Better Alone? Evidence on the Costs of Intermunicipal Cooperation

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Abstract

This paper provides new evidence on why municipalities are often reluctant to integrate. Exploiting a unique setting of forced intermunicipal cooperation in France, I assess the causal impact of integration on municipalities that opposed it. I first find that the loss of power over housing regulations led to a large increase in construction in high-demand urban municipalities, creating further increases in housing density. I next show that rural municipalities forced to integrate experienced a loss of public service facilities, increasing the distance to public services for their residents. I do not find the same effects for municipalities that had voluntarily integrated prior to the law, while both types of municipality enjoyed similar benefits in terms of public transport and fiscal revenues. These findings support the fact that municipalities resisted to avoid the local costs of integration and highlight the local consequences of changing the decision-making scale.

Keywords: Local governments, Intermunicipal cooperation, Difference-in-differences, Housing regulations, Local public services

JEL Codes: H70, R52, R53

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1 Introduction

Over the last century, developed countries have encouraged intermunicipal cooperation in order to create larger local jurisdictions and achieve economies of scale in the provision of public goods. However, municipalities are often reluctant to cooperate, slowing or even blocking the consolidation process.[1]

According to Alesina and Spolaore (1997)'s model, this opposition reflects the fundamental tradeoff of jurisdiction size. On the one hand, intermunicipal cooperation may improve overall efficiency by creating economies of scale and internalizing cross-municipal externalities. On the other, intermunicipal cooperation can be costly for municipalities, as they lose autonomy over local policies, reducing their ability to tailor policies to local preferences and protect local interests (Tiebout, 1956; Bolton and Roland, 1997; Alesina and Spolaore, 2003). Confronted with this tradeoff, municipalities are likely to select on gains and losses (Heckman et al., 2006), and municipalities expecting the costs of integration to overcome the benefits will oppose integration.

While the theory predicts that resistance should be explained by the actual consequences of integration, the public debate often emphasizes ideological or political motives, such as residents' attachment to local identity or mayors' attachment to their own political power. Assessing which type of explanation best accounts for municipalities' resistance is key for our understanding of consolidation failure and for the design of consolidation policies.

The literature has sought to elicit the determinants of cooperation by studying the ex-ante characteristics of municipalities that choose to integrate, which is methodologically quite challenging (Gordon and Knight, 2009; Saarimaa and Tukiainen, 2014). Instead, using a unique setting of forced integration, this paper assesses the causal impact of integration for municipalities that opposed it. By comparing their experience to that of municipalities that instead voluntarily integrated, I can test whether resistance is driven by the local costs of integration.

While there is some evidence on the overall impact of consolidation on total spending and tax competition (e.g., Bel and Warner, 2015; Blom-Hansen et al., 2016; Breuillé et al., 2018), there is still little evidence on the local consequences of integration faced by individual municipalities. This reflects in part the general focus on mergers: after consolidation, administrative data are collected at the post-merger level, making it challenging to compare the situations of preexisting municipalities before and after integration. Moreover, cooperation is usually voluntary, implying that we observe the consequences of integration only for municipalities that were willing to cooperate and

[1] Despite large financial incentives, voluntary mergers in Japan resulted in a higher number of municipalities than what the government would have chosen to impose (Weese, 2015); other countries, such as Denmark and Sweden, instead decided to force mergers (Lidström, 2010; Mouritzen, 2010); in the US, after a wave of consolidation in the 19th century, municipalities became more and more reluctant to integrate (Jackson, 1987); in France, the government failed to impose mergers in the 1970s and then strove to promote the formation of intermunicipal communities.

are thus least likely to bear the costs of integration.

This paper overcomes these challenges by exploiting a 2010 reform in France that made intermunicipal cooperation mandatory, forcing around 1,800 municipalities to enter an intermunicipal community (IC). Importantly, when entering a community, municipalities do not disappear as they do in mergers. They coexist with this new higher level of governance and share their public policies with neighboring municipalities in the same community. I can thus follow the same municipality before and after integration. While this form of cooperation is widespread across the world, France is the only country in which it became mandatory.[2]

Using a difference-in-differences strategy, I compare, before and after the 2010 law, municipalities forced to integrate with municipalities that have been part of an intermunicipal community for a long time. I provide extensive evidence supporting the common trend assumption. In particular, I show that those two groups, while selected, evolved similarly prior to the law. Along with extensive data collection at the municipal level, this strategy enables me to measure the causal impact of integration on municipalities that resisted it. I then compare their experience to that of municipalities that had voluntarily joined a community before the 2010 law, using a staggered adoption design in which the date of the treatment corresponds to the year of integration.[3]

Municipalities that are part of the same community share several public policies; losing autonomy over these can be costly. First, urban planning and housing policies become subject to the guidelines set by the IC, which decides where and how much to build in each member municipality. Transferring such policies at a higher level can help overcome local regulatory capture and allocate people across space better. However, this can be costly for desirable locations that have been using local housing regulations to prevent further increases in housing density (Gyourko and Molloy, 2015) and where would-be residents can now impose new construction.[4] Second, integrated municipalities jointly decide over the location of public service facilities, such as daycare facilities, and social, cultural, and sports facilities. With the aim of achieving economies of scale, ICs are likely to concentrate resources on facilities in high-density and central areas. While this is likely beneficial for the IC as a whole, low-density and peripheral municipalities might end up

[2]Many countries have intermunicipal organizations similar to those in France, including Finland, the Netherlands, Ireland, Brazil, Spain, and Italy (Hulst et al., 2009; De Mello and Lago-Peñas, 2013). Similarly, shared services in the UK and special or school districts in the US involve the creation of a new level of governance. In France, the integration process remained voluntary from the creation of ICs in the 1970s until 2010, when integration became mandatory.

[3]In light of the recent literature on the issues associated with the staggered adoption design, I also use an alternative estimation procedure developed by de Chaisemartin and D'Haultfoeuille (2020).

[4]This local opposition to new construction is commonly referred to as NIMBYism ("Not In My BackYard"). Higher-level governance has been advocated as a way to overcome housing regulations and increase construction (Rusk, 1995; OECD, 2012; Glaeser, 2014; Glaeser and Gyourko, 2018). This typically triggers strong resistance from municipalities. For instance, Orfield (1997) describes the fierce opposition of municipalities in Minnesota against the transfer of urban planning policies to the Metropolitan Area level. In France, we say that a mayor who builds will lose the next election ("*maires batisseurs, maires battus*").

with fewer facilities, increasing the distance to public services for their residents.

If municipalities resisted integration to avoid such consequences, we should see that entering an intermunicipal community leads to more construction and fewer public services in municipalities forced to integrate. Moreover, we would expect the first effect to be driven by central (urban) municipalities and the second by peripheral (rural) municipalities.

Before taking those predictions to the data, I first provide some descriptive statistics on municipalities that opposed integration. Resisting municipalities are strikingly similar to an average French municipality based on large set of socio-demographic, land-use, and political characteristics. This is reassuring for the empirical strategy and consistent with resistance being driven by the local consequences of integration, as different costs apply to different types of municipalities. Moreover, resisting municipalities do not experience less political turnover, showing that resistance is unlikely to be driven by one mayor personal preferences, as municipalities kept refusing to integrate across electoral mandates. One key distinctive feature of resisting municipalities is that they have a lower average bargaining power inside their IC once integrated: they tend to be smaller compared to their neighbors and thus to end up with a lower share of the seats inside the intermunicipal council, making them less able to fight ICs' decisions. Using the empirical strategy described above, I assess whether the actual consequences of this lower bargaining power can account for their resistance.

Exploring the loss of autonomy over urban planning, I find that municipalities forced to enter an IC experienced a large increase in construction: the number of building permits delivered on their territory increased on average by 12.5 percent per year after 2010. Given that control and treated municipalities displayed similar trends prior to the law, this effect can be interpreted as the causal impact of their integration.[5] Turning to municipalities that voluntarily joined an IC before the law, I find that they did not experience a significant change in their housing supply following their integration. Hence, only municipalities that did not want to enter an IC experienced a significant increase in construction, supporting the view that they refused to integrate to avoid a rise in housing supply. Moreover, consistent with the effect coming from ICs imposing new constructions in desirable places that were restricting development, I find that the increase in building permits is driven by municipalities close to the employment center, in which the demand for housing is high. The effect is even stronger for municipalities already densely built up, for which an increase in construction most reduces the space available, and for municipalities losing the most bargaining power. This last result is consistent with resisting municipalities' inability to fight ICs decisions explaining why they experience costs that others municipalities were able to avoid.

[5] I show that all main results are robust in magnitude and significance to using propensity score matching, using higher-level clustering, and including time-varying controls at the municipality level. They are also robust to using alternative definitions of the control group, and in particular to removing control municipalities that received treated municipalities in their IC as a result of the law.

Additional results suggest that the increase in construction did not lead to a significant drop in housing prices. This null effect can be explained by the fact that housing regulations are relaxed in high-demand municipalities, where a demand spillover from nearby municipalities could compensate the direct supply side effect (Lin and Wachter, 2020), or by the fact that housing regulations affect price mainly through a change in density and population composition (Glaeser and Ward, 2009), which takes time to materialize. Together with evidence showing that the impact is not stronger for municipalities with a higher share of homeowners, these results suggest that the main cost coming from the loss of regulatory power is the increase in density and associated decrease in the quality of life rather than a decrease in housing value. Finally, I also show that the increase in construction is neither preceded nor followed by an increase in economic activity, providing additional evidence that the rise in housing supply is due to a change in regulation after integration, rather than to a change in economic conditions. This also suggests that the disutility associated with new construction is not compensated for by economic gains.

