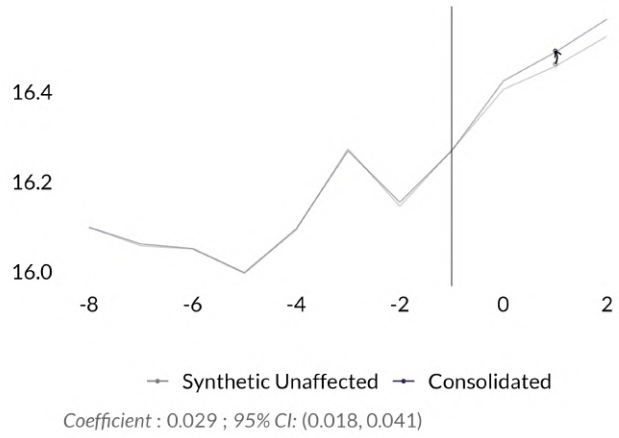
(b) Core



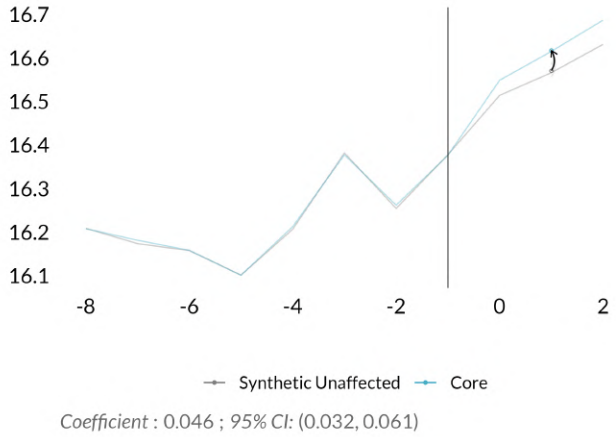
(c) Periphery

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) *core* (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.

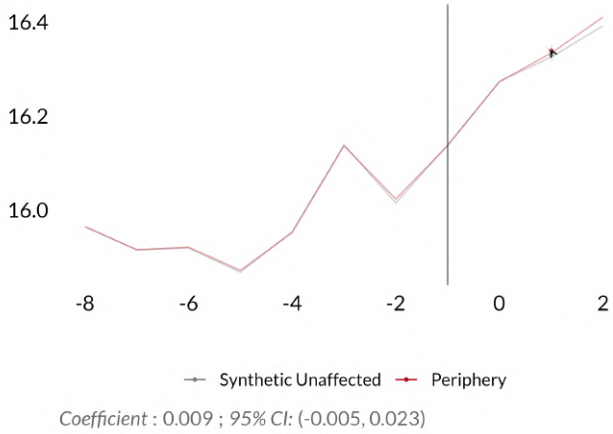
Figure A11 — Effect of consolidation on tax revenues: synthetic DID



(a) Consolidated



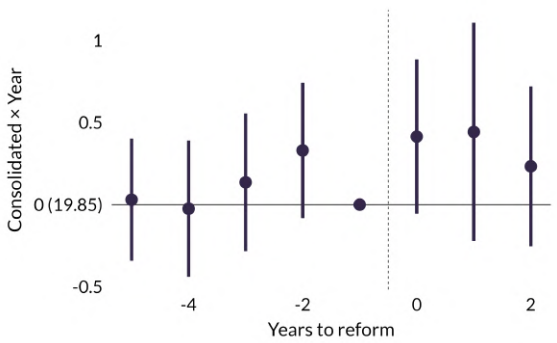
(b) Core



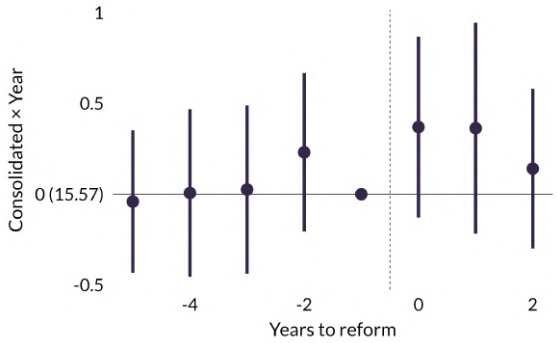
(c) Periphery

Notes. The plot reports the evolution of (log) tax revenues for (i) *consolidated* (panel a), (ii) *core* (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).

