Are Bigger Jurisdictions Better? The Effects of Local Consolidation*

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[PRELIMINARY VERSION]

Abstract

What is the optimal size of a jurisdiction? We study the consequences of increasing jurisdiction size using the 2015 NOTRe reform in France, which forced mergers of small intermunicipal entities. We implement a difference-in-differences design and leverage newly assembled, comprehensive data on public finances, services, employment, and housing transactions. We find no evidence of scale economies. Instead, consolidation triggers a spatial reallocation of public services away from peripheral places and toward political centers, and lowers house prices throughout the territory—by 5.9% after seven years—with the largest declines in the periphery. These results point to welfare losses from larger jurisdictions and to a widening of within-territory inequalities.

JEL: H11, H72, H42, R51, R31, D72

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1 Introduction

Larger jurisdictions entail a fundamental trade-off. On one hand, size allows governments to exploit economies of scale and internalize spillovers across neighboring areas. On the other, larger jurisdictions are less able to match the heterogeneous preferences of their residents (Tiebout 1956; Oates 1972; Alesina and Spolaore 2005). In practice, the pursuit of efficiency and scale economies has pushed many governments toward greater consolidation and centralization of subnational governance in recent decades (OECD 2019).

Yet despite the prominence of these trade-offs in the theoretical literature, empirical evidence on whether larger jurisdictions are actually welfare-improving remains scarce. The central obstacle is endogeneity: jurisdictional borders are man-made (Alesina and Spolaore 1997), and mergers are typically voluntary. A second obstacle is practical—boundary changes complicate the construction of consistent data over time.

We address the first challenge by exploiting France's 2015 NOTRe reform, which forced small jurisdictions to merge, redrawing local boundaries across nearly half of the country. We address the second through a large-scale collection of local-level data. Prior work has largely focused on measuring the effect on aggregate spending or taxation (e.g., Blom-Hansen et al. 2016; Breuillé et al. 2018), offering a partial view of what consolidation actually does. We instead provide a comprehensive analysis spanning local government budgets, public and private employment, public service delivery, and housing prices. Beyond aggregate effects, we document how consolidation redistributes resources within merged territories.

France has a long history of intermunicipal cooperation. Since 2010, all municipalities have been part of intermunicipal communities (ICs), inside which they jointly provide public services and raise revenues, so that ICs became a new level of governance above municipalities. The 2015 NOTRe law increased the minimum population of an IC to 15,000. This triggered a large redrawing of the IC maps throughout France, reducing the number of ICs by nearly 40% by 2017. We use this law as an exogenous shock to the size of jurisdictions. Our unit of analysis is a territory, defined by the boundaries of post-merger ICs in 2017. We use a difference-in-differences design, comparing territories that were formed of several ICs before the law and had to merge into one with territories already made of a single IC before the law that remained unchanged.

In short, our findings suggest that calls for the further centralization of local public services may be misplaced. First, we find no evidence of aggregate economies of scale: consolidation has no effect on total local spending or total public employment across the territory; our estimates rule out effects larger than 2.2% and 1.8%, respectively. We next turn to the study of public facilities to explore whether these null effects could mask an

expansion in service delivery. Instead, we find no change in the number of cultural or sport facilities available on the territory, and, if anything, a small decrease in the number of primary schools and health services.[1]

Second, we uncover large distributional effects. Exploiting data at the municipal level, we show that the null effect on overall public employment masks a 10.7% decrease in public employment in places far from the political center—defined as the consolidated IC's headquarter—compensated by a 7.8% increase in places close to it. Moreover, the negative effect on schools is exclusively driven by peripheral places, which experience a loss of 4.2% on average following the mergers. The distributional effects are stronger when comparing peripheral to central places than when studying the re-allocation from rural to urban places. Our preferred interpretation is a political economy story, whereby places that are further from the center of power often wield less influence. In line with this interpretation, we find that the loss of schools in the periphery is stronger in territories with larger power imbalance—as measured by the concentration of the population across the former ICs. Indeed, in territories dominated by one former large IC, this player can more easily impose its decisions than in territories that were previously fragmented between equal-sized partners.

Third, we find no improvement in economic activity, but a significant and gradual decrease in housing prices. Seven years after consolidation, housing prices are 5.9% lower in treated territories than in control ones. The negative effect materializes in both the center and the periphery, although it is largest for peripheral places—the ones losing public services. Considering house price capitalization as a measure of the willingness to pay for centralization, this implies a decline in the valuation of services provided by consolidated territories. In particular, it makes it unlikely that the decline in service delivery in the periphery was compensated by an increase in service quality.

Finally, we study spillover effects. A possible channel our local analysis may miss is that, although consolidation has negative effects on the treated jurisdictions, our empirical design may fail to capture external effects on nearby places. We therefore conduct a more macroeconomic analysis at the "département" (state) level, where we compare states with a large number of consolidations to those with a small number of consolidations. We find that states with a large number of consolidations see house prices fall, ruling out positive spillovers.

Overall, our findings show that consolidation deepens spatial inequalities in access to public services, concentrating provision in political centers at the expense of peripheral places. These dynamics speak to a growing concern across economics and political science: the un-

[1]The effect on health services is smaller and less robust than the one on schools, so we focus primarily on the latter when studying redistributive effects.

even geography of public services and the sense of being "left behind" have become powerful drivers of political discontent and support for populist movements (Rodríguez-Pose, 2018; Coquard, 2019; Algan et al., 2020; Cremaschi et al., 2024). Institutional consolidation, often pursued in the name of efficiency, can thus widen these territorial divides without delivering economies of scale.

2 Contributions

Our paper advances the literature on the optimal amount of decentralization and size of jurisdictions. Alesina et al. (2005) argue "our understanding of economic performance... can be greatly improved by bringing the issue of country size at the forefront of the analysis of growth." By studying the local setting, we capture hundreds of boundary reforms among thousands of jurisdictions, allowing us to shed light on the macro question from a local perspective. Doing so, we contribute to several strands of the literature.

A first related literature examines the effects of intermunicipal cooperation—either through municipal mergers or the creation of intermunicipal communities. This literature has mainly focused on the aggregate effect of consolidation on tax competition and revenues (Breuillé et al., 2018, Breuillé and Duran-Vigneron, 2025, Agrawal et al., 2025), or total spending (Frère et al., 2014; Bel and Warner, 2015; Blom-Hansen et al., 2016). [2] A few recent studies have focused on the local effects for individual municipalities. Tricaud (2025) identifies the costs faced by municipalities compelled to join intermunicipal communities, and Dahis and Szerman (2025) shows that service delivery improves in municipalities that split in Brazil.

In this paper, we provide a unified analysis by mobilizing a large range of outcomes and by measuring both the overall and distributional effects of consolidation. First, we overcome the typical endogeneity issues of voluntary mergers by exploiting the 2017 French reform that *forced* mergers in areas below the population threshold and where higher level authorities mapped out plans of who to merge with. Second, we analyze how expansions in jurisdiction size affect not only public finances, but also public employment, service provision, economic outcomes, and housing prices. Third, thanks to the use of local data, we can go beyond aggregate effects to assess the distributional consequences within a consolidated area. Fourth, our department-level analysis enables us to quantify spillover effects at a larger scale.

A second strand studies the political economy of centralization and the spatial allocation of public resources. Grossman and Lewis (2014) and Grossman et al. (2017) predict that centralization reallocates resources toward more politically influential or better-connected

[2]For a survey see the amalgamations and mergers section in Agrawal et al. (2024).

locations.[3] Empirical evidence—-largely from low-income settings—suggests that decentralization can improve allocation (Faguet, 2004), that proximity enhances accountability (Jha and Talathi, 2025), and that institutional quality declines with distance from centers of power (Michalopoulos and Papaioannou, 2014; see also Campante et al., 2019, Campante and Do, 2014). However, causal evidence on how changes in jurisdiction size affect within-jurisdiction allocation remains limited, particularly in high-capacity environments. Consistent with the evidence in developing countries, our paper shows that consolidation in France led to a loss of public services in areas far from the political center. We also show that the reallocation is stronger along this political dimension—reallocation from the periphery to the center—than along the density dimension—reallocation from rural to urban areas.

Finally, our paper relates to a growing literature on housing markets and decentralization. Hatfield et al. (2024), following Hatfield and Kosec (2013), finds that decentralization increases house prices using cross-sectional IV strategies, while Sovera (2025) shows that forced cooperation in Italy leads to price declines. We complement this work by embedding housing market responses within a broader set of outcomes and by examining within-jurisdiction heterogeneity in price effects. Our results show that the negative effect of centralization on housing prices is mainly driven by the spatial reallocation of public services.

3 Setting and Data

3.1 Intermunicipal cooperation

Historical background. France has historically had three tiers of subnational government: municipalities ("communes") at the lowest level, departments ("départements") at the intermediate level, and regions ("régions") at the top. With approximately 35,000 municipalities accounting for 40 percent of all municipalities within EU member states, France is one of the most fragmented countries in the OECD: nearly half of French municipalities have fewer than 500 inhabitants and over 70 percent fewer than 1,000.

This extreme fragmentation has long been viewed as an obstacle to the efficient provision of local public goods. Historical attempts to reduce the number of municipalities through mergers have repeatedly failed, with the most notable reform resulting in a mere 3 percent reduction in the total number of municipalities. Facing the impossibility of consolidating municipalities outright, the French government progressively turned to intermunicipal cooperation as a pragmatic alternative: rather than eliminating municipalities, cooperation groups them into a new layer of governance, i.e., the establishments for inter-municipal co-

[3]This issue is central in both developing and developed contexts (Bardhan, 2002).

operation (ICs), while preserving municipal elected councils and mayors. In practice, the fourth institutional tier has thus gradually emerged alongside the other three levels.

Although intermunicipal cooperation in France dates back to the late nineteenth century, ICs in their modern form rose to prominence following the 1992 and 1999 reforms, the latter introducing large financial incentives for municipalities to join them. This marked the beginning of near-universal intermunicipal coverage: whereas only half of municipalities belonged to an IC in 1999, 95 percent did by 2010. Then reforms in 2010 completed this process by making membership mandatory for the remaining isolated municipalities by 2014, and introduced a minimum IC population threshold of 5,000 inhabitants. Following the completion of intermunicipal coverage, the focus of reforms shifted toward consolidating existing structures, as explained in the next section.

Functions. ICs exercise, in lieu of municipalities, both mandatory functions imposed by law and optional functions transferred voluntarily by municipalities. The scope of mandatory competencies varies across IC type; it generally encompasses economic development, spatial planning, environmental management, and tourism.[4]

Several public services examined in this paper fall within the competency sphere of municipalities and ICs. Municipalities are in charge of the location and maintenance of primary schools ("maternelles" and "primaires"), and they can transfer this competence to the IC level.[5] Similarly, cultural facilities, sports facilities, as well as health centers fall within the optional competency block that municipalities may transfer to the IC. Middle schools and high schools, by contrast, fall under departmental and regional authority respectively. Once a function is transferred at the IC level, the associated budget becomes managed at the IC level and employees working on transferred functions move from municipal administrations to the IC administration.

In terms of fiscal capacity, ICs levy taxes on four main local tax bases (the residence tax[6], the property tax on developed and undeveloped land, and the business property tax).

[4]There are four types of ICs, which mainly differ in their population thresholds, degree of institutional integration, and scope of competencies. Communautés de Communes (CC) are generally smaller, more rural, and operate a more restricted mandatory set of tasks than Communautés d'Agglomération (CA), Communautés Urbaines (CU), and Métropoles, which exercise broader statutory functions (see Breuillé and Duran-Vigneron, 2025). Given that CCs constitute the majority of ICs (60%) and that the largest types of ICs were not impacted by the law, the analysis mainly focuses on CCs (see Section 4.1).

[5]While the school function was rarely transferred to the IC level before 2010, it became increasingly common over the last decade, and by 2018 around one third of ICs held this competence. More generally, a recent Senate report stresses that the central government increasingly coordinates directly with ICs rather than municipalities in its effort to reduce the number of schools across the territory—regardless of whether the school function has been transferred to them—something mayors complain about. See: https://www.senat.fr/rap/r24-900/r24-900_mono.html.