I then assess the consequences of integration on local public services. I gathered data on two different public services transferred to the community level after integration: daycare services and public libraries. The results suggest that, in the average year after 2010, rural municipalities forced to integrate ended up with 20 to 30 percent fewer daycare spots and public libraries, compared to rural control municipalities, increasing the distance to public services for their residents. Urban municipalities, however, did not experience any decline in local public services. I provide additional evidence that the impact comes from their loss of control over the location of facilities and from ICs concentrating resources in more densely populated areas: rural municipalities forced to integrate did not experience any change in the number of schools, the location of which is decided nationally and thus not directly affected by intermunicipal cooperation. Hence, while resisting urban municipalities experienced an increase in construction, resisting rural municipalities experienced a loss of local public services.

Finally, I investigate the consequences of integration on two additional aspects of consolidation: public transport and fiscal revenues. These two aspects should mainly entail benefits without specific local costs. Indeed, the ICs are in charge of expanding the public transport networks, with the explicit goal of connecting all member municipalities equally. Moreover, ICs are likely to generate extra fiscal revenues through a decrease in tax competition and thanks to the additional grants provided by the government to ICs. In line with these predictions, I find that municipalities that were forced to enter an IC became twice as likely to have access to public transport. They also experienced an increase of 14.5 percent per year in the fiscal resources available per resident, an increase driven in equal proportion by additional state transfers and an increase in tax revenues. Crucially, those benefits are similar to those experienced by municipalities that voluntarily integrated prior to the law. While resisting municipalities experienced costs that other municipalities

did not face, they entered ICs generating at least as much revenue and they benefited equally from an increased access to public transport. This suggests that they did not resist due to lower benefits, but to avoid the costs of integration: municipalities resisted integration knowing that they would not be able to prevent their neighbors from imposing new construction on them or decreasing their access to public services.

The last part of the paper provides additional evidence supporting this interpretation. I show that the costs experienced by resisting municipalities are unlikely to be driven by the fact that they entered an IC later on and were forced to do so, which could have triggered punishment by already integrated neighbors for having resisted so long. Indeed, the impact is similar whether resisting municipalities entered longstanding communities, recently created ones, or even newly created ICs, suggesting that they would have experienced the same effects had they integrated earlier.

Overall, this paper provides new evidence that municipalities' opposition to consolidation is driven by local consequences of integration, beyond ideological or political considerations. They also show that rural and urban municipalities do not face the same costs of integration. While the form of urban-rural divide described in this paper may be specific to France, the key message is that the loss of control over urban planning is costly for high-demand locations trying to avoid a rise in density, while the loss of power over local public services is costly for low-density places, due to the rationalization process. As most forms of cooperation among local jurisdictions imply sharing urban planning policies and public services, these results can help explain resistance against integration beyond the French case, as long as the decision process involves some jurisdictions losing more power than others.

These findings have several policy implications. First, the fact that municipalities reluctant to cooperate do face some costs of integration calls for a comprehensive welfare assessment of consolidation reforms that would weight the overall benefits against the local costs of integration. Second, assuming that the overall benefits prevail, knowing the costs preventing municipalities from integrating could help policymaker design more targeted compensation schemes to make municipalities internalize the positive spillovers of their own integration. Finally, this paper stresses the consequences of changing the scale of decision making. I provide causal evidence that transferring urban planning policies to a higher level of governance helps increase construction in desirable places. Hence, on top of achieving economies of scale—which is the usual stated motive behind consolidation policies—intermunicipal cooperation could be a powerful instrument to overcome local regulatory capture and allocate people and activity across space better.

1.1 Contribution to the literature

In the first half of the nineteenth century, Alexis de Tocqueville described the US federal system as a way of "combining the different advantages which result from the magnitude and the littleness of nations" (Tocqueville, 1945). In their seminal works on decentralization, Tiebout (1956) and Oates (1972) emphasized that the provision of public good at the local level reflects the population preferences more adequately than provision at the national level. Ultimately, the tradeoff was formalized in Alesina and Spolaore (2003) and Bolton and Roland (1997), in which the efficiency gains of large jurisdictions are weighted against the costs associated with heterogeneity and with the loss of local control. Additionally, Alesina and Spolaore (2003) predict that political agents will tend to choose a higher number of jurisdictions than optimal, as the ones bearing the costs of size do not internalize the aggregate benefits of belonging to a larger jurisdiction.

Assessing whether and which local consequences of integration explain resistance in practice is the focus of this paper.

Studies of the (un)willingness to consolidate have analyzed which types of municipality choose to integrate (see Bel and Warner (2016) for a review). Using structural or spatial models, they stress the role of expected change in fiscal revenues, expected distance to public services, and neighbors' characteristics (Alesina et al., 2004; Gordon and Knight, 2009; Saarimaa and Tukiainen, 2014; Weese, 2015; Bel and Warner, 2016; Porto et al., 2017; Di Porto and Paty, 2018). Focusing on political determinants, Durante and Gutierrez (2015) and Magontier et al. (2022) show that political alignment between neighbors fosters cooperation regarding crime prevention in Mexico and coastal development in Spain, respectively, whereas Porto et al. (2017) find that it does not predict voluntary cooperation in France. Relying on surveys, Sorensen (2006) and Bergholz and Bischoff (2018) show, respectively, that politicians' reported opposition to integration is driven by expected losses in revenues and decision-making power.[6]

Instead of estimating preferences for integration or modeling the choice to cooperate, this paper assesses the *actual* consequences of integration.

So far, the literature has mainly measured the impact of intermunicipal cooperation at the aggregate level to assess whether it leads to efficiency gains, as hoped by central governments. The results are mixed (see Bel and Warner (2015) and Bel and Sebo (2019) for a review). Although Reingewertz (2012), Blesse and Baskaran (2016), and Cobban (2019) find some evidence of efficiency gains in Israel, Germany, and Ontario, there is no evidence of cost savings in France, Denmark, the Netherlands, or Italy (Frère et al., 2014; Blom-Hansen et al., 2016; Allers and De Greef, 2018; Luca and Modrego, 2020).[7] While intermunicipal cooperation might not achieve its

[6] Looking at secession instead and using referendum data in Canada, Lapointe (2018) shows that income and language differences affect voters' preferences concerning municipal borders.

[7] In contrast to Luca and Modrego (2020), Ferraresi and Migali (2018) find that intermunicipal communities (*Unioni*

goal of reducing the cost of public goods, it does seem to reduce tax competition. In France, several studies find that the voluntary integration into intermunicipal communities led to higher tax rates and tax revenues (Carbonnier, 2013; Charlot et al., 2015; Breuillé et al., 2018).

Beyond the overall impact of cooperation, a few recent papers have used geo-coded data to investigate the distributional impact of mergers on voting behaviors (Horiuchi et al., 2015; Saarimaa and Tukiainen, 2016; Lapointe, 2018), local public sector jobs (Harjunen et al., 2019) and night-light intensity (Pickering et al., 2020; Egger et al., 2021). Looking at mergers makes it particularly challenging to study local effects. First, the set of outcomes is limited, as data are typically collected at the post-merger level. Second, most mergers are voluntary, providing little insight on the impact for municipalities that did not want to merge and are the most likely to bear the costs of consolidation.[8]

In contrast, my setting of forced cooperation enables me to make three important contributions to the literature. First, exploiting the 2010 law, I can assess the causal impact of integration on municipalities that refused to cooperate, which is typically unobserved. By comparing their experience to that of municipalities that had voluntarily integrated, I provide evidence that municipalities refused to integrate in order to avoid the local costs of integration. Second, I can investigate the local consequences of integration on a large set of outcomes, including housing, economic activity, local public services, public transport, and fiscal revenues. Third, given the large number of French municipalities, I can perform heterogeneity analysis along several dimensions and show that different types of municipality face different costs of integration.

In particular, while the regulation of construction is one of the most important functions of local government, this paper is the first to study the impact of intermunicipal cooperation on housing. I provide novel evidence that opposition to construction (NIMBYism) helps explain urban municipalities' reluctance to integrate and to share urban planning policies. These results are consistent with survey evidence showing that residents, whether renters or homeowners, oppose nearby constructions (Hankinson, 2018), which they consider nothing but a bother (Glaeser, 2014).

My findings also show that transferring urban planning to a higher level—allowing outsiders and would-be residents to participate in the decision making—reduces local housing restrictions. These results speak to the case of the US, where housing regulations have become tighter, arguably due to a shift of power to the micro scale (Hankinson, 2018; Purcell, 2006). Indeed, following the rise of neighborhood institutions, residents have become more involved in urban planning decisions (Rohe and Gates, 1985; Angotti, 2011), and there is evidence showing that US municipalities with

di Comuni) lead to a reduction in public expenditures in the Emilia Romagna region of Italy.

[8]Two recent papers provide evidence of the local impacts of jurisdiction size in alternative settings. Wilner (2022) focuses on the 2016 merger of French regions to assess the impact of higher-level centralization on life satisfaction. Dahis and Szerman (2022) focus on administrative splits instead of mergers and find that rural Brazilian municipalities that successfully split experienced some improvements in public service delivery.

ward-based representatives impose more zoning restrictions than those with at-large representatives (Clingermayer, 1994; Mast, 2022). Such regulations have important negative spillovers, as shown by the growing literature stressing the overall negative impact of local regulation on productivity, intergenerational mobility, and greenhouse gas emissions through urban sprawl (Glaeser and Maré, 2001; Glaeser and Kahn, 2010; Jones and Kammen, 2013; Chetty et al., 2016; Ganong and Shoag, 2017; Glaeser and Gyourko, 2018; Hsieh and Moretti, 2019; Duranton and Puga, 2019). In this context, urban scholars have advocated for a shift of power from the municipalities to the metropolitan areas (Rusk, 1995; Glaeser, 2014; Glaeser and Gyourko, 2018). My results provide causal evidence that consolidation is indeed an effective tool to overcome local housing regulations.

The remainder of the paper is organized as follows. Section 2 presents the institutional setting and outlines the conceptual framework guiding the analysis. Section 3 provides descriptive statistics on resisting municipalities. Section 4 describes the empirical strategy and discusses the identification assumption. I present the main results on housing supply in Section 5 and on local public services in Section 6. Section 7 studies additional consequences of integration and Section 8 discusses the interpretation of the results. Section 9 concludes.

Appendix F provides a detailed description of the data and their sources.