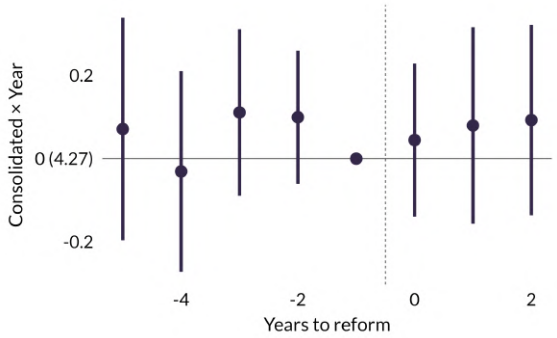
Figure A12 — Effect of consolidation on tax audits



(a) Tot. Amount Audit



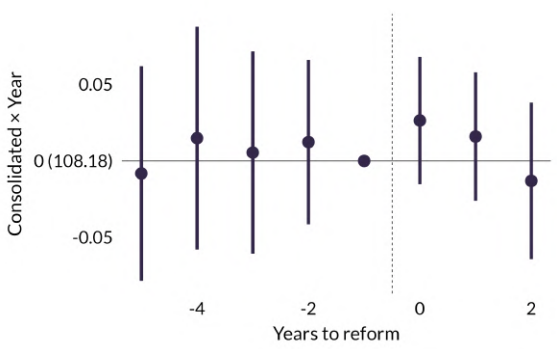
(b) Avg. Amount Audit



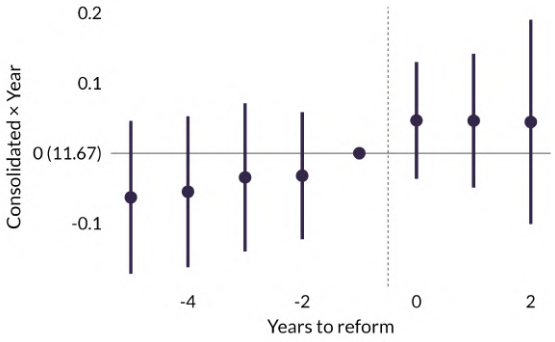
(c) N. audits

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.

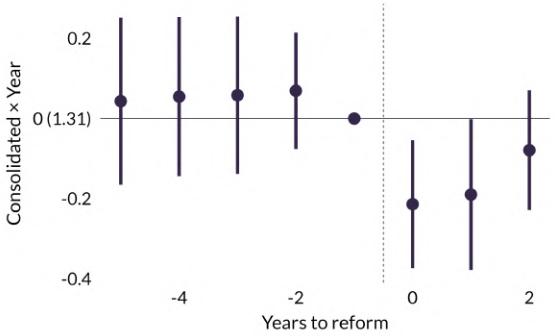
Figure A13 — Effect of consolidation on size personnel



(a) Frontline



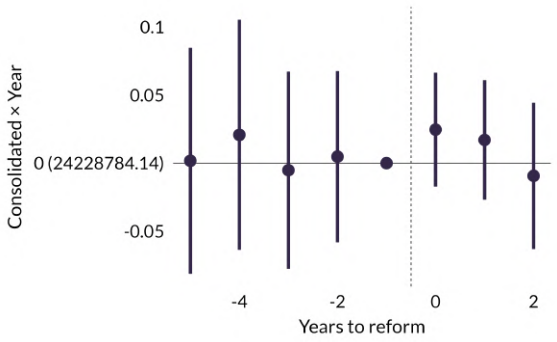
(b) Middle managers



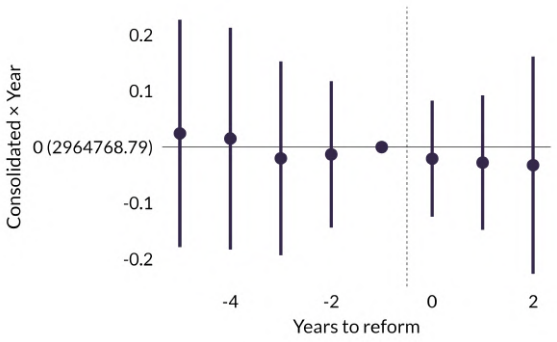
(c) Top managers

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.