[6]The residential tax on primary residences was gradually phased out between 2020 and 2023, while the tax continues to apply to secondary residences.

Depending on the tax regime, ICs may either share these tax bases with municipalities or collect the entire business property tax themselves at the intermunicipal level.

Governance. Inside the IC, decisions are made by the intermunicipal council, which is composed of members drawn from the municipal councils of all member municipalities. Since 2014, voters in municipalities with more than 1,000 inhabitants simultaneously elect both their municipal councilors and their IC representatives through a single ballot, a reform that has strengthened the democratic accountability of ICs to local citizens and highlighted their increasingly autonomous role in local governance.[7] The number of seats allocated to each municipality is broadly proportional to its population. The president of the council is elected by its members and is generally the mayor of the largest city in the IC, where the IC headquarter is located. As shown by Abidi Perier and Merlin, 2025, the current seat-allocation rule leads to an over-representation of major cities at the expense of smaller, more rural municipalities.

3.2 The NOTRe Law and the consolidation of ICs

The loi NOTRe (August 7, 2015), the reform exploited in this paper, was the most ambitious attempt to strengthen the French intermunicipal landscape. It pursued three goals: efficiency (aligning IC perimeters with functional living areas to capture economies of scale and internalize spillovers), equity (pooling resources to reduce fiscal disparities across municipalities), and simplicity (consolidating an excessively fragmented institutional map).

The law raised the minimum population threshold for an IC from 5,000 to 15,000 inhabitants. Notwithstanding some exceptions, more than one third of existing ICs were estimated to require perimeter modifications.[8] Implementation was entrusted to the prefects—the State's delegated authority in each department whose main role is to ensure the implementation of national laws and regulations. They were required to draw up revised intercommunal maps "schémas départementaux de coopération intercommunale" (SDCI). Beyond the population threshold, SDCIs were expected to respect criteria of territorial coherence, including geographically contiguous territories without enclaves, and alignment with living

[7] In municipalities with fewer than 1,000 inhabitants, IC representatives are appointed among municipal representatives according to their rank within the municipal council, following the election of the mayor and deputy mayors.

[8] Exceptions were provided for i) ICs with a population density below 30 percent of the national average, ii) ICs in which at least half of constituent municipalities were classified as mountainous or located on islands, and iii) ICs in low-density departments, for which the threshold was adjusted proportionally by the ratio of departmental to national population density. A temporary exemption was additionally granted to ICs exceeding 12,000 inhabitants that had themselves resulted from a merger between January 2012 and the date of promulgation of the law.

areas. The SDCIs were drafted by the prefects, reviewed by the departmental commissions, and then submitted to the local governments. Although municipalities and ICs were consulted and could shape the proposals, the prefects were granted special power to override local disagreements. As a result, the choice of merger partners was largely imposed upon local governments.[9] The SDCIs were drafted between 2015 and 2016 and the IC mergers were enacted on January 1, 2017.

The aggregate impact on the intermunicipal map was substantial. The number of ICs declined from 2,062 in 2016 to 1,266 in 2017, corresponding to a 39 percent reduction in a single year. The average IC grew from 17 to 29 municipalities and from approximately 24,000 to 52,300 inhabitants.

Beyond redrawing intermunicipal boundaries, the NOTRe reform also reshaped how competencies were allocated within newly merged ICs. Compulsory competencies were harmonized "upward": those exercised by the most integrated former IC applied immediately across the entire merged territory. Optional and discretionary competencies, by contrast, entered a transitional harmonization process, during which they continued to be exercised only within the former IC boundaries until the new intermunicipal council decided whether to extend them territory-wide or return them, in whole or in part, to the member municipalities. Hence, the NOTRe law reshaped the spatial organization of public services and public employment both by enlarging ICs and by deepening their functional integration.

The NOTRe law differs from earlier reforms in a way that is consequential for identification, as it mandated the reorganization of ICs that were already formed and actively delivering services. Because fixed establishment costs had already been incurred, the reform primarily affected the scale of service provision, making it a particularly clean setting to test for economies of scale and, more broadly, the canonical arguments for consolidation.

4 Empirical Strategy

4.1 Treatment and Control Groups

We exploit the NOTRe law to assess the causal impact of consolidation. Our unit of analysis is a territory—defined by the perimeter of intermunicipal communities as of January 2017, after the mergers took place. The analysis focuses on mainland France, excluding overseas territories and Corsica. Before the law, some territories were fragmented into several ICs that were forced to merge in 2017. They constitute our treatment group. By contrast, some territories were made of a single IC that was large enough before the law and remained

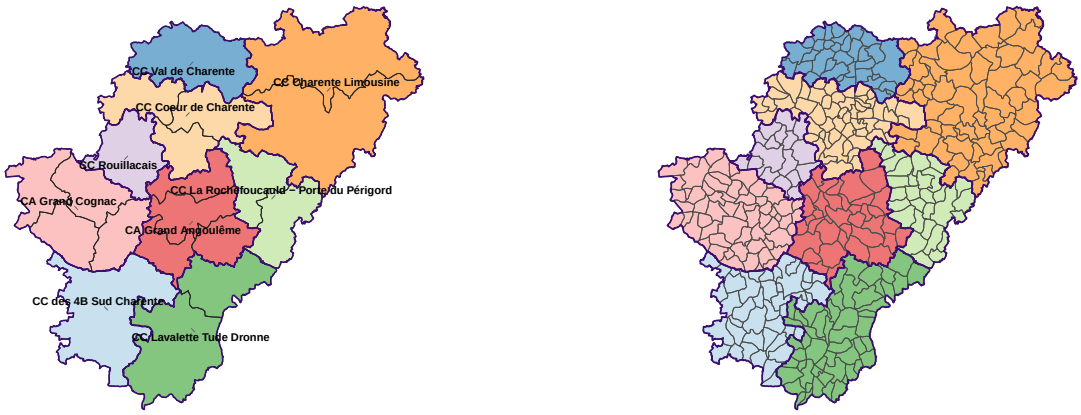
[9]See for instance: Finistère prefectural decree, p. 82.

unchanged. These territories constitute our control group.[10]

Figure 1 (a) illustrates our units of analysis, focusing on the Charente department: 2017 IC perimeters are delineated by thick lines, each shaded in a different color. Inside the treated territories, the grey lines represent the borders of the former ICs. As an example, the territory in red was fragmented into four different ICs before the law and merged into one (treated territory), whereas the territory just below in blue consisted of a single IC both before and after the law (control territory).

Figure 1 (b) further shows the municipal composition of each territory. All outcomes studied in this paper are measured at the territory level and are constructed as the sum of municipal and IC outcomes. For instance, when studying spending, we consider the sum of municipal and IC spending to measure total local spending. This is important because the mergers can affect the allocation of service provision between municipalities and ICs (see Section 3.2). Not accounting for it could lead us, for instance, to observe an increase in IC spending that is not due to the consolidation process but to the fact that the IC is in charge of additional services that were before managed by municipalities. Aggregating the two levels enables us to accurately capture the effect of consolidation on the territory.

FIGURE 1: ICs in the Charente département



(a) 2017 IC perimeters with pre-reform IC borders (b) 2017 IC perimeters with municipality borders

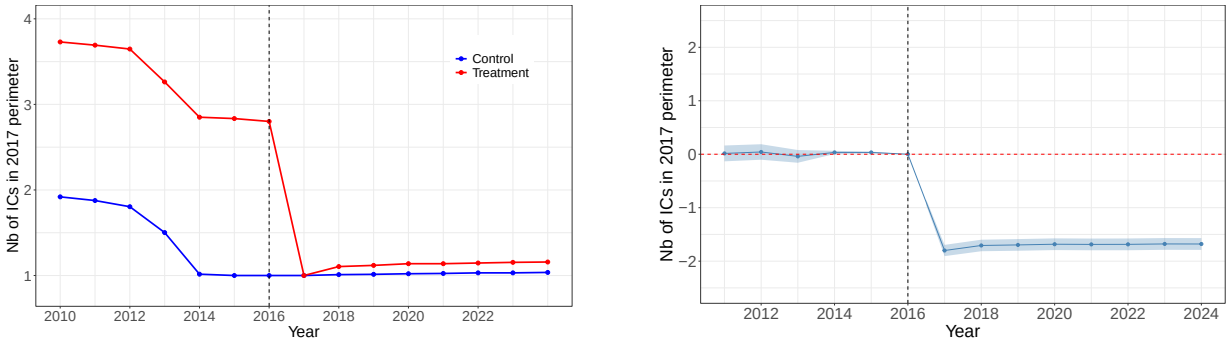
Notes: 2017 IC perimeters are delineated by thick lines, each shaded in a different color. The grey lines in Figure (a) represent the borders of the former ICs, while the grey lines in Figure (b) represent the borders of the municipalities.

[10] As indicated in Section 3.2, the law was enacted in 2015 but required compliance by 2017. Almost all mergers took place at the latest implementation date in January 2017. Only a few mergers took place between 2015 and 2016. We exclude these early cases from the analysis given that they could be considered as voluntary changes rather than imposed by the law. In particular, ICs that merged before the deadline were likely to differ systematically from other ICs, for instance, because local officials were more supportive of intermunicipal integration or because merger negotiations were already underway prior to the reform.

We impose a few additional restrictions. First, we remove "Métropoles" and "Communautés Urbaines," accounting for 30 ICs. These correspond to the most integrated and largest ICs (see footnote 4) and were almost all part of our control group, with no direct comparison in the treatment group. Second, we remove 30 consolidated ICs that were formed exclusively of ICs with a population larger than 15,000 inhabitants in 2015, and thus entirely exempt from the law. Finally, we exclude 31 ICs that changed boundaries after 2017 due to cross-border municipal mergers. In total, this led us to remove 91 observations (8%).

The resulting sample is a balanced panel of 1,089 territories: 507 in the treatment group (46.6 percent) and 582 in the control group. To illustrate the variation we exploit, Figure 2 plots the average number of ICs within treated (red) and control (blue) territories from 2010 until 2024. Both groups followed similar trends before the law.[11] Control territories reached full consolidation by 2014 and have since consisted of a single IC. By contrast, treated territories still comprised an average of about three ICs in 2014 and converged to the control group in 2017, as a result of the NOTRe law.[12]

FIGURE 2: Evolution of the average number of ICs



Notes: The left-hand side graph plots the average number of distinct ICs in treated territories (in red) and control territories (in blue). The right-hand side graph plots the coefficients obtained using equation 2 described in Section 4.4, taking as outcome the number of ICs.

4.2 Data

The empirical analysis draws on several administrative data sources, mostly from 2010 to 2024.

[11] The common downward trend between 2010 and 2014 reflects the gradual implementation of the 2010 RCT reform that imposed a population threshold of 5,000 inhabitants.

[12] In Figure 2 panel (a), the red line is slightly above one after 2017 due to a small number of municipalities changing ICs after the law.

IC perimeters and characteristics. The composition of ICs over time, including the list of member municipalities, legal status, population, and the location of the IC headquarters, is drawn from BANATIC, the national database on intermunicipal cooperation maintained by the Ministry of the Interior. These data are used to identify which ICs merged as a result of the NOTRe law, and to construct the treatment and control groups described in 4.1. They also allow us to identify the political center of each IC, defined as the municipality hosting the IC headquarters, to analyze distributional effects in 5.2 and to compute the number of ICs at the department level for the spillover analysis presented in 5.4.

Public finances. Consolidated fiscal data are drawn from DGFIP (the French tax and public finance administration in charge of the fiscal accounts of the State and local governments), combining both IC and municipal fiscal accounts within a common area. These data provide annual information on total revenues and expenditures per capita, as well as the share of expenditures allocated to equipment, purchases of goods and services, and personnel costs. The data are missing for 79 ICs (5.5% of the sample) for at least one year between 2010 and 2022. In Section 5.1, we focus on a balanced panel when considering public finance outcomes, and the results are robust to using the full unbalanced sample.