2 Institutional setting and conceptual framework

2.1 Intermunicipal cooperation in France

France is divided into about 36,000 municipalities with an average size of 1,800 inhabitants. Municipalities are the lowest of three tiers of local government and account for 11 percent of total public spending. They are responsible for local urban planning, social housing, the provision of primary schools,[9] daycare services, municipal roads, public transport, and social, sports, and cultural facilities. Municipalities' revenues come mainly from local taxation (54 percent) and state transfers (23 percent). They raise four local taxes: the local business tax paid by firms and accounting for 45 percent of local tax revenues, the housing tax paid by all residents on the cadastral value of their accommodations (whether they are renters or owners), the property tax paid by owners, and the land tax. Each municipality is governed by a municipal council chaired by the mayor. Elections for municipal councilors take place every six years.

[9]Municipalities' responsibilities in the education sector are very limited. They are only responsible for the equipment of primary schools and for the organization of extra curricular activities. Decisions over the location of the schools, size of the classrooms, teachers' recruitment and assignment, and learning programs are centralized at the state level. Secondary schools (resp. high schools) are managed at the national and departmental (resp. regional) levels.

In the 1970s, the French government intended to pass a law that would have reduced the number of municipalities by 20 percent through mergers. Mayors massively blocked the reform and only a few mergers took place, reducing the number of municipalities by only 3 percent. Following this failure, the government decided to pivot toward promoting the creation of a new administrative structure: intermunicipal communities.[10] When entering an IC, the municipality does not disappear as in mergers; it continues to exist under a new level of local governance. The mayor and municipal council stay in place, but the municipality has to share some public policies with the other municipalities that are part of the same community.

By law, intermunicipal communities are in charge of "territory and economic development"; that is, urban planning and the promotion of local businesses. Since the 1980s decentralization laws, French municipalities are in charge of building permits and urban planning. Municipalities can control their housing supply by including restrictive regulations as part of their planning and development plan (*plan local d'urbanisme*) or by choosing not to establish any planning document at all, in which case housing development is strictly limited to projects considered as serving the municipality interest by the municipal council. Once a municipality is part of an IC, local planning becomes subject to guidelines set by the community. In the most stringent case, the IC issues a housing plan (*plan local d'habitat* or PLH) detailing where and how much to build in each member municipality. While the PLH is mandatory only for the largest ICs, many adopt it voluntarily and it covered 65 percent of the French population in 2011. ICs that do not issue a PLH can still set urban planning guidelines through the territorial coherence plan (*schéma de cohérence territoriale* or SCOT) or the intermunicipal development plan (*plan local d'urbanisme intercommunal* or PLUi). Overall, more than 90 percent of ICs in 2014 had at least one type of planning document that they can use to overrule municipal regulations (BANATIC, 2014).

Municipalities also decide which additional public services to transfer to the IC. The most commonly delegated ones (in 80 to 90 percent of cases) are services for which cooperation is likely to result in economies of scale: waste management, public transport, and social, cultural, and sports facilities. Once the public service is transferred, the IC becomes the sole decision maker and the sole owner of the financial and material resources attached to it.

Finally, an IC's revenues consist of state transfers and local taxes. It can either apply an additional tax rate to the four municipal tax rates or set a single business tax rate while still applying an additional tax rate on the other three. In the latter case, the municipalities lose the right to set

[10] In France, intermunicipal communities are called EPCI, for *établissement public de coopération intercommunale*. France has four types of IC: *communautés de communes* (CC), *communautés d'agglomerations* (CA), *communautés urbaines* (CU), and *métropoles*—defined mainly by the number of mandatory competences to be transferred to the community and by the size of the member municipalities. The vast majority of municipalities are part of a CC (more than 80 percent), the form of cooperation I describe in this section and the one that gives the most freedom to municipalities in deciding which public services to transfer.

their own business tax. Municipalities choose the tax system when creating the community.

The IC is run by a board made up of members of the municipal councils of all participating municipalities. The number of seats held by a municipality on the intermunicipal council is proportional to its population. By law, each municipality has at least one seat and no municipality has more than half of the seats. Once the allocation of seats is decided, each municipal council elects the municipal councilors who will be part of the intermunicipal council. Then, the intermunicipal council elects its president. Decisions over which public services to transfer to the community or over which tax system to adopt require the approval of either (a) two-thirds of the municipal councilors representing more than half of the IC's total population or (b) at least half of the municipal councilors representing more than two-thirds of the population, as well as the approval of all municipal councilors representing more than one-fourth of the IC's population. Then, day-to-day decisions about urban planning or the delivery of public services are made by the intermunicipal council, by majority. Hence, when joining an intermunicipal community, a municipality loses power over policies, the more so the smaller its population compared with the population of the other municipalities in the same IC.

Until 2010, municipalities were free to decide whether or not to create or join an IC. While municipalities could choose to do so starting in the 1970s, only about half of them were integrated by 1999. In order to boost the integration process, the government introduced large financial incentives: since the "Chevènement law" in 1999, ICs receive a state transfer on top of the individual transfers to each municipality, which remain unchanged whether the municipality integrates or not. This law marked a turning point and 95 percent of municipalities were part of an IC by 2010. In 2010, mainland France had 2,575 ICs, each with an average of 13 municipalities and 21,295 inhabitants. Still, 5 percent of the municipalities—about 1,800—remained isolated and were subsequently forced to integrate by the 2010 law.

Overall, it took more than 40 years and massive financial transfers from the French government to achieve full integration. This stresses the need to better understand municipalities' reluctance to integrate in order to design consolidation policies more effectively.

2.2 The 2010 law

In December 2010, a new law passed requiring that **(1) all municipalities must be part of an intermunicipal community** and (2) all intermunicipal communities must contain at least 5,000 inhabitants. This law followed a report issued by the French Court of Auditors indicating that, in general, ICs were too small to achieve economies of scale. The goal of the reform was thus to draw a new map of intermunicipal communities and organize the territory more effectively.[11]

[11]The 2010 law also changed the rule for the allocation of seats on the intermunicipal council (which still remains tightly linked to population size), as well as the way municipal councilors are elected (directly by voters during

This law forced municipalities that were isolated to enter a community. The only exceptions were Paris and municipalities in three départements around it[12] as well as a few islands that make up one municipality. The law also forced existing small ICs to merge with other communities in order to comply with the 5,000 threshold. This threshold did not apply to ICs in mountain zones, where municipalities are far from each other. I focus on the first aspect of the law and look at what happened to the municipalities that were not part of an IC before 2010. I call them "resisting municipalities," given that they chose not to join any IC until forced to.[13]

Appendix Figure A1 shows the geographic distribution of French municipalities depending on their integration status in 2010. Red indicates that the municipality was still not integrated in 2010; blue indicates that the municipality was already part of an IC. The gray areas are municipalities excluded from the sample, as further explained in Section 4.1. Even if many isolated municipalities are located in the center-north, red municipalities are present in all French regions.

Implementation of the reform took place between 2011 and 2014 and was conducted by the departmental prefect (the state representative in the département). In 2010, municipalities forced to enter an IC shared a border with 1.7 intermunicipal communities on average. Those neighboring more than one IC could choose which to join, but the decision had to be approved by the prefect. When possible and if allowed by the prefect they could also create a new IC with neighboring isolated municipalities. The process had to be finalized by 2014. If a municipality was still isolated in 2013, the prefect could force its integration into the IC she chose. At the end, 77 percent of the isolated municipalities entered an IC between 2011 and 2013 and 23 percent entered in 2014. This timing was greatly affected by the prefects themselves, some having initiated the process sooner after the law than others. The majority of municipalities (73 percent) joined an existing community; the rest created new ICs. As shown in Section 8, the consequences of integration do not depend on the number of options the municipality had or on the type of IC it was forced to enter. This is consistent with all the integration options being considered equally undesirable and with resisting municipalities' decision not to join *any* neighboring ICs prior to the law.

On average, municipalities forced to integrate made up 5.6 percent of the population of the community they joined. In 2014, the average intermunicipal community included 17 municipalities and 28,126 inhabitants.

municipal elections). These changes became effective only after the 2014 municipal elections. All municipalities were already part of an IC by then, so the changes affected all municipalities the same way.

[12]Paris and municipalities in three départements around Paris (Hauts-de-Seine, Seine-Saint-Denis, and Val-de-Marne) were exempt from this law as they are part of the "Great Paris" Project, which aims at consolidating the Paris metropolitan area.

[13]In particular, it never happened that one of these municipalities tried to integrate before 2010 but was rejected by an IC. More generally, an IC can refuse the entrance of a municipality only under very specific conditions and under the approval of the prefect. In practice, it almost never happened.

2.3 Conceptual Framework

The goal of the paper is to better understand municipalities' resistance to integration. Following Alesina and Spolaore (2003)'s model, the jurisdictions opposing consolidation are the ones bearing the local costs of integration.

To test this empirically, I borrow the intuition of the essential heterogeneity model (Heckman et al., 2006). In this model, the treatment impact is heterogeneous and agents choose whether or not to take the treatment anticipating its effects and thus sorting on the gains and losses. Under these assumptions, the treatment effect identified on the compliers is different from the effect on those who decided not to take it, which is typically unobserved. The French setting gives us a unique opportunity to assess the impact of the treatment—here, integration—on those who opposed it. Following Heckman et al. (2006), if municipalities resisted integration because they anticipated bearing the costs, we should see that the impact of integration is different for them than for municipalities that instead chose to integrate.

Prediction 1: After integration, resisting municipalities experienced costs that municipalities that voluntarily integrated did not experience.

To test this prediction, we first need to characterize the costs of integration. As described in Section 2.1, municipalities part of an IC share two main sets of policies that involve a tradeoff between aggregate benefits and localized costs.