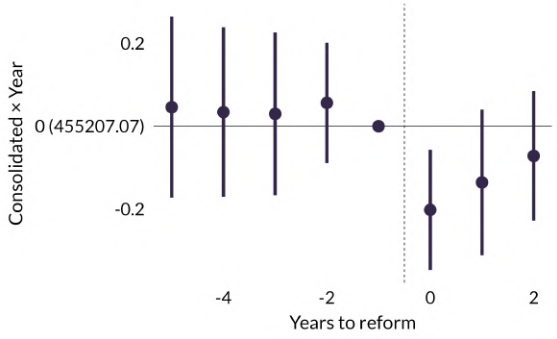
Figure A14 — Effect of consolidation on personnel cost



(a) Frontline



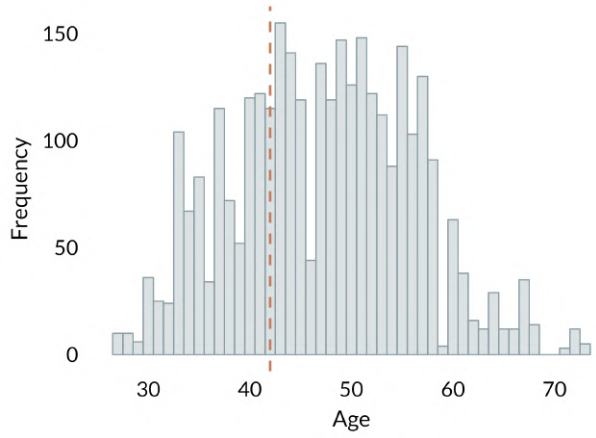
(b) Middle managers



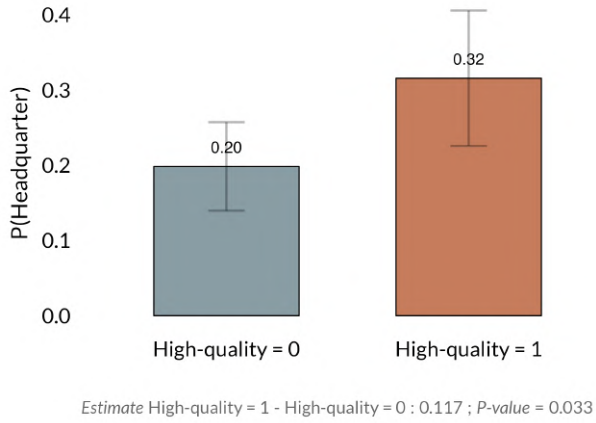
(c) Top managers

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



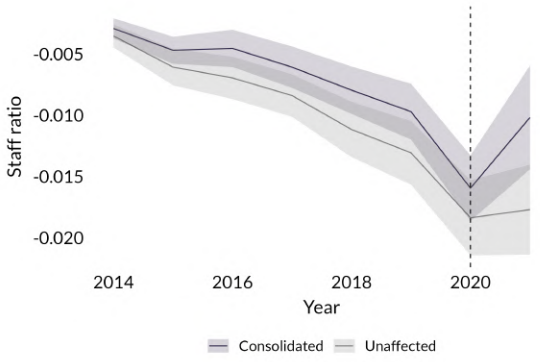
(a) Age at first promotion



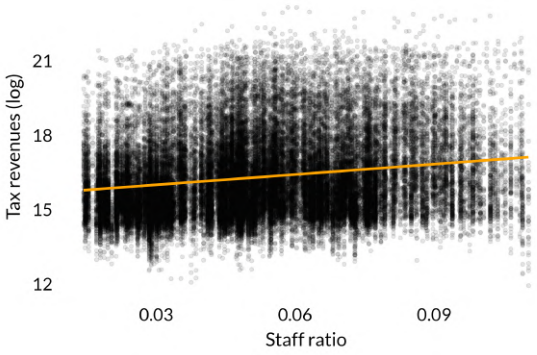
(b) Validation measure

Notes. This figure displays the definition and validation of the high-quality manager measure. The top 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 bottom 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.