Public employment. Public employment data come from SIASP (Système d'Information sur les Agents des Services Publics), an exhaustive administrative register compiled by INSEE, covering all public-sector employees in France and accessible via the CASD (Centre d'Accès Sécurisé aux Données), a secure remote infrastructure for access to confidential microdata. We focus on public employment in local governments, defined as employees hired by municipalities and ICs. We exclude employees of the state and hospital civil services. The data include, at the territory and yearly level, the total number of hours worked and total wage bill—which we use to measure public employment.

Private sector activity. Data on private sector employment are drawn from the BTS (*Base Tous Salariés*), an exhaustive employer-employee register produced by URSSAF and INSEE, also accessible via the CASD. We focus on gross wage and hours worked within each territory, as measures of local economic activity.

Public service facilities. Data on public service facilities come from the BPE (*Base Permanente des Équipements*), an annual census of facilities and services compiled by INSEE. It is available every year during our period of interest, except in 2021 when the census was not conducted. We focus on services that (i) fall under IC competencies, and are therefore

potentially affected by consolidation, and (ii) have been collected consistently over time, so that they can be used in a panel over our analysis period.[13] The analysis covers four categories of facilities: health services (general practitioners, dentists, and gynecologists), primary schools ("maternelles" and "primaires"), cultural facilities (cinemas), and sports facilities (tennis courts, bocce courts, large sports fields, and multisport facilities). We additionally collect data on high schools, which we use as a placebo, since ICs have no authority over these facilities. All facility counts are geocoded to the municipal level, enabling both aggregate and spatially disaggregated analysis.

Urban-rural classification. In order to identify rural and urban areas within the territories, we rely on the INSEE's seven-level density classification (G7 classification), which groups municipalities based on both their own population density and that of the surrounding area. We collapse these categories into a simple urban-versus-rural indicator. This classification is used alongside distance quartiles to the IC headquarters to study the distributional effects of consolidation documented in 5.2.

Housing prices. Transaction-level data on housing prices come from DV3F, an enriched version of the DVF (*Demande de Valeurs Foncières*) register compiled by the DGFIP and processed by Cerema, which augments raw transaction records with additional property characteristics.[14] To isolate price changes from compositional shifts in the housing stock, we construct a hedonic price index following the two-step procedure developed in Tricaud, 2025: we first regress the log price per square meter on a rich set of transaction characteristics (including house age, number of rooms, living space, parking spaces, and dependency space) estimated separately for each year, and then aggregate the residuals augmented by the estimated constant to the IC level. The resulting index captures quality-adjusted price variation across ICs and over time, and serves as a summary measure of the capitalized welfare effects of consolidation.

4.3 Descriptive statistics

As shown in Appendix Figure A1, treated and control territories are both spread across all regions of France, ensuring broad spatial variation in treatment status.

Table 1 presents descriptive statistics for the 507 treated and 582 control territories prior

[13] We assess consistency using three cumulative criteria: whether the BPE documentation reports a break in the collection method; whether the variable belongs to the set used by INSEE in its own panel data (*base en évolution*); and whether the national trend exhibits any major break.

[14] For this outcome, data are unavailable for Alsace-Moselle territories.

to the reform, as of 2016. As expected, treated territories are on average more populous and less dense. However, the two groups are very comparable in other socio-demographic characteristics, including immigrant share, unemployment, age composition, share of workers, educational attainment, homeownership, income, and political preferences.

TABLE 1: Descriptive statistics

	Treated (N=507)				Control (N=582)			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Population	37567	39225	4939	306306	30117	36917	3869	433221
Pop. density	85.4	103.6	6.4	1030.8	129.9	202.1	8.2	2426.9
Immigrants (%)	4.6	2.9	0.7	22.6	5.5	4.0	0.7	32.1
Unemployment (%)	12.2	3.0	5.0	23.9	12.5	3.6	3.3	25.8
Pop. 0–14 (%)	17.7	2.6	10.4	24.9	17.9	2.7	11.0	25.2
Pop. 65+ (%)	21.9	4.7	11.9	38.1	21.4	5.3	5.6	35.7
Working-age pop. share (%)	60.0	2.6	51.6	66.5	60.4	3.2	50.9	69.3
High-skill workers (%)	5.2	2.2	1.3	17.1	5.8	3.1	2.1	23.7
Low-skill workers (%)	15.0	3.4	6.8	30.9	14.7	3.8	3.7	31.1
No degree (%)	33.1	5.2	19.7	47.6	32.0	6.0	14.9	47.7
College degree (%)	21.3	5.1	11.4	41.0	22.6	6.6	10.9	55.2
Homeownership (%)	70.0	7.7	36.4	85.3	69.6	8.1	40.8	85.0
Median income (EUR)	20218	1800	16268	32204	20747	2688	15954	37596
Share agri. workers (%)	12.6	7.9	0.2	45.3	10.8	8.2	0.1	44.0
Share industry workers (%)	13.7	5.7	2.8	35.4	13.6	6.3	2.7	41.5
Share service workers (%)	64.0	8.8	39.9	84.4	66.2	9.5	39.7	91.5
Left-wing vote share (%)	45.6	10.9	3.2	73.7	44.5	11.2	16.6	74.1
Far-right vote share (%)	14.2	4.9	4.1	31.4	15.1	5.6	3.2	42.1

Notes: Socio-demographic variables come from the 2016 census. Electoral variables come from the first-round of the 2012 parliamentary election.

4.4 Identification

We implement the following difference-in-differences specification for territory i in year t:

$$Y_{it} = \alpha + \beta Treat_i \times Post_t + \zeta_i + \zeta_t + \varepsilon_{it} \tag{1}$$

where Y_{it} denotes various outcomes of interest, ζ_i are territory fixed effects, and ζ_t are year fixed effects. Then, $Treat_i$ equals one if the territory is in the treatment group (i.e., consolidated in 2017), and $Post_t$ equals one if the year is 2017 or later. Because the treatment

year is the same for all territories, our empirical design is not staggered and thus *not* subject to recent critiques concerning identification from variation in timing. Given the panel nature of the data, we cluster standard errors ε_{it} at the territory level.

Following Tricaud (2025), all outcome variables are normalized by the territory's 2016 total population. Thus, if studying total spending for instance, the dependent variable can be interpreted as spending per 2016 capita. This normalization facilitates comparison across territories of different sizes.

The identification assumption is that, absent the law, treated and control territories would have followed the same trajectory. The main threat is selection into treatment, which the design of the NOTRe law makes unlikely (see Section 3.2). First, ICs that wanted to merge could have done so before the law; that they did not, despite financial incentives, indicates that the mergers were driven by the population threshold rather than by self-selection. Second, the 15,000-inhabitant threshold and the contiguity requirement sharply limited the available merger partners. Third, prefects could override local resistance and impose the new maps. To provide direct empirical support for the parallel-trends assumption, we estimate event-study specifications, replacing $Post_t$ in (1) with event-time indicators for each year, omitting 2016:

$$Y_{it} = \alpha + \sum_{k \neq 2016} \beta_k Treat_i \times 1(k = t) + \zeta_i + \zeta_t + \varepsilon_{it}. \tag{2}$$

5 Main results

5.1 Overall Effects on Public Finances, Employment, and Services

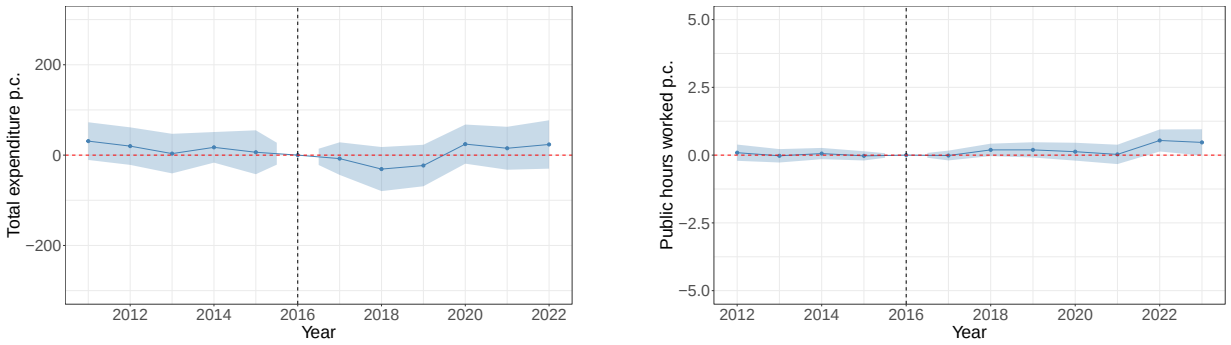
Economies of scale in public service delivery are often cited, both in the academic literature and in the institutional reports, as a justification for government consolidation. Yet convincing empirical evidence on this question remains scarce, because most changes in jurisdiction size result from voluntary mergers (Reingewertz, 2012).

We first examine the effect of consolidation on total spending and total public employment. At the territory level, we compute total spending as the sum of municipal and intermunicipal spending per capita (using the 2016 population), and total public employment as the total number of hours worked of local public workers, also per capita. As shown in Figure 3, treated and control territories followed parallel trends in the pre-reform period. In the post-reform period, nearly all coefficients are close to zero and statistically insignificant. The one exception is a small increase in hours worked starting six years after the reform.

Table 2 presents the formal results. Consistent with the graphical evidence, pre-trends are insignificant and post-reform effects on total spending and public employment are small

and statistically insignificant (columns 1 and 3). Based on our estimates, we can reject any effect larger than 2.2 and 1.8% on overall spending and public employment, respectively. We likewise find no effect on total revenue (column 2), nor on the total public wage bill (column 4). Turning to spending categories, consolidation produced a small composition effect (Appendix Table A1 and Figure A2): a decrease in equipment spending (2.8%), compensated by an increase in both purchasing expenses and personnel expenditures (2.6% and 1.6%, respectively).

FIGURE 3: Impact on Spending and Public Employment



(a) Total spending

(b) Number of Hours Worked

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcomes are total spending (figure (a)) and the number of hours worked by public employees in the territory (figure (b)), both divided by the number of inhabitants in 2016.