First, municipalities lose regulatory power over urban planning and housing policies. Having such policies decided at the IC level make housing development less subject to local regulatory capture (NIMBYism) and can help allocate population and economic activity across space better (Glaeser and Gyourko, 2018; Hsieh and Moretti, 2019). In particular, the IC can increase construction in desirable locations close to the employment center, bypassing the local regulations that were preventing would-be residents to move there. Indeed, attractive locations tend to have a larger share of developed land and therefore oppose new construction so as to prevent further increases in housing density (Hilber and Robert-Nicoud, 2013; Gyourko and Molloy, 2015). While overcoming such local restrictions is likely beneficial for the IC as a whole, desirable locations face an increase in density that they were previously able to prevent.

Second, municipalities lose control over the location of public service facilities. In order to reduce the costs and increase the efficiency of public service delivery, ICs are likely to concentrate resources on facilities located in central and dense areas. While economies of scale are likely beneficial for the IC as a whole, peripheral low-density municipalities lose local facilities, increasing the distance to public services for their residents. Even if they might have access to more efficient facilities in the center, the loss of proximity can be an important source of discontent, as evidenced by the yellow vests movement in France, which was tightly linked to the loss of public service facilities in peripheral places (Algan et al., 2020; Boyer et al., 2020).

If those local costs explain resistance, we should see that after their forced integration:

Prediction 2: Resisting municipalities experienced an increase in construction.

Prediction 3: Resisting municipalities experienced a loss in local public services.

Prediction 4: (a) The first impact is driven by urban (central, high-density) municipalities, while (b) the second impact is driven by rural (peripheral, low-density) municipalities.

Two additional aspects of consolidation should entail benefits without specific local costs. First, the transfer of public transport to the IC level is made with the explicit goal of connecting all member municipalities equally (CEREMA, 2020). Second, municipalities can expect an increase in their fiscal revenues, thanks to the additional transfers from the state and to a lower tax competition between neighbors. Hence, if municipalities sorted on the local costs of integration, we should see no differences when it comes to transport and fiscal consequences.

Prediction 5: Resisting municipalities experienced similar benefits of integration in terms of public transport and fiscal revenues as municipalities that integrated voluntarily.

If the above predictions are verified and resisting municipalities did oppose integration anticipating bearing the local costs, this begs the question of how other municipalities were able to avoid them? To come back to Heckman et al. (2006)'s framework, we are now interested in identifying the "fundamental factors" that drive the heterogeneity. As stressed by Prediction 4, integration involves different costs that apply to different types of municipalities. For instance, a municipality density level and whether it is urban or rural do not predict resistance, but which type of cost the municipality will face. Instead, what will most likely determine whether a municipality will bear the costs of integration is its bargaining power inside the IC. Intuitively, an IC will be able to build more in the center (resp. close facilities in the periphery) only if the central (resp. peripheral) municipality is unable to oppose the decision. This can happen because the municipality is smaller relative to its neighbors and thus has a lower share of the seats inside the council. This leads us to our final prediction.

Prediction 6: The main underlying difference between the average resisting municipality and the average municipality that chose to integrate is their bargaining power inside their IC.

3 Descriptive statistics

Table A1 provides descriptive statistics on resisting municipalities at the time of the law in 2010 and compares them with municipalities that were already integrated.[14]

[14]Instead of looking at all integrated municipalities, Appendix Table B1 provides the same statistics as in Table A1 but restricting the second group to the control municipalities used in the main estimation (as defined in Section 4.1). The t-tests of the differences between the control and treatment groups are displayed in Appendix C3, along with the matching analysis. Appendix Table B2 replicates the first panel of Table A1 using municipalities' characteristics in 1999 instead of 2010. Finally, Appendix Tables B3 and B4 replicate Table A1 for urban and rural municipalities

Socio-demographic and land-use characteristics. As shown in Panel A, on average, a municipality forced to integrate had 1,640 inhabitants in 2010, versus 1,711 for an integrated municipality. While resisting municipalities are slightly denser on average, they exhibit similar population growth between 1999 and 2010 and the share of urban municipalities is quite comparable across the two groups. The population composition of resisting municipalities is also similar compared to other municipalities, in terms of age distribution, average number of children per family, and share of immigrants and unemployed workers. They have a larger share of executives on average (7.0 vs. 5.3 percent for integrated municipalities). Accordingly, residents are on average richer: the average annual taxable income per capita is 14,064 euros in 2010 in municipalities forced to integrate against 12,621 euros in the other municipalities. Note, however, that the standard deviation is large (4,362), indicating that resisting municipalities encompass both rich and poor municipalities.[15] Finally, both types of municipalities exhibit similar shares of commuters and homeowners.

Resisting municipalities' land use is also similar to that of integrated municipalities in 2010, suggesting that they do not systematically differ in their housing and land-use regulations (Panel B). The share of built land in municipalities forced to integrate is 0.74 percent on average (against 0.73 percent in integrated municipalities), the average height of housing constructions is 1.6 stories for both types of municipality, and their measures of floor-to-area ratio (FAR) stringency are very similar. Additionally, the average housing price to income ratio of resisting municipalities is very close to that of integrated municipalities.

Overall, municipalities forced to integrate are quite representative—based on observables—of an average French municipality. This is consistent with resistance being driven by the costs of integration, as different costs apply to different types of municipalities, as outlined in the conceptual framework. This is also reassuring for the identification strategy, as we would not expect completely different groups to display parallel trends in the outcomes of interest (see Section 4.2).

Political characteristics. The last panel displays municipalities' political characteristics, based on the results of the 2008 municipal and 2007 presidential elections. The main difference is that resisting municipalities are more likely to have a right-wing mayor (62.0 vs. 55.9 percent). Instead, the turnout rate in municipal and presidential elections, the share of voters voting for the far-right in presidential elections, and the probability of having a mayor not affiliated with any party ("NC" for "non classified") are comparable across the two groups. Mayors of municipalities forced to

separately. The same patterns emerge in all those tables.

[15] Given that integrated municipalities benefit from additional state transfers allocated to their IC and that they share their tax revenues at the IC level, comparing resisting and integrated municipalities' fiscal revenues in 2010 would not accurately reflect their relative wealth. Instead, I compare the 2002 fiscal revenues of resisting municipalities with those of municipalities that integrated voluntarily before 2010 but later than 2002 (the first year for which account data are available). In 2002, the average fiscal revenue of resisting municipalities was 581 euros per capita, compared to 523 for municipalities that voluntarily integrated after 2002. This is consistent with the gap found on residents' income in Table A1 and, as for income, the standard deviation for resisting municipalities is large (346).

integrate are slightly more likely to be women, but they have the same age on average as mayors in other municipalities. Finally, resisting municipalities have as much political turnover as any other municipality. The average probability that the mayor is the incumbent and has thus been in place since at least 2001 and the probability that the municipality changed political orientations between 2001 and 2008 are very similar in resisting and integrated municipalities (66.1 vs. 62.4 percent and 21.0 vs 22.1 percent, respectively).

The fact that municipalities forced to integrate are as likely as others to have elected a new mayor in 2008 suggests that their resistance is unlikely to be driven by mayors' individual interests or political preferences, as they kept refusing to integrate across electoral mandates.[16] This is also consistent with evidence suggesting that political affiliation and alignment does not predict early integration patterns in France (Porto et al., 2017). Assessing the actual consequences of integration is thus key to understanding why municipalities forced to integrate resisted integration.

Bargaining power. Differences in municipalities' bargaining power inside their IC could explain why some municipalities are able to fight ICs' decisions while others have no other choices but to bear the costs of integration. Hence, if municipalities resisted integration to avoid the local costs, they are likely to have a lower bargaining power inside their IC once integrated. This can be the case if resisting municipalities—although on average similar to the rest of the country—tend to be smaller than their neighbors. As the number of seats a municipality gets in the intermunicipal council is proportional to its population, I measure a municipality's bargaining power as its share of the IC's total population divided by the share of the IC's total population an average municipality from the same IC represents. The greater this ratio, the larger the share of seats the municipality has compared with the average number of seats others get. I look at the composition of ICs in 2014—that is at the end of the period of integration. While the average value of the ratio is 1 by construction, it is only 0.85 for municipalities that were forced to integrate, suggesting that resisting municipalities have a 15-percent lower bargaining power on average. A municipality loses even more power when joining an IC encompassing large municipalities that can impose their decisions. Resisting municipalities are also more likely to end up in such situation: 51.2 percent end up part of an IC encompassing a big city (of more than 5,000 inhabitants) in 2014, versus 42.3 percent for the full sample.[17]

Overall, the descriptive evidence are in line with Prediction 6: the main underlying difference between the average resisting municipality and the average municipality that chose to integrate is their bargaining power inside their IC. The next section presents the empirical strategy used to assess which consequences resisting municipalities had to face as a result of this lower bargaining

[16]Furthermore, Appendix Tables B14 and B15 show that the costs of integration—the effects on construction and public services—are similar whether the mayor was the incumbent or newly elected.

[17]The threshold of 5,000 inhabitants corresponds to the minimum legal size of an IC. Only 5 percent of French municipalities had more than 5,000 inhabitants in 2010.

power and test Predictions 1-5.

4 Empirical Strategy

This paper uses a difference-in-differences strategy in order to assess the impact of integration on resisting municipalities. More precisely, I compare, before and after the 2010 law, municipalities that were not part of an intermunicipal community in 2010—and thus forced to integrate—with municipalities that were already part of an IC in 2010 and thus not affected by the law. I first define the treatment and control groups, before presenting the specification and discussing the identification and validity of the design.

4.1 Treatment and control groups

I consider a balanced panel of municipalities and I focus on metropolitan France, excluding overseas territories and Corsica. I also exclude law exceptions: municipalities exempt from the law (Paris, municipalities in the three départements around Paris, and a few islands), and where the law applied differently (municipalities in mountain zones).[18]

My treatment group is made of municipalities that were still not integrated in 2010. I distinguish two types of municipality among those already integrated: (a) those that were already integrated by 1999 and thus had entered an IC long before the law and (b) those that integrated between 2000 and 2010 and thus entered an IC closer to the law. To make sure that control municipalities are not affected by their shift in integration status during the period of interest, I restrict the control group to group (a) and focus on the time period around the 2010 law, from 2004 to 2018.[19] The results are robust in both significance and magnitude to varying the latest date of integration of the control municipalities, from including only municipalities that integrated before 1999 to including all municipalities already integrated in 2010 (see Appendix C1).