Figure A16 — Consolidation, staff ratio, and tax revenues



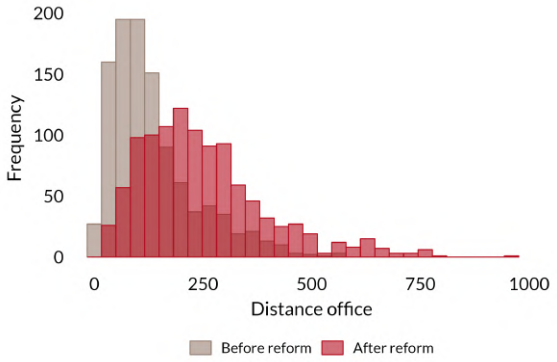
(a) Evolution of staff ratio by group



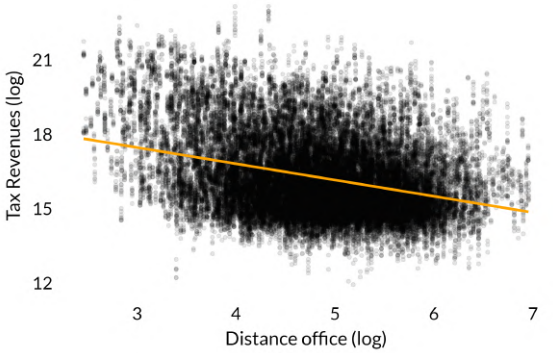
(b) Staff ratio and tax revenues

Notes. The left panel shows the evolution over time of the staff ratio, defined as the number of tax agents per 1,000 inhabitants in the jurisdiction, comparing *consolidated* and *unaffected* jurisdictions. The right panel reports the relationship between tax revenues and staff ratio. Each dot is a municipality-year observation. I also report a linear fit (solid line) with 95% confidence interval (shaded area).

Figure A17 — Consolidation, distance from tax office, and tax revenues



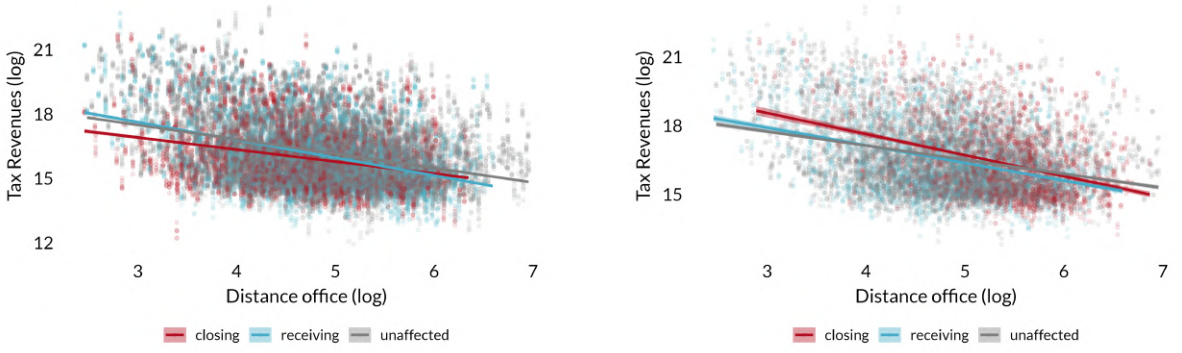
(a) Distance from tax office for *periphery* municipalities



(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).