TABLE 2: Impact on the Budget and Public Employment

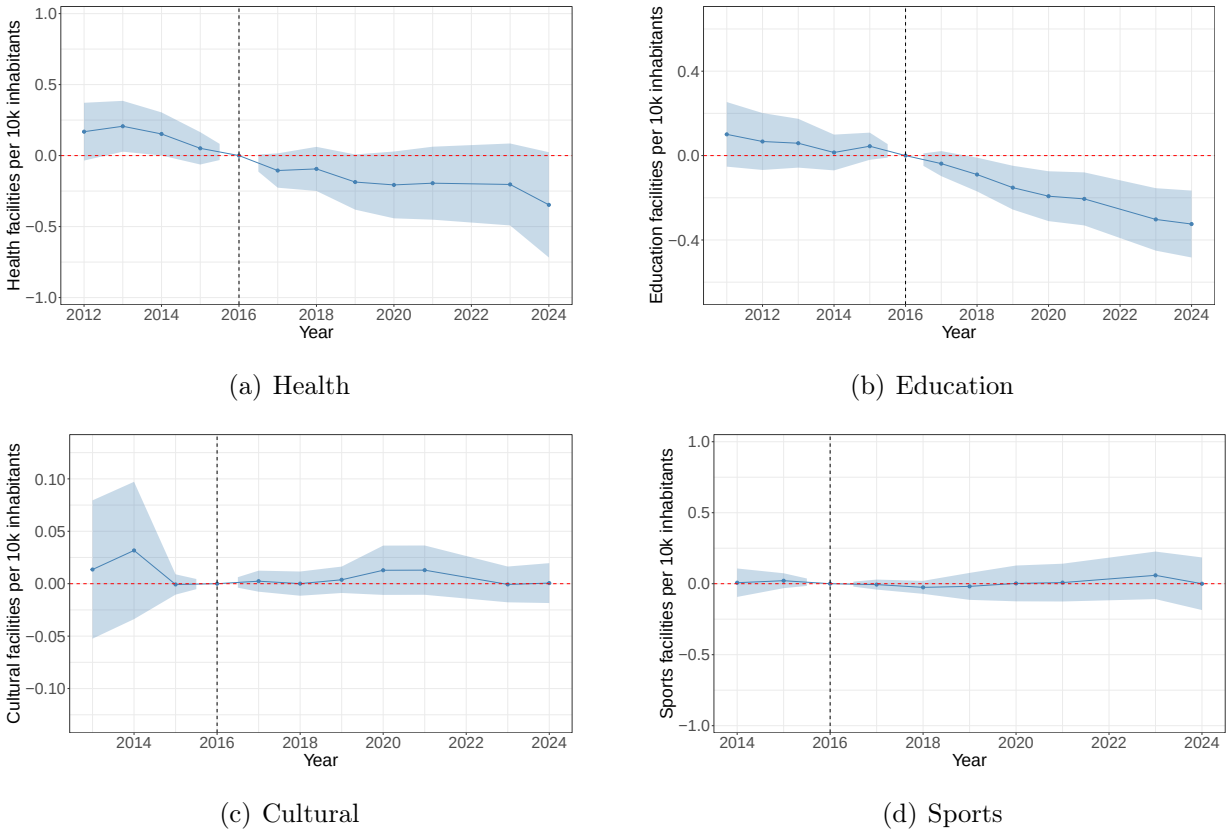
	Expenditure	Revenue	Hours worked	Public wage bill
Treated × Post	-11.45	-16.22	0.20	0.62
	(14.74)	(15.69)	(0.15)	(2.41)
Pre-trend p-value	0.546	0.326	0.359	0.260
Pre-treatment mean (T)	1,867.53	2,098.01	28.69	370.97
Year FE	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes
Observations	12,348	12,348	13,068	13,068

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. In column 1 (resp. 2), the outcome is total spending (resp. total revenue), computed as the sum of municipal and IC spending on the territory. In column 3, the outcome is the total number of hours worked by public employees in the territory. In column 4, the outcome is the total public wage bill. All outcomes are divided by the number of inhabitants in 2016. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These results suggest that consolidation did not deliver the economies of scale policymakers had hoped for. One possibility, however, is that any savings were offset by the con-

solidated IC providing new, expanded public services, leaving overall spending unchanged. To test this, we examine the number of public facilities in the territory. Table 3 and Figure 4 report the effect of consolidation on health services (general practitioners, dentists, and gynecologists), primary schools, cultural facilities (cinemas), and sports facilities (tennis courts, bocce courts, large playing fields, and multi-sport facilities). Again the effects are modest and, if anything, point to a decline in service availability. We find a decrease of 0.31 in the number of doctors and of 0.23 in the number of schools, both corresponding to a 2 percent decrease compared to the pre-reform mean. We detect no effect on cultural or sports facilities. The results are robust if we use a Poisson regression model (Appendix Table A2).

FIGURE 4: Impact on Public Service Facilities



(a) Health (b) Education

(c) Cultural (d) Sports

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcomes are the number of health facilities (figure (a)), schools (figure (b)), cultural facilities (figure (c)), and sports facilities (figure (d)). Health counts the number of general practitioners, dentists, and gynecologists in the territory; Education counts the number of primary schools ("maternelles" and "primaires"); Culture counts the number of cinemas; Sports counts the number of tennis courts, bocce courts, large playing fields, and multi-sport facilities. All outcomes are expressed per 10,000 inhabitants (using the 2016 population).

The health result, however, is sensitive to pre-trends: Figure 4(a) shows some pre-existing decline in health facilities, and accounting for it by starting the analysis in 2015 reduces the coefficient by almost a third. We therefore focus on schools, where the effect is more robust.

Given the overall reduction in the number of schools in France over this period, this effect is best interpreted as reflecting school closures; we investigate where these closures took place in the next section. Consistent with the closures being driven by IC consolidation, we find no effect on high schools, which are managed at the regional level and should therefore be unaffected by local consolidation (Appendix Table A3).

Overall, we find no evidence that consolidation led to an expansion of public services; if anything, it produced a closure of facilities.

TABLE 3: Impact on Public Service Facilities

	Health	Education	Cultural	Sports
Treated × Post	-0.307***	-0.234***	-0.007	-0.007
	(0.113)	(0.076)	(0.017)	(0.050)
Pre-trend p-value	0.197	0.258	0.304	0.693
Pre-treatment mean	13.401	11.157	0.415	19.972
Year FE	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes
Observations	13,068	14,157	11,979	10,890

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. In column 1 (resp. 2, 3, and 4), the outcome is the number of health facilities (resp., schools, cultural facilities, and sports facilities). Health counts the number of general practitioners, dentists, and gynecologists in the territory; Education counts the number of primary schools ("maternelles" and "primaires"); Culture counts the number of cinemas; Sports counts the number of tennis courts, bocce courts, large playing fields, and multi-sport facilities. All outcomes are expressed per 10,000 inhabitants (using the 2016 population). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Distributional Effects

The limited evidence of changes in expenditures and public services may mask significant heterogeneity in where spending flows. Before the reform, the treated territories were divided into several ICs, each with its own headquarters and fiscal autonomy; after the mergers, only one remains, reinforcing political and administrative centralization at the expense of peripheral areas. Beyond political and administrative centralization, territories may also have incentives to concentrate facilities in the densest areas in search of economies of scale. We therefore explore whether consolidation shifts the distribution of public services between the center and the periphery, and between urban and rural areas within the territory.

To do so, we leverage data at the municipal level and group the municipalities in each territory in two ways. The first captures geographical proximity to the political center of the IC. For each municipality, we compute the crow-fly distance to the municipality hosting the IC's headquarter, and sort municipalities into quartiles. We then estimate separate DiD regressions for the nearest 25% and the farthest 25%, in each case comparing the top (or

bottom) quartiles in treated territories to the top (or bottom) quartiles in control territories. The second grouping splits municipalities within the territory into urban and rural, as defined in Section 4.2. We further test for the significance of the differences between the groups by adding an interaction term in our baseline regression.[15]

We focus on public employment and public facilities that can be geocoded.[16] The public services we consider are well described by Hirsch (1959)'s two main categories: centrally integrated services (e.g., city hall) and horizontally integrated services (e.g., schools). Under his classification, the latter perform best when located close to residents, whereas centrally integrated services can often be centralized at lower cost. Still, centralizing administrative functions may create additional costs and resident dissatisfaction if their placement is not perceived as equitable. More broadly, a growing literature highlights the closure of local public services and increases in the distance to service provision as important drivers of discontent and collective protest (Hirschman, 1972; Rodríguez-Pose, 2018; Coquard, 2019; Algan et al., 2020; Cremaschi et al., 2024).

TABLE 4: Impact on Public Employment Across the Territory

	All	Center (Q1)	Periphery (Q4)	Urban	Rural
Treated × Post	0.20	8.24***	-2.39***	1.48***	-1.06*
	(0.15)	(1.36)	(0.40)	(0.53)	(0.57)
p-value		Q1 = Q4: 0.000		Rural = Urban: 0.002	
Pre-trend p-value	0.359	0.000	0.011	0.021	0.215
Pre-treatment mean	28.69	106.07	22.38	36.87	20.01
Year FE	Yes	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes	Yes
Observations	13,068	12,924	13,068	12,360	12,648

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the total number of hours worked by public employees in the territory, divided by the number of inhabitants in 2016. Column 2 (resp. 3) only considers employment located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Column 4 (resp. 5) only considers employment located in urban (resp. rural) municipalities, excluding territories composed of only urban or rural areas. The third row reports the p-value of the difference between the coefficients for the center and the periphery, and for urban and rural areas, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 and Figure 5 show the distributional effects on public employment.[17] The prior null effects mask significant heterogeneity. Municipalities near the political center experience

[15]Specifically, we estimate the following regression: $y_{i,t,g} = \beta(D_i \times \text{Post}_t) + \gamma(D_i \times \text{Post}_t \times \mathbf{1}[g = 1]) + \mu_{i,g} + \lambda_{t,g} + \varepsilon_{i,t,g}$, where g denotes the group (center vs. periphery or urban vs. rural), and where $\mu_{i,g}$ and $\lambda_{t,g}$ denote IC×group and year×group fixed effects, respectively. The reported p-value is that of the interaction coefficient γ.

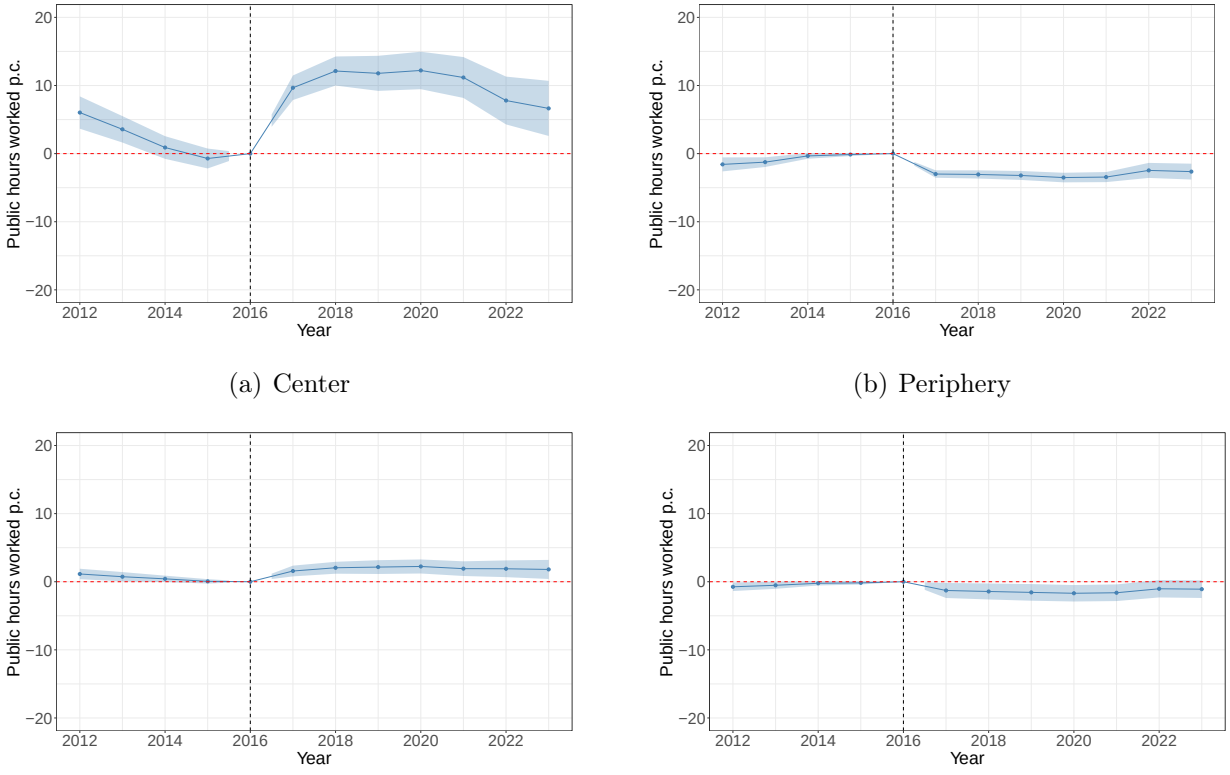
[16]In contrast, budget data do not tell us where spending is actually directed.

[17]The graphs show slight pre-trends prior to the reform, but in every case working in the opposite direction of the post-reform effects, suggesting we underestimate the causal effects.

an increase of 8.2 public hours worked per inhabitant, corresponding to 7.8% of the pre-treatment mean while municipalities that are further away experience a 10.7% decrease. Similarly, urban municipalities experience a 4% increase in public employment, compared to a 5.3% decrease for rural municipalities. Both differences are significant at the 1 percent level. Interestingly, the percent change when looking at the bottom quartile based on distance to the headquarter is twice as large as when looking at rural municipalities.