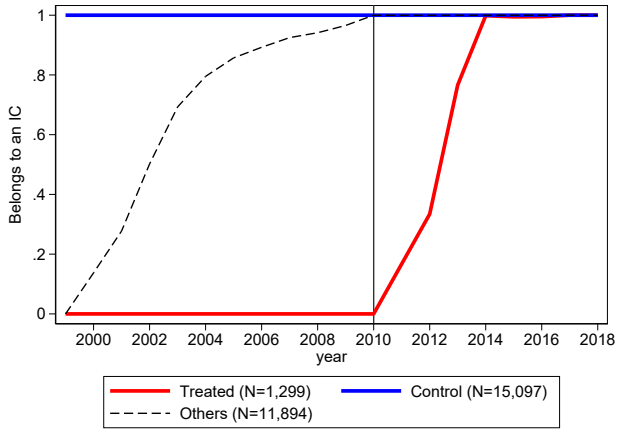
The final main sample of analysis is a balanced panel of 16,396 municipalities: 15,097 in the control group and 1,299 in the treatment group (8 percent). To illustrate the sample, Figure 1 plots the share of municipalities part of an IC, separately for the control group (blue line) and the treatment group (red line). By construction, all municipalities in the control group belonged to an IC

[18] As noted in Section 2.2, as municipalities in mountain zones are small and far from each other, the 2010 law applied differently. First, the prefect had less leverage to force them to enter an IC, as her decision had to be approved by the local "mountain committee," which slowed down the process. By 2013, only 60 percent of these municipalities were integrated, compared to 77 percent outside of mountain zones. Second, communities in mountain zones were not subject to the 5,000 population threshold, meaning that isolated municipalities were free to create or join smaller ICs.

[19] This definition of the sample excludes the less than 1 percent of municipalities whose integration status changed over 1999–2010: isolated municipalities in 2010 that were part of an IC at some point between 1999 and 2010 as well as municipalities that were part of an IC in 2010 but briefly isolated at some point between 1999 and 2010. The results remain unchanged if I include them.

by 1999 and over the whole period. In contrast, no municipality in the treatment group belonged to an IC before 2010. After the law, treated municipalities gradually enter a community and, by 2014, all municipalities from the sample are integrated.[20] Finally, the gray dotted line displays the integration of the 11,894 municipalities that integrated between 2000 and 2010. While these municipalities are excluded from the main sample of analysis, I will take advantage of their staggered integration to assess the impact of integration on municipalities that voluntarily integrated and compare it with the experience of municipalities that were forced to integrate (see Section 5.2).

Figure 1: Integration status



Notes: This graph plots the share of municipalities part of an intermunicipal community separately for the treatment group (red line) and the control group (blue line). The gray dotted line represents municipalities that integrated between 2000 and 2010 and are excluded from the main sample of analysis.

4.2 Specification and identification

I estimate the following specification for all municipalities in the main sample of analysis over 2004–2018:

$$Y_{mt} = \alpha + \beta 1_{\{t>2010\}} 1_{\{treated_m=1\}} + \delta_t + \theta_m + \varepsilon_{mt}, \tag{1}$$

where m stands for the municipality and t for the year. $1_{\{t>2010\}}$ is an indicator variable equal to 1 for years after the reform, starting in 2011. $1_{\{treated_m=1\}}$ is an indicator variable equal to 1 for municipalities that were isolated in 2010 and thus forced to join an IC (treatment group), and 0 for municipalities already integrated by 1999 (control group). δ_t and θ_m are time and municipality fixed effects, respectively. The inclusion of municipality fixed effects controls for any

[20] There are only two exceptions: one municipality integrated in 2015 and another in 2017. Both went to court to challenge the decision made by the prefect, but ultimately had to comply with it.

time-invariant unobserved factors; the inclusion of year fixed effects captures changes over time that affect all municipalities the same way. Standard errors are clustered at the municipality level.[21]

Thanks to the sharp design, namely the fact that I consider only two periods and that all municipalities in the treatment group were effectively forced to integrate after 2010, the analysis circumvents the issues raised by the recent literature studying treatment effects in the context of staggered adoption designs (e.g., de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2022).

In order to explore the dynamics of the effect, I also plot for each outcome the coefficients of the following leads-and-lags regression:

$$Y_{mt} = \alpha + \sum_{k=2004}^{2018} \beta_k 1_{\{t=k\}} 1_{\{treated_m=1\}} + \delta_t + \theta_m + \varepsilon_{mt}, \tag{2}$$

where $1_{\{t=k\}}$ is an indicator variable equal to 1 for year k. All coefficients are normalized relative to 2010. Moreover, to account for the fact that some treated municipalities integrated later than others after the law (Section 2.2), I further report the results for different subsamples, by splitting the treatment group according to the exact year municipalities entered an IC (see Section 5.1).

Outcomes are standardized in order to give the same weight to municipalities of different sizes. I divide each outcome by the 2010 municipal population and then multiply by 10,000. Hence, Y_{mt} is, for instance, the number of housing building permits delivered in municipality m during year t per 10,000 inhabitants, using the 2010 population. In an alternative specification, I divide the outcome by the current population of year t using the number of households living in the municipality.[22] The results remain qualitatively unchanged, as further discussed in the main text.

The identification assumption is that absent the law, municipalities in the control and in the treatment groups would have evolved the same way. Under this assumption, the main coefficient of interest β captures any deviation from a parallel evolution in the outcome of interest between the treatment and the control groups due to the 2010 law. I discuss below the potential threats to identification and present the additional analyses I perform to support the identification strategy.

Selection into treatment. The first concern is the selection into treatment: municipalities in the control group chose to integrate early, whereas those in the treatment group chose to remain isolated. The two groups are thus clearly selected. This would be an issue if I were to compare the two groups in levels. Instead, the empirical strategy consists in comparing their trends. Hence, as

[21] I also run specifications with standard errors clustered at the IC level, considering municipalities' IC either in 2014 or 2018. While, as expected, the standard errors are generally slightly larger, all main results remain significant at similar levels (see Appendix C2).

[22] The municipal population computed by the annual census is an average over 5 years, with different municipalities being surveyed at different times. The census survey's method makes any census measures challenging to use in a panel. I thus approximate municipalities' size in year t using the number of households computed by the Ministry of Finance based on income tax declarations.

long as the treatment and control groups have similar trends around the time of the law, any deviation from this trend after the law can be interpreted as the causal impact of the treatment—forced integration—on the treated—resisting municipalities. The question is thus whether we can expect those two groups to display parallel trends in the outcomes of interest. Two main aspects of the setting might threaten the validity of the common trend assumption. I address them formally below. First, the fact that treated municipalities resisted integration implies that they differ in some aspects and these differences might affect their trends. Reassuringly, Section 3 shows that treated municipalities are quite similar to other municipalities based on socio-demographic, land-use, and political characteristics. Still, I also show that, while similar on average, treated municipalities tend to join ICs encompassing larger municipalities and lose more bargaining power upon integration, ultimately explaining why they bear the costs of integration. The identification thus rests on the assumption that these differences are orthogonal to the outcomes' trajectories absent treatment. This seems plausible: if having relatively larger neighbors influenced public service and construction trends prior to 2010, we would expect the two groups to differ in 2010 based on socio-demographic and land-use characteristics, which is not the case (Table A1). Instead, I argue that these differences start to matter only once part of an IC, as they make municipalities less able to fight ICs' decisions. Second, one could question whether control municipalities experience different trends because they are part of an intermunicipal community. Removing municipalities that integrated after 1999 alleviates the concern that control municipalities' shifts in integration status affect their trends during the period of interest. Nevertheless, being part of an IC might cause control municipalities to evolve differently across time and to react differently to shocks.

This is ultimately an empirical question and, while the common trend assumption cannot be directly tested, I run several analyses to provide support for it: I test for the presence of pre trends for all outcomes, I estimate a series of placebo reforms by pretending that treated municipalities were forced to enter an IC before 2010,[23] and I show the robustness of the results to using propensity score matching and to including time-varying controls—namely, municipality size proxied by the number of households and the average household's annual taxable income. I present the results of these tests for each outcome in Sections 5 to 7.

Possible effects of the reform on control municipalities. We can interpret the estimated β of Equation (1) as the causal impact of integration on the treated municipalities, provided that only municipalities in the treatment group are affected by the law. However, some municipalities in the control group might also be impacted—those that are part of an intermunicipal community into which a treated municipality entered, as well as those that are part of ICs that fall below the 5,000-inhabitant threshold and therefore had to enter a larger IC or whose IC had to merge with another

[23] I cannot run the placebo tests on daycare and public libraries, given the few pre-treatment periods I have in the data. This analysis is thus restricted to the number of building permits, fiscal revenues, and public transport.

one. To address this concern, Appendix C5 reports the main results using several alternative control groups: excluding control municipalities whose IC received a municipality forced to integrate; excluding all control municipalities whose IC changed between 1999 and 2014 as a result of the 2010 law; excluding all control municipalities sharing a common border with a treated municipality to address potential spillovers; excluding all the aforementioned control municipalities. The effects are very similar across these samples, supporting the fact that the estimated effects capture the impact on the treated municipalities only.