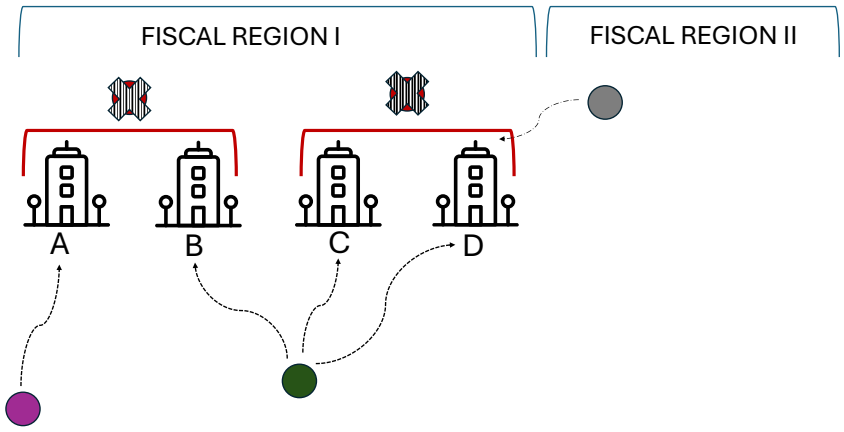
Figure A18 — Distance from tax office and tax revenues by exposure to the reform



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

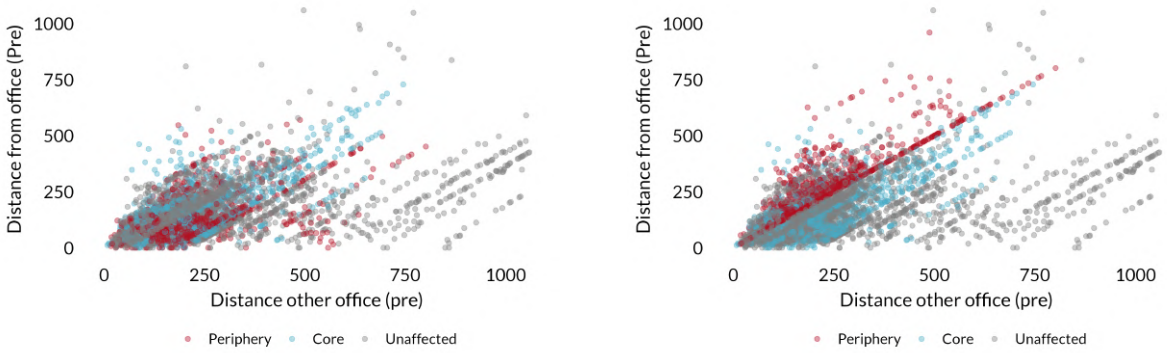
Notes. This figure reports the relationship between tax revenues and the travel distance (in minutes) from the jurisdiction tax office. Each dot is a municipality-year observation. It also shows the best linear fit (solid line) with 95% confidence interval (shaded area) separately for the three groups 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 A19 — 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).

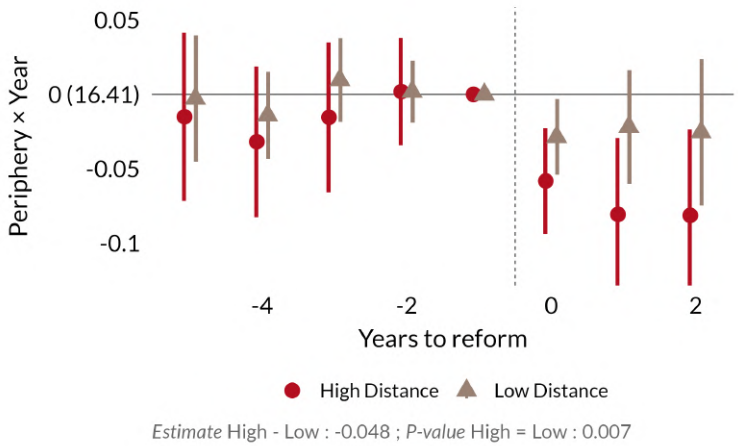
Figure A20 — Empirical strategy for distance



(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 *core* municipalities; gray dots denote *unaffected* municipalities.

Figure A21 — Mechanism: distance from office and revenues



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. 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.