We conclude that more remote places are likely to be harmed: public employees previously working out of remote municipalities appear to be transferred to more central municipalities. This pattern is consistent with the concentration of administrative functions and local public services within the political and administrative core of the enlarged ICs. It also suggests that intermunicipal consolidation may have strengthened existing core-periphery inequalities in local public service provision.

FIGURE 5: Impact on Public Employment Across the Territory



(a) Center

(b) Periphery

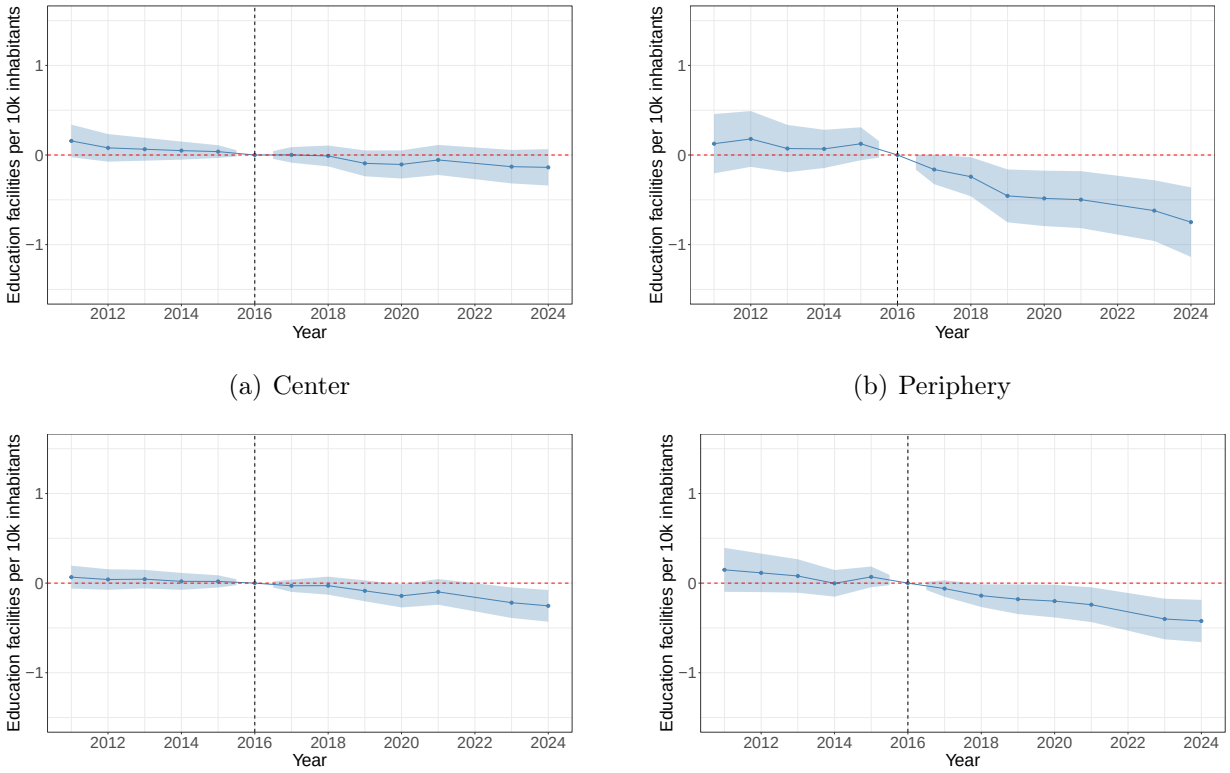
(c) Urban

(d) Rural

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the total number of hours worked by public employees in the territory, divided by the number of inhabitants in 2016. Figure (a) (resp. (b)) only considers employment located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Figure (c) (resp. (d)) only considers employment located in urban (resp. rural) municipalities, excluding territories composed of only urban or rural areas.

To study public service provision more directly, we repeat this exercise for the number of schools. Recall that Table 3 shows that the number of schools in the territory declines after consolidation, suggesting that consolidated ICs closed some schools within their borders. Table 5 and Figure 6 show that these closures fall mainly on remote municipalities. We see little change in municipalities closest to the headquarter, but a 4.2% decline in the number of schools in the periphery. Instead, we do not detect meaningful differences between urban and rural municipalities: the difference between the point estimates is not significant, and the percentage effect is similar in both places. Given that distance to the headquarter appears as the dominant factor explaining which places lose the most from consolidation, we focus on this aspect when running additional heterogeneity analysis in the rest of the paper.[18]

FIGURE 6: Impact on Schools Across the Territory



(a) Center (b) Periphery

(c) Urban (d) Rural

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the number of primary schools in the territory, per 10,000 inhabitants (using the 2016 population). Figure (a) (resp. (b)) only considers schools located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Figure (c) (resp. (d)) only considers schools located in urban (resp. rural) municipalities, excluding territories composed of only urban or rural areas.

[18] Appendix Figure A3 shows the effects for the second and third quartiles, in order to see the effect on the full distribution. These results again compare treated places in a given quartile to comparison places in that quartile. Appendix Table A4 shows that there is limited heterogeneity in other public services.

TABLE 5: Impact on Schools Across the Territory

	All	Center (Q1)	Periphery (Q4)	Urban	Rural
Treated × Post	-0.234***	-0.141	-0.554***	-0.154**	-0.303***
	(0.076)	(0.089)	(0.166)	(0.069)	(0.115)
p-value		Q1 = Q4: 0.016		Rural = Urban: 0.214	
Pre-trend p-value	0.258	0.454	0.374	0.763	0.157
Pre-treatment mean	11.157	10.096	13.335	9.287	14.042
Year FE	Yes	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes	Yes
Observations	14,157	14,157	14,157	13,390	13,702

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the number of primary schools in the territory, per 10,000 inhabitants (using the 2016 population). Column 2 (resp. 3) only considers schools located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Column 4 (resp. 5) only considers schools located in urban (resp. rural) municipalities, excluding territories composed of only urban or rural areas. The third row reports the p-value of the difference between the coefficients for the center and the periphery, and for urban and rural areas, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Overall, places that end up far from the new headquarter after consolidation lose public services. Our interpretation is a political economy story whereby places that are further from the center of power often wield less influence over their local constituencies and therefore are more exposed to service closures. This is particularly important for schools, which play a key role in the residential attractiveness and long-term vitality of peripheral municipalities.

We test this interpretation further by leveraging variation in the distribution of the decision-making power within the territory. If this interpretation is correct, we should see that the periphery is particularly penalized when the merger involved one large dominant IC, as opposed to a merger occurring among similarly-sized partners, as, in the former case, the largest player can more easily impose its decisions. To capture this, we construct an Herfindahl index, which takes on a relatively small value if the pre-merger ICs were relatively equal sized (in terms of population) but a high value if there was a dominant IC.

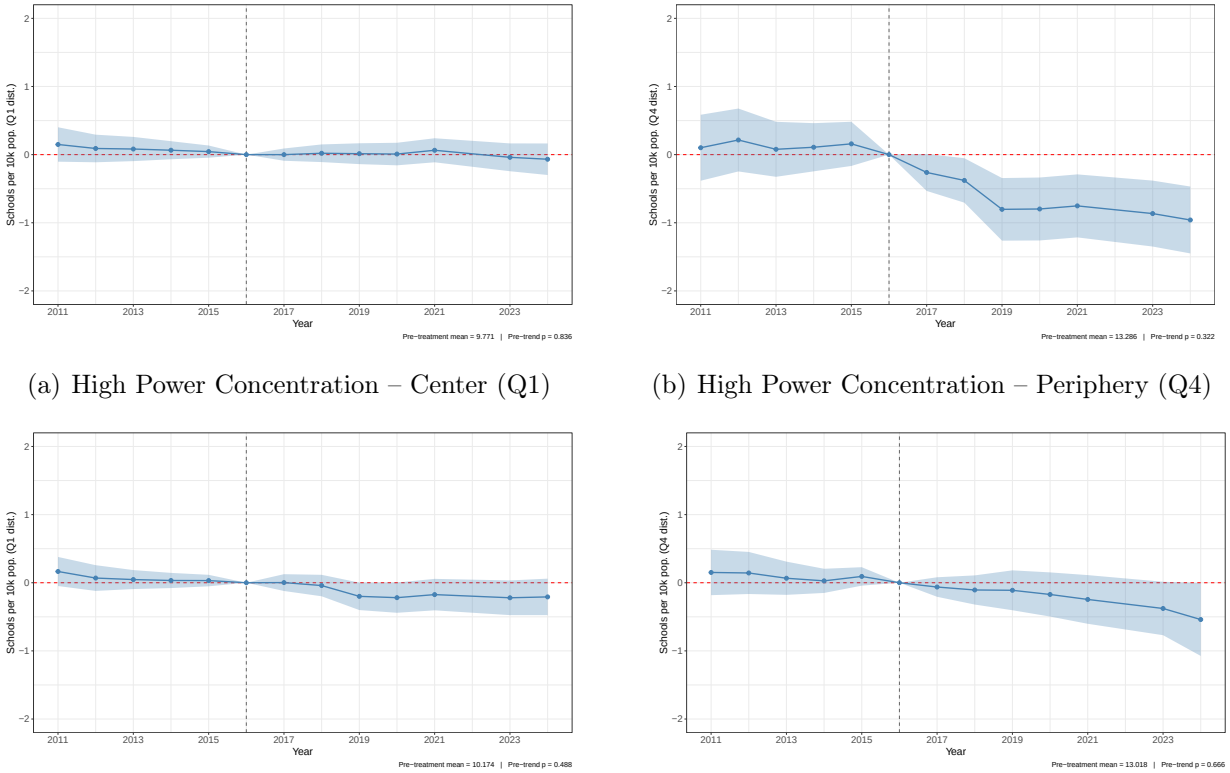
Table 6 and Figure 7 show the results. The difference between the center and the periphery is most pronounced in treated territories that initially had a large power imbalance. In territories with an above-median Herfindahl value, the decrease in the number of schools is solely driven by the periphery and is stronger than in the full sample (-0.89 vs. -0.61). In contrast, in territories where the power balance was more equal, the effect in remote places is lower (-0.34) and is not statistically different from the effect in central places. This result suggests that the initial political power distribution is critical: places with dominant ICs appear to have forced more closures on the smaller places that they merged with.

TABLE 6: Impact on Schools Across the Territory, by Power Concentration

	High Power Concentration		Low Power Concentration	
	Center (Q1)	Periphery (Q4)	Center (Q1)	Periphery (Q4)
Treated × Post	-0.072	-0.798***	-0.209*	-0.311*
	(0.104)	(0.251)	(0.118)	(0.172)
p-value	Q1 = Q4: 0.005		Q1 = Q4: 0.567	
Pre-trend p-value	0.836	0.322	0.488	0.666
Pre-treatment mean	9.771	13.286	10.174	13.018
Year FE	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes
Observations	10,855	10,855	10,868	10,868

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the number of primary schools in the territory, per 10,000 inhabitants (using the 2016 population). Columns 1 and 3 (resp. 2 and 4) only consider schools located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Columns 1 and 2 (resp. 3 and 4) further focus on territories with high power concentration, as reflected by an above-median Herfindahl value (resp. low power concentration, as reflected by a below-median Herfindahl value). The third row reports the p-value of the difference between the coefficients for the center and the periphery. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 7: Impact on Schools Across the Territory, by Power Concentration



(a) High Power Concentration – Center (Q1)

(b) High Power Concentration – Periphery (Q4)

(c) Low Power Concentration – Center (Q1)

(d) Low Power Concentration – Periphery (Q4)

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the number of primary schools in the territory, per 10,000 inhabitants (using the 2016 population). Figures (a) and (c) (resp. (b) and (d)) only consider schools located in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. Figures (a) and (b) (resp. (c) and (d)) further focus on territories with high power concentration, as reflected by an above-median Herfindahl value (resp. low power concentration, as reflected by a below-median Herfindahl value).