5 Impact on housing supply

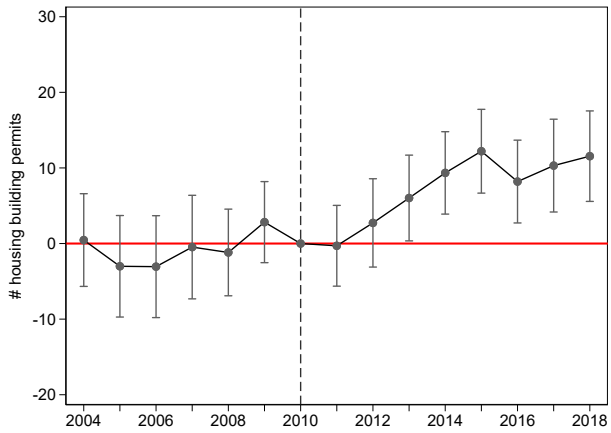
5.1 Impact on municipalities forced to integrate

In order to test whether resisting municipalities experienced an increase in construction after integration (Prediction 2), I estimate the impact of integration on the number of housing building permits delivered in municipalities forced to enter an IC. The outcome is the number of housing units allowed for construction in a given year per 10,000 inhabitants (using the 2010 population).

Figure 2 plots the coefficients of the leads-and-lags regression. Vertical lines represent the 95-percent-confidence intervals. First, no coefficient before 2010 is significant and all coefficients prior to the law are close to 0 in magnitude. The F-stat for the joint significance of the pre-reform estimates is 0.74 (p-value 0.617). In line with the absence of pre-trends, none of the placebo reforms have a significant effect on housing supply (Appendix Figure C4). Instead, after the 2010 law, we observe a large increase in the building permits delivered in treated municipalities compared to control municipalities. The increase starts in 2012, reflecting the fact that most municipalities forced to integrate joined an IC after 2011.

To better understand the dynamics of the effect, Appendix Figure A2 further splits the treatment group according to the exact year municipalities entered an IC. For each integration period, we see that the increase in construction starts right after integration, and we see no pre-trends before the actual integration year. This shows that the main effect captures the impact of entering an IC, rather than the effects of other potential factors affecting the two groups differently after 2010. Moreover, the impact is similar for municipalities that integrated just after the law or a few years later, and it lasts over the whole post-treatment period, suggesting that the increase in construction is unlikely to come from a temporary adjustment but rather reflects a more permanent change in regulations.

Figure 2: Impact on housing building permits



Notes: This graph plots the estimates and 95-percent-confidence intervals from the leads-and-lags regression (see Equation (2)). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population).

Table 1 provides the formal estimate. On average, the number of building permits increased by 8.1 per year per 10,000 inhabitants after 2010 in treated municipalities compared to control municipalities, an effect significant at the 1-percent level.[24] As, on average, resisting municipalities were delivering 64.8 building permits per year per 10,000 inhabitants before 2010, their integration led to a yearly increase of 12.5 percent. The impact is similar in magnitude when dividing the outcome by the number of households in year t instead of the population in 2010 (13.1 percent) and robust to controlling for municipality size and for household annual income in year t (Appendix Table A2), showing that resisting municipalities experienced an increase in building permits per current capita.

Finally, as shown in Columns 2 and 3, the increase is mainly driven by new construction: the number of building permits to build new housing units on empty land increased by 6.8 on average, while the number of building permits to add units to existing building increased by only 1.3. This result shows that resisting municipalities had to expand their share of built land once integrated, which is consistent with municipalities losing control over both housing supply and land-use policies.[25] As nothing prevented them from building more prior to the law, this rise in construction is unlikely to reflect their preferences; it is best interpreted as a consequence of their

[24]The size of the impact is very similar when using propensity score matching (7.2) and the estimate remains significant at the 1-percent level (see Appendix Table C3.3).

[25]While the data do not specify whether the building permit is delivered for social or regular housing, the increase in construction is most likely coming from an increase in regular housing. In France, social housing is concentrated in the largest municipalities (above 3,500 inhabitants), where it is mandatory (Gobillon and Vignolles, 2016). As shown in Appendix Table B5, the impact on housing supply is very stable across municipality size, with municipalities below 1,000 or below 500 inhabitants experiencing a similar rise in construction per capita.

loss of autonomy.[26]

Table 1: Impact on housing building permits

	(1)	(2)	(3)
Outcome	Number of building permits per 10,000 inhabitants		
	All	New	Extensions
Treatment	8.141***	6.810***	1.331***
	(1.517)	(1.391)	(0.496)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	245,940	245,940	245,940
Mean DepVar	64.836	53.922	10.914
Sd DepVar	90.844	82.793	35.660

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The last two columns distinguish between two types of building permits: for the construction of a new unit on empty land (Column 2) or for the extension of an existing housing building (Column 3).

5.2 Comparison with municipalities that voluntarily integrated

I now investigate the impact of integration on municipalities that joined an IC voluntarily prior to the law. If municipalities resisted integration to avoid such increase in housing supply, we should see a differential impact for municipalities that instead chose to integrate (Prediction 1).

I consider municipalities that were previously excluded from the main sample of analysis: those that joined an IC between 2000 and 2010.[27] I apply the same geographical restrictions as for the main sample of analysis (see Section 4.1), resulting in a balanced panel of 11,894 municipalities.

[26]These effects are unlikely to come from a change in the benefits associated with new construction. If anything, the fact that the housing tax revenues are shared at the IC level after integration should make municipalities even less likely to start new construction. Moreover, if a change of incentives explains the results, we should see similar effects for municipalities that chose to integrate voluntarily prior to the law. Instead, as shown in the next section, municipalities that joined an IC voluntarily prior to the law did not experience the same increase in construction.

[27]I focus on municipalities that joined an IC voluntarily starting in 2000 for two reasons. First, the municipal composition of French ICs is only available from 1999, meaning that we do not know in which year municipalities that integrated earlier entered an IC. Second, the structure of ICs changed following the Chevènement law in 1999. Hence municipalities that integrated voluntarily after 1999 entered ICs closer in characteristics to the ICs that resisting municipalities entered, making the comparison more relevant.

To estimate the impact of their voluntary integration, I take advantage of the fact that they entered an IC at different times. Formally, I use a staggered adoption design, in which the date of the treatment is the year when the municipality first joined an IC. I run the analysis over the period from 1999 to 2018: in 1999, no municipality in this sample is part of an IC, whereas they all are starting in 2010. I observe municipalities up to 11 years prior to the integration (for municipalities integrating in 2010) and up to 18 years after the integration (for those integrating in 2000). I estimate the following dynamic specification:

$$Y_{mt} = \sum_{d=-5}^{d=5} \beta_d 1_{\{t=t_{m0}+d\}} + \gamma_1 1_{\{t<t_{m0}-5\}} + \gamma_2 1_{\{t>t_{m0}+5\}} + \delta_t + \theta_m + \varepsilon_{mt}, \tag{3}$$

where the year of integration of municipality m is denoted t_{m0}, and d indexes time-to-integration (negative before integration and positive after). The β_ds measure the difference between municipalities that are part of an IC and those not already part of a community, for each of the five years preceding and following the integration. γ_1 (resp. γ_2) estimates the effect for being more than five years before (resp. after) the integration. All coefficients are normalized relative to the year preceding the integration ($d = -1$). I include time and municipality fixed effects; standard errors are clustered at the municipality level.

In light of the recent literature on the issues associated with event study designs (e.g., de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2022), I also use an alternative estimation procedure developed by de Chaisemartin and D'Haultfoeuille (2020). By using "not-yet-switchers" as controls, the authors provide new estimators that are robust to heterogeneous treatment effects across groups and over time.[28]

Figure 3 plots the coefficients, taking as outcome the number of housing building permits delivered per year per 10,000 inhabitants[29] and applying the same scale as in Figure 2. The left-hand graph shows what I obtain using the regular staggered adoption design (referred to as method 1) and the right-hand graph what I obtain using de Chaisemartin and D'Haultfoeuille (2020)'s method (referred to as method 2).

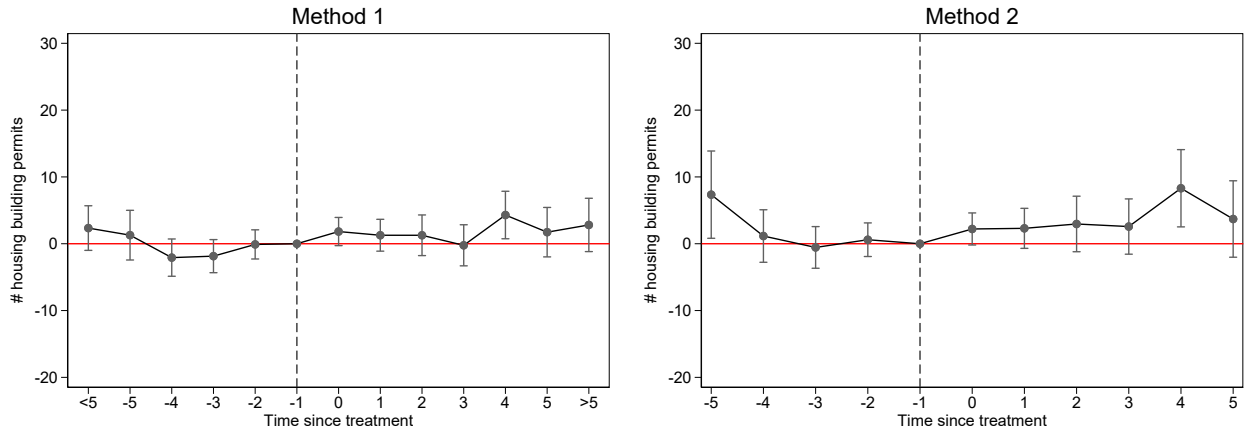
In both graphs, only one coefficient is statistically significant in the post-treatment period. The impact of integration is close to 0 except in the fourth year after integration, when the number of building permits increases by 4 to 8 depending on the method. The effect vanishes the year after and remains nonsignificant afterwards. These results contrast with the experience of municipalities forced to integrate, namely a large increase in construction starting right after their integration and

[28] For more details on de Chaisemartin and D'Haultfoeuille (2020)'s method for staggered adoption design, see Section 5.2 of their paper. Note that this method allows me to compute the estimated effect of being a given number of years before or after the treatment, but not to compute the estimated effect of being more than 5 years before or after the date of the treatment. Both estimates are thus absent in the right-hand graph in Figure 3.

[29] Because I consider municipalities integrating from 2000, I divide the outcome by the 1999 population instead of the 2010 population.

continuing over the entire post-treatment period (Figure 1 and Appendix Figure A2).

Figure 3: Comparison with municipalities that voluntarily integrated: housing building permits



Notes: The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample is made up of municipalities that voluntarily integrated between 2000 and 2010. The left-hand graph plots the estimates and 95-percent-confidence intervals from a regular staggered adoption design (method 1). The right-hand graph uses de Chaisemartin and D'Haultfoeuille (2020)'s method, implemented using the Stata command did_multiplegt, available on SSC repository (method 2). In method 1, the period of analysis goes from 1999 to 2018, whereas in method 2 it goes from 1999 to 2009. More information on the two methods in Section 5.2.

While municipalities that voluntarily integrated and municipalities forced to integrate entered an IC in different periods and thus in different economic climates, this is unlikely to account for the difference I find. First, when using method 1, I include all years from 1999 to 2018, meaning that I also measure the impact of voluntary integration on the number of building permits delivered after 2010, in the same period as for resisting municipalities. Second, the results are similar when focusing on municipalities that voluntarily integrated starting in 2004, closer to the integration date of the ones forced to integrate (Appendix Figure A3). In 2004, the number of building permits delivered in France was close to its 2010 level (around 400,000) and it was following a similar increasing trend as in 2010 (Appendix Figure A4).

Hence, only municipalities that did not want to enter an IC experienced a significant rise in construction. This differential impact supports the view that treated municipalities opposed integration to avoid the local costs of integration, beyond purely ideological or political considerations. Section 8 discusses further the differential impact between resisting municipalities and those that voluntarily integrated and provides additional evidence supporting this interpretation.

5.3 Heterogeneity analysis

According to Prediction 4 (a), the impact on construction should be driven by desirable municipalities. Indeed, they are the ones the most likely to use local regulations to avoid an increase in density and this is where ICs are the most likely to impose new construction, making the loss of autonomy over urban planning particularly costly for them.

Attractiveness. I proxy municipalities' attractiveness by their location in the urban area. Consistent with the monocentric model,[30] the demand for housing in French municipalities is the highest closer to the local center of employment and thus closer to the core of the urban area (Combes et al., 2018).[31] As shown in Table 2 and Appendix Figure A5, the impact on housing supply is driven by treated municipalities that are part of an urban area: after integration, they experienced an average increase of 10.5 in the number of building permits delivered per year per 10,000 inhabitants, an effect significant at the 1-percent level, while municipalities outside any urban area experienced an average increase of only 3.1, which is not significant (Columns 2 and 3).

The increase is largest for municipalities in the core (24.5 or 38.3 percent per year, Column 4) and a shorter Euclidean distance to the core is associated with a stronger effect of integration on housing supply (Appendix Table D1). Using residents' income as an alternative measures of attractiveness, I also find that the impact on construction is higher the richer the municipality, whether I divide the sample according to the median (Appendix Tables A3) or interact the treatment variable with income (Appendix Table D2).[32]

Density. Among desirable locations, the denser the municipality is, the more likely it is to use restrictive housing regulations, as this is when new construction most reduces the space available. Focusing on municipalities inside an urban area, I further show that the effect is indeed even larger for those that are relatively more densely built (Appendix Table A4 and Figure A6). Although the coefficients for urban municipalities are noisier—they make up 30.5 percent of the sample—the increase after 2010 is larger in magnitude: the number of building permits increased by 15.5 per

[30]The monocentric model predicts that the housing demand is the highest in the center (Alonso, 1964). This is consistent with most housing markets in developed countries, with the notable exception of the US, where the more desirable places are usually in the suburbs (see, for instance, Jackson, 1987).

[31]Close to the definition of American metropolitan areas, a French urban area is defined as a group of neighboring municipalities encompassing an urban core (urban unit) providing at least 5,000 jobs, and by rural districts or urban units (urban periphery) among which at least 40 percent of the employed resident population works in the core or in the municipalities attracted by this core. I use the 2010 urban area breakdown and consider both large (providing at least 10,000 jobs) and medium (providing between 5,000 and 10,000 jobs) urban areas. The results are left unchanged when using the 1999 breakdown instead. In 2010, France counted 372 urban areas, encompassing two thirds of the municipalities and 85 percent of the population. The average share of built land in municipalities that are part of an urban area is 1.1 percent, versus 0.4 percent for municipalities outside any urban area.

[32]Due to income sorting, residents' income is positively correlated with the probability that a municipality is part of an urban area. To alleviate the concern that the income heterogeneity results are solely driven by this correlation, Appendix Table B6 replicates Table A3 considering only municipalities that are part of an urban area. As for the full sample, the impact is twice as large for municipalities above the income median.

year (23.6 percent, Column 2) versus 9.9 in rural municipalities (15.7 percent, Column 3).[33] Similarly, when splitting the sample according to the number of housing units per square kilometer in 2010, the impact is larger for municipalities above the median (13.9 vs. 9.3, Columns 5 and 4). Interacting the treatment variable with housing density, we see that a one-standard-deviation increase in housing density increases the treatment effect by 8.5—or more than half—compared with the impact on housing for an average treated municipality in the urban area (Appendix Table D3).[34]

Overall, the results presented so far support the predictions 1, 2 and 4 (a): only resisting municipalities experienced an increase in construction and this effect is driven by high-demand urban municipalities, where the IC was able to overrule local restrictive regulations.

Table 2: Impact on housing supply: Urban area

	(1)	(2)	(3)	(4)
Outcome	Number of building permits per 10,000 inhabitants			
	All	Outside	Inside	Core
Treatment	8.141***	3.105	10.494***	24.471***
	(1.517)	(2.585)	(1.812)	(5.362)
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	245,940	94,440	151,500	28,785
Mean DepVar	64.836	66.897	63.660	63.875
Sd DepVar	90.844	99.952	85.197	97.750

Notes: Column 2 (resp. 3) includes only municipalities that are not part (resp. part) of an urban area. Column 4 considers only those in the core of the urban area. The municipal composition of urban areas is based on the INSEE 2010 breakdown and I consider both large (providing at least 10,000 jobs) and medium (providing between 5,000 and 10,000 jobs) urban areas. Other notes as in Table 1.

Bargaining power. Finally, I test whether the impact is higher for resisting municipalities that lost the most bargaining power, as their low bargaining power can explain why they bear the local costs of integration. Focusing on municipalities part of an urban area that are driving the main effect, Table 3 shows that the impact on construction is larger for resisting municipalities that

[33] Based on international standards, an urban municipality is a municipality belonging to an urban unit, defined as a continuously built up zone (no cut of more than 200 meters between two constructions) with at least 2,000 inhabitants.

[34] The differential impact between more or less dense municipalities is unlikely to be explained by the fact that these municipalities enter different types of ICs. As shown in Appendix Table B7, the impact is twice as large for urban municipalities as for rural municipalities, even when considering only municipalities ending up in ICs required by law to issue the most stringent housing plans ("CA" and "CU"; see Section 2.1).

joined ICs encompassing big cities that can impose their decisions more easily (13.0 vs. 6.1), a difference that is statistically significant (pvalue of 0.050).

Interestingly, the impact is instead not stronger for municipalities that are surrounded by neighboring municipalities that are more different from them, in terms of income, unemployment, immigrant share or political preferences (Appendix Tables B8-B11 and D4-D5).[35] This shows that what make them more vulnerable to ICs decisions is their relatively lower bargaining power inside the IC rather than other observable differences with their neighbors. This also suggests that ICs are particularly effective at overcoming NIMBYism when the municipalities restricting housing development have a relatively lower bargaining power compared to the other members.

Table 3: Impact on housing depending on whether the IC encompasses a big city: Urban areas

	(1)	(2)	(3)
Outcome	Number of building permits		
	All	No big city	Big city
Treatment	10.494***	6.093**	12.961***
	(1.812)	(2.507)	(2.453)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	151,500	67,860	83,625
Mean DepVar	63.660	65.957	61.974
Sd DepVar	85.197	87.040	83.792

Notes: The sample includes only municipalities part of an urban area. Column 2 (resp. 3) includes only municipalities that, in 2014, were part of an IC in which all municipalities were below 5,000 inhabitants (resp. encompassing a municipality above 5,000 inhabitants). Other notes as in Table 1.

5.4 Impact on housing prices and economic activity

I next explore the implications of the increase in construction for housing prices and economic activity in municipalities forced to integrate.

[35]For each socio-demographic characteristic, I construct a ratio equal to the value for the municipality divided by the average value in neighboring municipalities, weighted by their population. To measure local political alignment, I first look at whether, in 2010, the mayor had the same political orientation as the member of parliament elected by the municipality's legislative district. I then consider the absolute value of the difference between the share of voters in the municipality and the share of voters in surrounding municipalities who voted for the right-wing candidate in the second round of the 2007 presidential election, in which a candidate from the right faced a candidate from the left. I define surrounding municipalities as either all other municipalities from the same département or considering only direct neighbors—those sharing a border. Appendix Tables B8-B11 split the sample according to the median value of the heterogeneity variable, while in Appendix Tables D4 and D5 I interact the treatment variable with the heterogeneity variable. The interactions terms are small and only one out of 10 is significant at the 10 percent level across the different heterogeneity variables and specifications.

Housing Prices

To investigate the impact of integration on municipalities' housing prices, I built indices based on official housing transactions, which can be interpreted as the price per square meter of a reference dwelling. As data on housing transactions for the Parisian region of Île-de-France and for the rest of France come from different databases, I present the results for the two parts of France separately (see Appendix E for more details).

While the increase in construction in resisting municipalities is likely to affect housing prices throughout the local housing market, my goal is to assess the consequences for resisting municipalities only. I thus run the analysis at the municipal level and I consider a balanced sample of municipalities, restricting the analysis to those in which at least one transaction took place every even year. This results in a sample of 459 municipalities in the Parisian region and 6,756 in the rest of France (44.0 percent of the initial sample in total). Considering instead all municipalities with at least one transaction during the period of interest almost doubles the sample size and leaves the results unchanged (Appendix Figure E1). Given that municipalities that are part of an urban area have more housing transactions, municipalities driving the impact on construction are overrepresented: municipalities part of an urban area (resp. urban core) make up 76.7 percent of the sample (resp. 23.8 percent) versus 61.6 percent (resp. 11.7 percent) of the full sample.