5.3 Economic Effects

In this section we study the effects of further centralization on private sector employment to capture economic effects. In addition, we study house prices and wages in the territory as measures of the incidence of the consolidation. These incidence measures also act as welfare metrics. Indeed, even if consolidation did not lead to economies of scale and, if anything, reduced the number of facilities, it could have still improved the quality of public services—something that is inherently difficult to measure at the local level. Similarly, even if the center has not experienced losses in public services, it is not necessarily better off if the central neighborhoods become more congested. We thus use prices as a capitalization

measure that reveals willingness to pay of the new IC policies (Oates, 1969).[19]

Table 7 shows the effect on hours worked, hourly wages, and house prices. Figure 8 shows the corresponding event study graphs. Focusing first on the economic effects, we see a very small decline in the number of hours worked after consolidation, suggesting that consolidation did not improve private employment.

With respect to wages, we divide the aggregate wage bill by total hours worked. We see that hourly wages are basically unchanged, consistent with consolidation not changing the composition of the labor force and thus not changing the average job in the territory.

Regarding house prices, we exploit data on housing transactions. As explained in Section 4.2, to address any compositional changes and to isolate a pure price effect, we estimate a hedonic model by regressing the log price per square meter on dwellings' characteristics. The resulting index can be interpreted as the price per square meter of a reference dwelling in the territory. We see that housing prices fall relative to the comparison group by 3.7% after consolidation. Given that house prices are rising in France in this period, the effect should be interpreted as a relative slowing of the rate of appreciation in treated territories. Beyond the negative effect, several aspects of the event study in Figure 8 are worth noting. First, there is no pre-trend in house prices prior to the reform. Second, the relative decline in house prices occurs gradually after the reform. Such a dynamic process is expected as household mobility and capitalization into transaction prices occurs gradually over time.

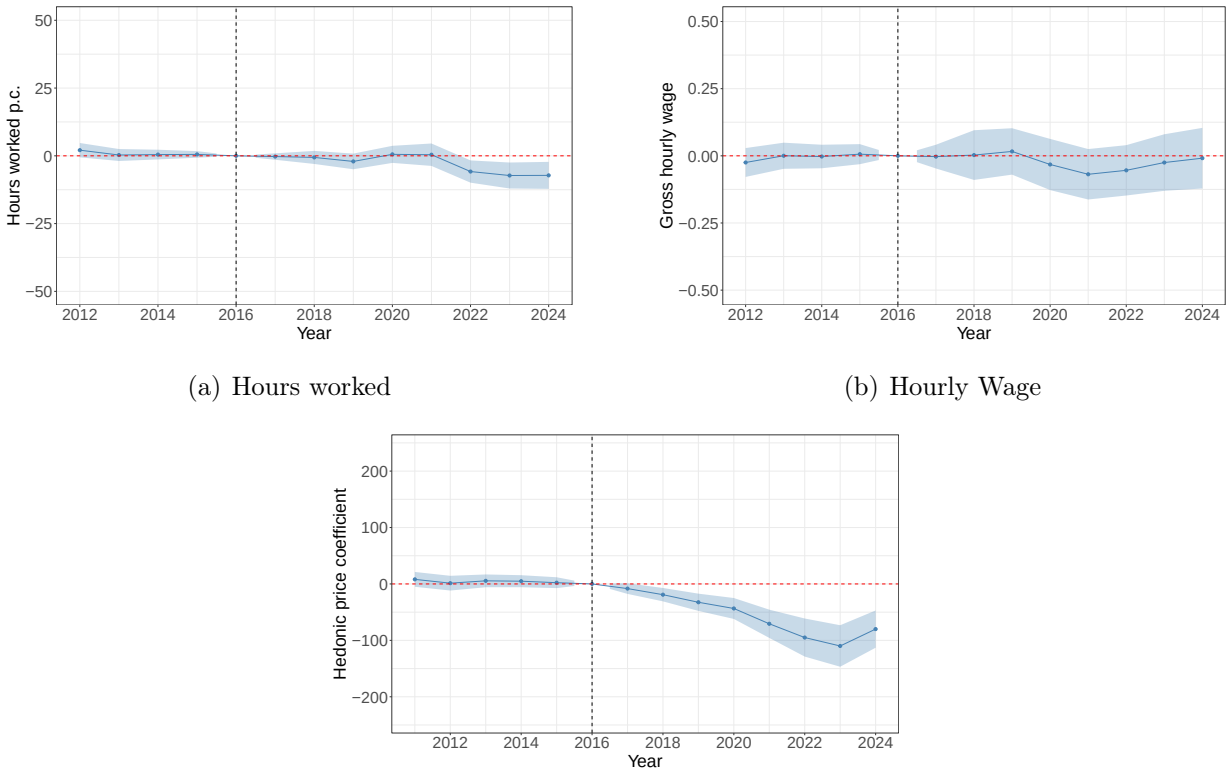
TABLE 7: Impact on Economic Activity and Housing Prices

	Hours worked	Gross Hourly Wage	Housing price
Treated × Post	-3.45*	-0.017	-61.03***
	(1.92)	(0.042)	(11.14)
Pre-trend p-value	0.038	0.475	0.775
Pre-treatment mean	272.00	15.916	1682.14
Year FE	Yes	Yes	Yes
IC FE	Yes	Yes	Yes
Observations	14,157	14,157	14,462

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. In column 1, the outcome is the number of hours worked in the territory by private workers, per inhabitant (using the 2016 population). In column 2, the outcome is the total wages of private workers divided by the number of hours worked. In column 3, the outcome is the housing price index as defined in Section 4.2. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[19] Similar to Hatfield et al. (2024) the price regressions do not capture the valuation of a specific amenity like education spending, but rather the composite effect of all changes (taxes, spending, labor market, economic effects, etc.) resulting from less fragmentation.

FIGURE 8: Impact on Economic Activity and Housing Prices



(a) Hours worked

(b) Hourly Wage

(c) Housing price

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. In Figure (a), the outcome is the number of hours worked in the territory by private workers, per inhabitant (using the 2016 population). In Figure (b), the outcome is the total wages of private workers divided by the number of hours worked. In Figure (c), the outcome is the housing price index as defined in Section 4.2.

With respect to interpreting these effects as capitalization estimates, we verify there is little compositional change in housing supply. To do this, we analyze the number of building permits in the territory. Table A5 shows that there is no change in the number of building permits. Thus, consolidation does not appear to spur more building in the territory. Given this lack of a compositional change in housing construction, price changes can be interpreted as a capitalization effect of the reform.

Table 8 and Figure 9 further show the results on house prices separately for the center and the periphery. We find that places near the headquarter, experience a relative decline of only 3.0% against 4.12% for places away from the center.[20] Interestingly, more central places also see relative declines in house prices, albeit smaller than remote places. This result

[20] Appendix Figure A4 shows the housing effects for the second and third quartiles based on the distance to the headquarter. Appendix Table A6 provides the same heterogeneity analysis for hours worked and wages. We find null effects for the latter in all samples, and a modest decrease in the former for both the center and the periphery.

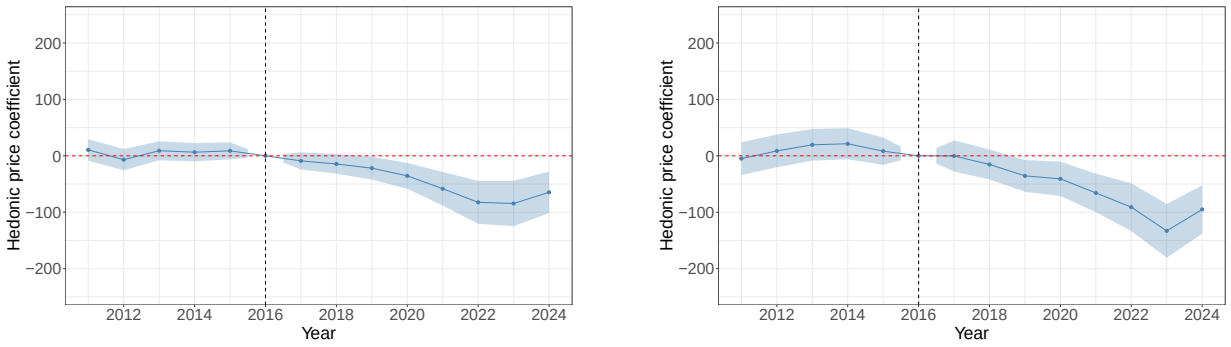
is important because these central places have almost no school closures while also seeing increases in public employment. Thus, theoretically, it could have been possible that prices may have risen in these more central areas as public employment centralized. The fact that they did not rise there suggests two possible mechanisms at work. First, closures of schools in more peripheral jurisdictions may have changed the composition of peers in central districts or may have caused extra congestion in these schools. Second, if mobility within the territory were at work, with households moving from the periphery to the center, this would have placed upward pressure on prices in the center. But price declines suggest that "migration" may have been outward from the territory. We place the word migration in quotes because even if the population were fixed, sorting may have been amplified: individuals who valued the services may have moved outside, with the individual moving into the territory valuing the services provided by the IC less. All-in-all, we conclude that there was a decline in the valuation of services provided by consolidated territories, with larger ICs less being able to match the preferences of residents to the services provided. These declines in valuation were largest in the most remote areas.

TABLE 8: Impact on Housing Prices Across the Territory

	All	Center (Q1)	Periphery (Q4)
Treated × Post	-61.03***	-51.00***	-68.42***
	(11.14)	(12.15)	(12.13)
p-value		Q1 = Q4: 0.084	
Pre-trend p-value	0.775	0.292	0.498
Pre-treatment mean	1682.14	1677.90	1656.30
Year FE	Yes	Yes	Yes
IC FE	Yes	Yes	Yes
Observations	14,462	14,462	14,434

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the housing price index, as defined in Section 4.2. In column 2 (resp. 3), the housing price index is built using only housing transactions taking place in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters. The third row reports the p-value of the difference between the coefficients for the center and the periphery. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 9: Impact on Housing Prices Across the Territory



(a) Center (b) Periphery

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the housing price index, as defined in Section 4.2. In Figure (a) (resp. (b)), the housing price index is built using only housing transactions taking place in the 25% of municipalities closest to (resp. farthest from) the IC's headquarters.

5.4 Spillovers at the Department Level Analysis

Dating to Oates (1972), spillover effects of local policies are well acknowledged. Given the prior analysis has shown that house prices fell in treated areas, one might worry that although these local effects are negative, perhaps they are offset by positive spillover effects at a more centralized level. To address this, we conduct a more macroeconomic analysis that exploits the department (state) as the unit of analysis. France is composed of 96 departments in the mainland. The basic idea is to exploit variation in the share of each department that was treated by the reform: more IC consolidations mean the department became more centralized than a department with fewer consolidations.

To formalize this, we can estimate for department d in year t:

$$Y_{dt} = \alpha + \beta N_{dt} + \zeta_d + \zeta_t + \varepsilon_{dt}, \tag{3}$$

where Y_{dt} is the hedonic house price in each department, N_{dt} is the number of ICs in the department, and the remaining variables are standard two-way fixed effects. However, the coefficient β may be biased if there is reverse causality of better/worse economic outcomes on the amount of decentralization. For this reason, we instrument N_{dt} with $S_d \times Post_t$ where S_d is the share of the department's population that consolidated under the reform. We use the pre-reform 2016 population, as any time variation in population may be endogenous. This share is then interacted with an indicator for year t being post-reform.

This panel data regression can easily be mapped into a difference-in-difference design of

the form:

$$Y_{dt} = \alpha + \beta S_d \times Post_t + \zeta_t + \zeta_d + \varepsilon_{dt}, \tag{4}$$

or an event study with $Post_t$ replaced by year dummies. However, Callaway et al. (2024) and Callaway et al. (2025) show that difference-in-differences designs that exploit a continuous dosage require a stronger identifying assumption than a standard binary design and the standard pre-period indicators are no longer informative with respect to the stronger assumption. We follow their recommendation and dichotomize the continuous treatment by comparing the top 25% of departments with the bottom 25% of departments based on their treatment dosage, S_d. Such a design also facilitates a Wald estimator where we first estimate a "first stage" regression by replacing the dependent variable with N_{dt}. We then run even studies using house prices as the dependent variable. The ratio of the two coefficients forms the Wald estimator that parallels the IV design.

We first show the two stage event study estimators. Figure 10 and Table 9 (columns 1 and 2) show the results of the first stage with the number of ICs as the dependent variable. The event study shows parallel trends in the period before the reform between departments with a high and low treatment dosage. After the reform, the top 25% of departments saw a persistent decline of 8.4 ICs compared to the lower quartile, representing a 31.7 percent reduction in the number of ICs. When comparing the top 50 and the bottom 50 percent of departments, we see a smaller decline of 5.1 ICs, as expected.

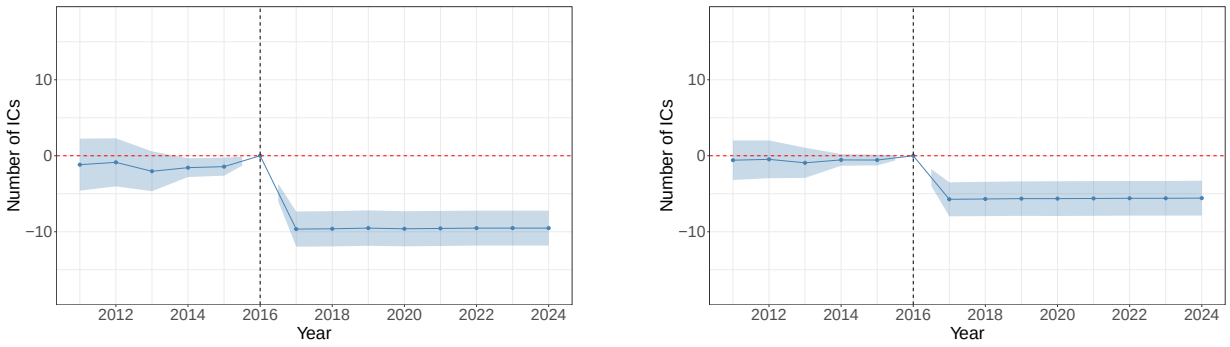
Figure 11 and Table 9 (columns 3 and 4) show the results of the second stage house price regressions. Again, we see flat pre-trends, indicating that departments with a high share of their population that is forced to merge did not have differential trends in house prices relative to those departments with a low share. Then focusing on comparing the top to the bottom quartile, we find that the most exposed departments experience an average relative decline in housing prices of 11.2% after the reform. Given that this corresponds to a 31.7% decline in the number of ICs, the implied elasticity is approximately 0.35. Keeping in mind these are the most exposed departments, we repeat this exercise by splitting at the median. This also helps with identification as the design implies that a lower dosage differential between the two groups should result in a smaller effect. Here, house prices fall by 5.7%.

Finally, in Table 10 we turn to the formal instrumental variable analysis in equation 3. The instrument is strong, with an F statistic around 30. The IV estimate implies that a one unit decrease in the number of ICs in the department lowers house prices per square meter by 20 Euros.[21] Results are robust to discretizing the instrument rather than using it

[21] Coefficients in the table are positive because they correspond to a one unit increase.

in continuous form, as shown in columns 2 and 3.

FIGURE 10: First Stage of the Department-Level Analysis

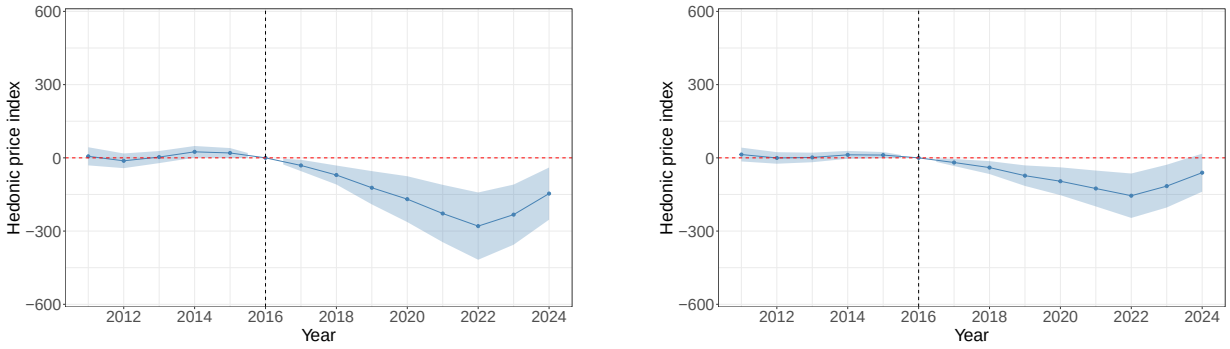


(a) Top vs. Bottom Quartile

(b) Above vs. Below Median

Notes: The dots represent the estimates from the event-study version of equation 4. The blue shaded areas represent the 95 percent confidence intervals. The outcome is the number of ICs in the department. Departments are compared by their reform exposure, defined as the share of the department's population located in ICs that merged (using the 2016 population). Figure (a) compares departments in the top quartile of exposure to those in the bottom quartile; Figure (b) compares departments above the median to those below.

FIGURE 11: Impact on Housing Prices at the Department Level



(a) Top vs. Bottom Quartile

(b) Above vs. Below Median

Notes: The dots represent the estimates from the event-study version of equation 4. The blue shaded areas represent the 95 percent confidence intervals. The outcome is the department-level hedonic house price index. Departments are compared by their reform exposure, defined as the share of the department's population located in ICs that merged (using the 2016 population). Figure (a) compares departments in the top quartile of exposure to those in the bottom quartile; Figure (b) compares departments above the median to those below.

TABLE 9: Department-level reduced form results

	#ICs		€/m^2	
	(1) 50/50	(2) 25/75	(3) 50/50	(4) 25/75
Treated × Post	-5.120***	-8.384***	-92.06***	-167.00***
	(1.253)	(1.371)	(31.99)	(46.79)
p-value 50/50 = 25/75	0.079		0.186	
Pre-treatment mean	27.26	26.47	1609.12	1493.22
Year FE	Yes	Yes	Yes	Yes
Dept FE	Yes	Yes	Yes	Yes
Observations	1,274	644	1,260	630

Notes: Each column reports the estimates obtained from equation 4. The outcome is the number of ICs in the department (columns 1 and 2) and the department-level hedonic house price index (columns 3 and 4). Departments are compared by their reform exposure, defined as the share of the department's population located in ICs that merged (using the 2016 population). Columns 2 and 4 (resp. 1 and 3) compare departments in the top quartile of exposure to those in the bottom quartile (resp. above the median to those below). All specifications include department and year fixed effects. Standard errors, reported in parentheses, are clustered at the department level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 10: Department-level IV (2SLS): Impact on Housing Prices

	Housing prices		
Number of ICs	20.106***	20.864***	17.760**
	(6.570)	(6.617)	(7.691)
First-stage coef.	-10.4257	-8.0969	-6.6003
	(1.805)	(1.411)	(1.243)
Kleibergen-Paap F	33.3	32.9	28.2
Instrument	Share × Post	High × Post	Medium × Post
Observations	1170	585	871

Notes: 2SLS estimates of the effect of the number of ICs in a department on the hedonic price index, instrumenting the number of ICs with department reform exposure interacted with a post-2017 indicator. The instruments are: *Share* × Post, where *Share* is the continuous share of the department's population located in merged ICs (using the 2016 population); *High* × Post and *Medium* × Post, where *High* (resp. *Medium*) is an indicator for departments in the top quartile (resp. middle half) versus the bottom quartile of that exposure share. All specifications include department and year fixed effects. Standard errors, reported in parentheses, are clustered at the department level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

First, although consolidation is long argued to be a means of capturing economies of scale, we find no evidence that government costs declined or that public employment changed in the aggregate. Second, it does not appear that there were changes in the number of service-related amenities, with the exception of declines in the number of schools.

Third, we instead find spatial relocation of services from more remote municipalities to more centralized municipalities. This relocation is most pronounced for centrally integrated public services (e.g., city hall services) where we see an increase in public employment in central locations and a decline in employment in more remote places. With respect to horizontally integrated services such as schools, we see closures in remote areas, but little evidence of expansions in more central areas. The loss of services in places further from the headquarter shows that peripheral places face added challenges for having their voice be heard in the political decisions.

To put it all together, we study house prices. We find declines in house prices throughout the territory—not just in the more peripheral places. This suggests that the aggregate effects of integration via changes in the amount and location of services have an overall negative effect on the territory. Under some assumptions, this house price decline can be viewed as a summary measure of the willingness to pay for centralization.

Of course, a remaining issue is that perhaps centralization has large positive spillover effects. To study these, we look at house prices across departments in France and find that departments that are most exposed to centralization see the largest relative house price declines. This suggests that our negative results are not a result of the level of aggregation being too local to capture any positive effects. If anything, the spillover effects of centralization also appear to be negative.

Overall, our paper provides evidence that calls for increased centralization of local public services may be misplaced.

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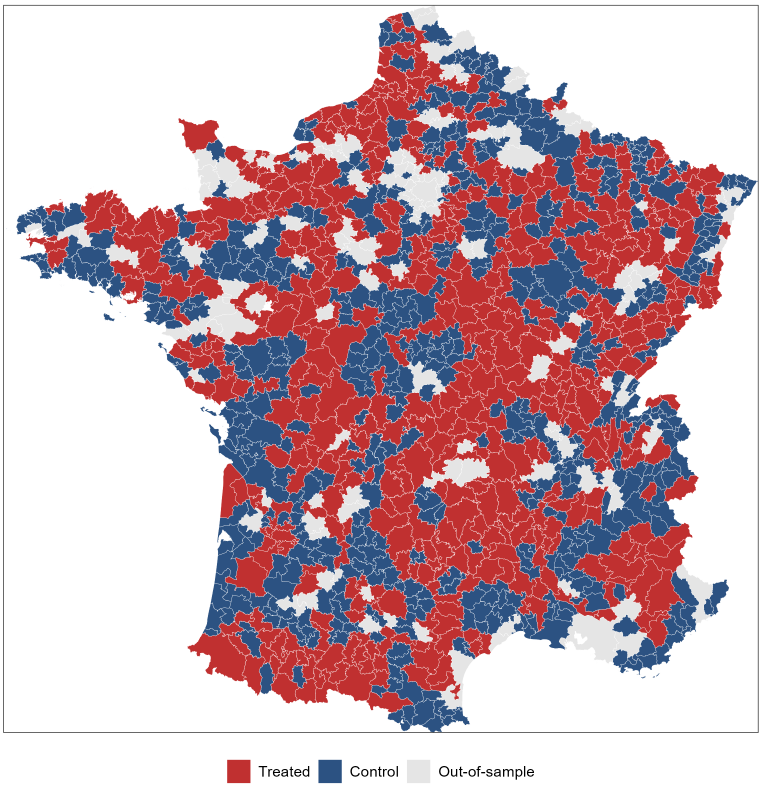
A Appendix (Online Only)

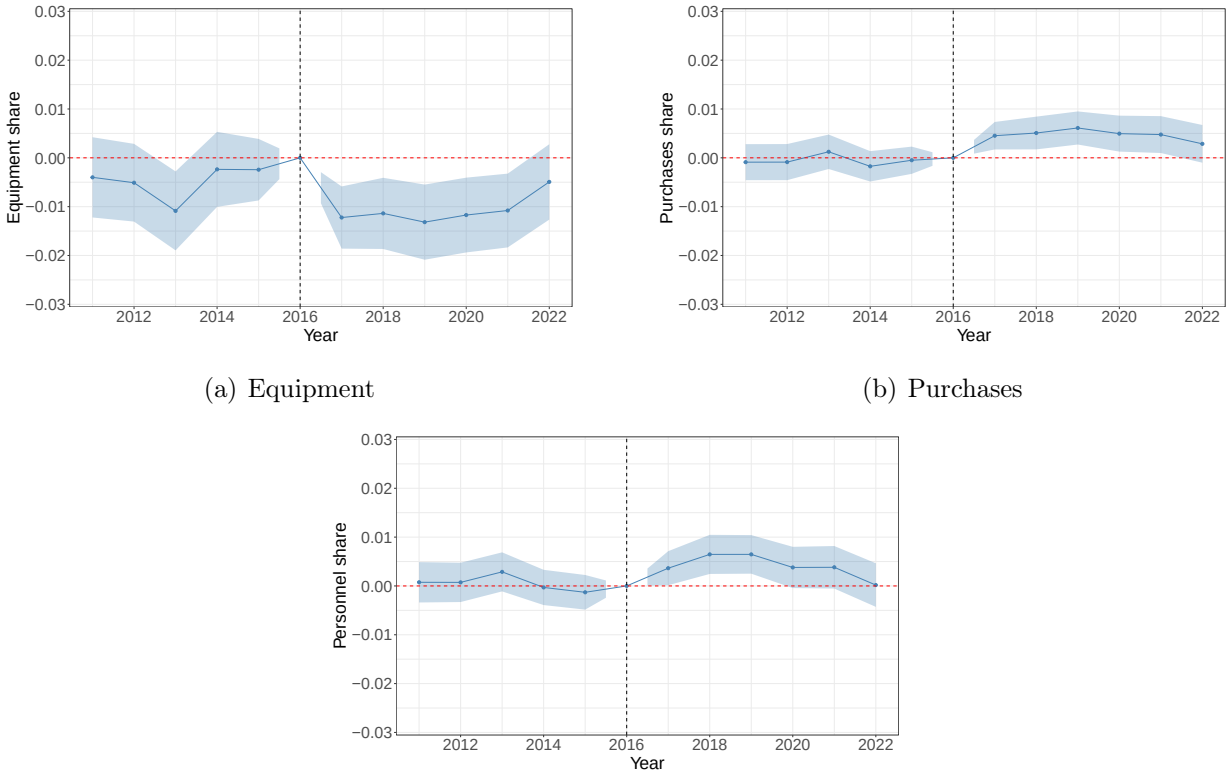
A.1 Additional Figures

FIGURE A1: Map of Treated and Control Territories



Notes: The map displays the treated territories in red (forced to merge following the NOTRe law) and the control territories in blue (unchanged after the law), across mainland France (excluding Corsica). Both groups are spread across all regions of France.

FIGURE A2: Impact on Spending Categories



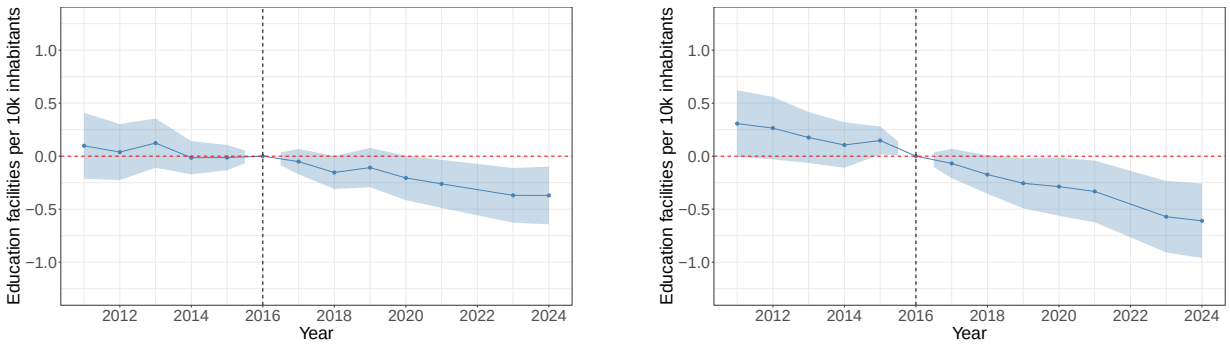
(a) Equipment

(b) Purchases

(c) Personnel

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcomes are the share of total consolidated spending allocated to equipment (figure (a)), purchases of goods and services (figure (b)), and personnel (figure (c)).

FIGURE A3: Impact on Schools Across the Territory, Second and Third Distance Quartiles

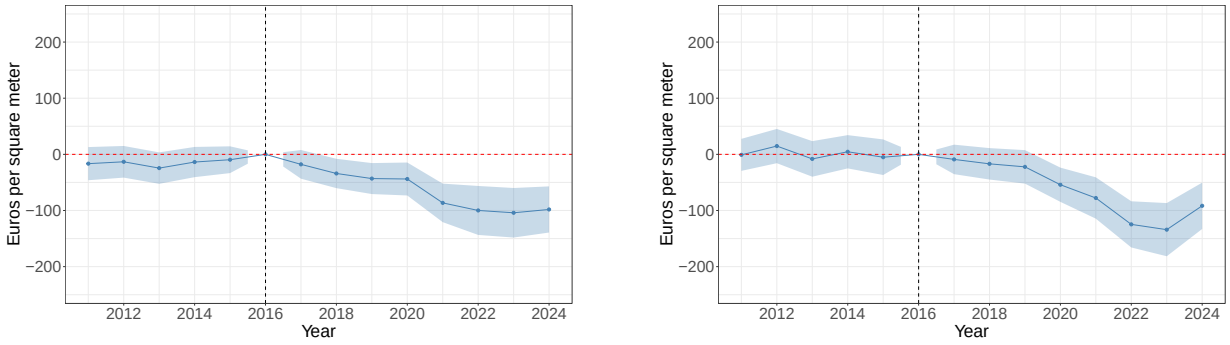


(a) Second Quartile

(b) Third Quartile

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the number of primary schools in the territory, per 10,000 inhabitants (using the 2016 population). Figure (a) (resp. (b)) only considers schools located in the second (resp. third) quartile of municipalities by distance to the IC's headquarters.

FIGURE A4: Impact on Housing Prices Across the Territory, Second and Third Distance Quartiles



(a) Second Quartile

(b) Third Quartile

Notes: The dots represent the estimates from the event-study regression (equation 2). The blue shaded areas represent the 95 percent confidence intervals. The outcome is the housing price index, as defined in Section 4.2. In Figure (a) (resp. (b)), the housing price index is built using only housing transactions taking place in the second (resp. third) quartile of municipalities by distance to the IC's headquarters.

A.2 Additional Tables

TABLE A1: Impact on Spending Categories

	Equipment share	Purchasing share	Personnel share
Treated × Post	-0.007***	0.005***	0.004***
	(0.002)	(0.001)	(0.001)
Pre-trend p-value	0.112	0.315	0.246
Pre-treatment mean	0.253	0.191	0.244
Year FE	Yes	Yes	Yes
IC FE	Yes	Yes	Yes
Observations	13,002	13,002	13,002

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. In column 1 (resp. 2 and 3), the outcome is the share of total consolidated spending allocated to equipment (resp. purchases of goods and services, and personnel). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A2: Impact on Public Service Facilities (PPML Estimation)

	Health	Education	Cultural	Sports
Treated × Post	-0.024***	-0.015**	-0.004	-0.000
	(0.009)	(0.007)	(0.038)	(0.002)
Pre-trend p-value	0.197	0.258	0.304	0.693
Pre-treatment mean	13.401	11.157	0.415	19.972
Year FE	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes
Observations	13,068	14,157	11,979	10,890

Notes: Each column reports the Poisson pseudo-maximum likelihood (PPML) estimates of equation 1. Standard errors clustered at the territory level are in parentheses. In column 1 (resp. 2, 3, and 4), the outcome is the number of health facilities (resp., schools, cultural facilities, and sports facilities) per 10,000 inhabitants (using the 2016 population). Health counts the number of general practitioners, dentists, and gynecologists in the territory; Education counts the number of primary schools ("maternelles" and "primaires"); Culture counts the number of cinemas; Sports counts the number of tennis courts, bocce courts, large playing fields, and multi-sport facilities. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3: Impact on High Schools

	OLS	Poisson
Treated × Post	-0.008	-0.012
	(0.011)	(0.021)
Pre-trend p-value	0.357	0.373
Pre-treatment mean	0.539	0.539
Year FE	Yes	Yes
IC FE	Yes	Yes
Observations	14,157	8,255

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the number of high schools in the territory. Column 1 is estimated by OLS and column 2 by Poisson pseudo-maximum likelihood (PPML); the number of observations differ because the Poisson estimator drops territories whose outcome is zero in every year of the panel. High schools are managed at the regional level and therefore serve as a placebo for local consolidation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A4: Impact on Public Service Facilities Across the Territory

	Health			Sports			Cultural		
	All	Q1	Q4	All	Q1	Q4	All	Q1	Q4
Treated × Post	-0.307***	-0.411**	-0.297	-0.007	0.044	-0.111	-0.007	-0.011	0.070
	(0.113)	(0.188)	(0.209)	(0.050)	(0.079)	(0.131)	(0.017)	(0.025)	(0.053)
p-value		Q1 = Q4: 0.675			Q1 = Q4: 0.297			Q1 = Q4: 0.163	
Pre-trend p-value	0.197	0.275	0.317	0.693	0.996	0.964	0.304	0.484	0.328
Pre-treatment mean	13.401	16.420	9.377	19.972	17.845	24.956	0.415	0.533	0.314
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,068	13,068	13,068	10,890	10,890	10,890	11,979	11,979	11,979

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcomes are the number of health facilities, cultural facilities, and sports facilities, each per 10,000 inhabitants (using the 2016 population). For each category, the first column considers the full territory, while the following columns only consider facilities located in the 25% of municipalities closest to (Center) and farthest from (Periphery) the IC's headquarters. Health counts the number of general practitioners, dentists, and gynecologists; Culture counts the number of cinemas; Sports counts the number of tennis courts, bocce courts, large playing fields, and multi-sport facilities. The third row reports the p-value of the difference between the coefficients for the center and the periphery. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A5: Impact on the Number of Authorized Housing Units

	All	Center (Q1)	Periphery (Q4)
Treated × Post	-1.780	-1.151	-2.404
	(1.183)	(1.567)	(1.981)
p-value		Q1 = Q4: 0.589	
Pre-trend p-value	0.025	0.767	0.076
Pre-treatment mean	49.569	48.452	49.296
Year FE	Yes	Yes	Yes
IC FE	Yes	Yes	Yes
Observations	15,246	15,246	15,246

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcome is the number of authorized housing units in the territory, per 10,000 inhabitants (using the 2016 population). Column 2 (resp. 3) only considers municipalities in the 25% closest to (resp. farthest from) the IC's headquarters. The third row reports the p-value of the difference between the coefficients for the center and the periphery. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A6: Impact on Hours Worked and Hourly Wage Across the Territory

	Hours worked p.c.			Gross hourly wage		
	All	Q1	Q4	All	Q1	Q4
Treated × Post	-3.45*	-18.14*	-5.36**	-0.017	-0.007	-0.054
	(1.92)	(10.97)	(2.66)	(0.042)	(0.049)	(0.068)
p-value		Q1 = Q4: 0.245			Q1 = Q4: 0.565	
Pre-trend p-value	0.038	0.818	0.185	0.475	0.764	0.970
Pre-treatment mean	272.00	909.89	185.72	15.916	15.940	15.206
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
IC FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,157	14,001	14,157	14,157	14,157	14,142

Notes: Each column reports the estimates obtained from equation 1. Standard errors clustered at the territory level are in parentheses. The outcomes are the number of hours worked by private workers (per inhabitant, using the 2016 population) and the gross hourly wage (total wages of private workers divided by the number of hours worked). For each outcome, the columns report the effect for the full territory and separately for the 25% of municipalities closest to (Center) and farthest from (Periphery) the IC's headquarters. The third row reports the p-value of the difference between the coefficients for the center and the periphery * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.