Appendix Figure A7 shows a small decrease in housing prices in 2014 for treated municipalities in the Parisian region, but the estimated impact is not significant and is small in magnitude (-56 euros per square meter, or 2.3 percent). The decrease is even smaller for the rest of France, suggesting that overall, forced integration had no significant impact on housing prices. While there are some early pre-trends for the rest of France, the post-treatment effect remains close to 0 when considering the unbalanced panel, for which the pre-trends are no longer significant (Appendix Figure E1). Moreover, the same conclusions hold when using an alternative version of the indices in which I include a larger set of housing characteristics in the hedonic regressions and when restricting the analysis to municipalities inside the urban area (Appendix Figures E2 and E3).

The null effect on prices could be explained by the fact that the increase in housing supply is concentrated in resisting municipalities, as opposed to occurring at the housing market level. Indeed, as stressed by Pollakowski and Wachter (1990) and Lin and Wachter (2020), when housing constraints are relaxed at the "individual" level, we can expect both a direct and an indirect effect on prices. On the one hand, the increase in housing supply leads to a drop in prices in the municipality (direct effect). On the other, the increase in construction affects the reallocation choice of households inside the housing market, as would-be residents can now move to the municipality in which regulations have been relaxed, depressing the demand in nearby municipalities and increasing the demand in the municipality (indirect effect). This spillover effect through the demand is larger the more attractive the municipality is compared to its neighbors. It can compensate for the

direct effect, leading to an overall null impact on prices. This compensation effect is particularly likely in our context, since desirable locations are driving the increase in construction.

One alternative explanation is that, while housing prices are supposedly forward looking, it takes time for the increase in building permits to lead to a significant decrease in prices. As shown by Glaeser and Ward (2009) in the context of Boston, land use restrictions impact housing prices mainly through changes in density and demographic composition, which take time to materialize.

Both interpretations suggest that the main cost coming from the loss of regulatory power and increase in construction is a decrease in the quality of life rather than a drop in housing values. This is consistent with the fact that the impact on construction is not stronger for municipalities with a higher share of homeowners (Appendix Table A5 and Appendix Table D6), showing that places that restrict housing supply do not have higher homeownership rates.[36] This is also in line with the lack of empirical evidence supporting the homevoter hypothesis in other contexts (e.g., Hilber and Robert-Nicoud, 2013) and with recent evidence showing that renters can express as much NIMBYism as homeowners, particularly in desirable locations (e.g., Hankinson, 2018).

Economic Activity

I then test whether the increase in building permits is associated with a rise in economic activity, which could mitigate the disutility associated with the increase in housing density. For instance, new housing developments could boost employment in the municipality and create incentives for new businesses to move there on top of new residents. I first proxy economic activity by firm creation and assess the impact of integration on the number of new establishments created per 10,000 inhabitants in municipalities forced to integrate. I then measure the impact of integration on residents' total wages, divided by the number of inhabitants.[37]

The point estimates are close to 0 and nonsignificant for both outcomes, whether we look at the full sample or only at municipalities that are part of an urban area (Appendix Figures A8 and A9). The fact that there are no pre-trends and no effect shortly after the 2010 law provides additional evidence that the rise in construction is caused by a change in housing regulations after integration, rather than by a change in economic conditions. The null effects over the whole the post-treatment period also suggests that, at least in the medium run, the increase in construction in residents' backyards is not compensated for by greater economic activity.

[36] To account for the fact that homeownership is negatively correlated with housing density, Appendix Table B12 reproduces the same analysis restricting the sample to urban municipalities. As for the full sample, the impact is equally strong for urban municipalities above and below the median.

[37] Those two variables are available for each year. Instead, the unemployment rate comes from the census and is computed as an average over 5 years, with different municipalities being surveyed at different times, making it challenging to use in a panel.

6 Impact on local public services

In order to test whether resisting municipalities experienced a loss of public services (Prediction 3), I gathered local data on two different public services transferred to the IC after integration: child daycare and public libraries. I explore the impact on the number of daycare spots available and on the number of public libraries located in municipalities forced to integrate, per 10,000 inhabitants (using the 2010 population). Moreover, to test Prediction 4 (b)—that the effect is driven by peripheral low-density places—I present the results separately for urban and rural municipalities.

The data are less comprehensive than the data on building permits. First, data on daycare and libraries start in 2007 and 2009, respectively, which does not leave enough time prior to the law to compare the main results with the effects of integration for municipalities that voluntarily integrated before 2010. Moreover, while data on daycare are available for the whole territory, data on libraries are available for only seven départements.[38] Still, the results go in the same direction for both daycare and libraries, strengthening the conclusions we can draw about the effects of integration on local public services in resisting municipalities.

As shown in Figure 4, the decreasing trends for rural municipalities indicate that rural municipalities forced to integrate ended up with fewer public services after the law, compared to rural control municipalities. We see no such decline for urban municipalities and, if anything, the trend is even slightly increasing after the law.[39]

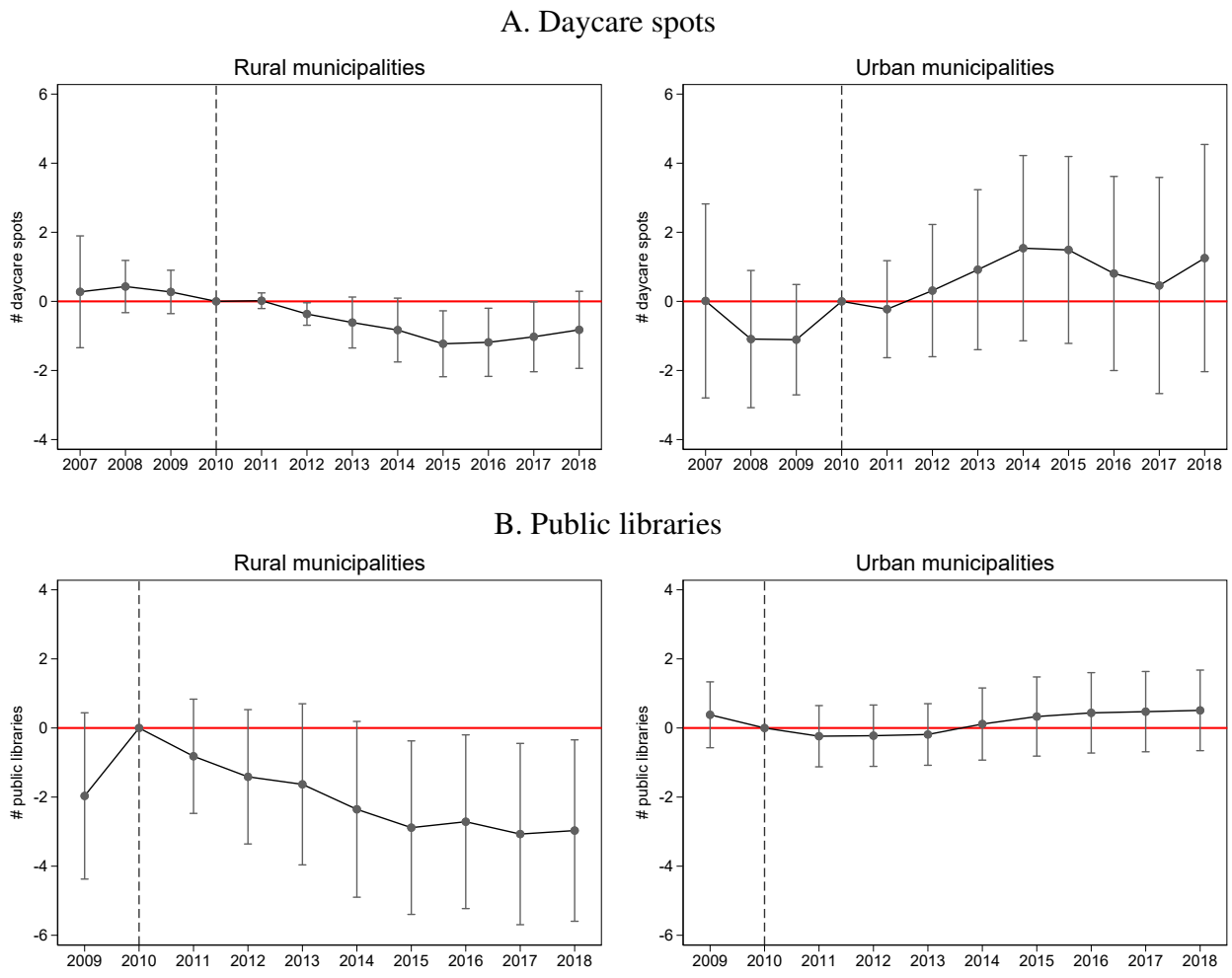
Table 4 provides the estimates for the full sample and separately for rural (Columns 2 and 5) and urban municipalities (Columns 3 and 6). In line with the graphical evidence, the coefficient for rural municipalities is negative and significant at the 5-percent level for both public services, whereas the coefficient for urban municipalities is either positive or close to 0 and not significant. The difference between the coefficients for rural and urban municipalities is statistically significant (pvalues of 0.085 and 0.080 for the number of daycare spots and public libraries, respectively). In the average year after 2010, rural municipalities forced to integrate had 1.0 fewer daycare spot and 1.2 fewer public libraries per 10,000 inhabitants, compared to rural control municipalities (a decrease of 27.7 and 23.6 percent, respectively). The number of daycare facilities follows a similar pattern, suggesting that the loss of entire daycare facilities is what is driving the decrease in the number of daycare spots (Appendix Figure A11 and Table A6). Interestingly, the resulting effect on the gap in public good provision between rural control and rural treated municipalities differs for the two public services. On the one hand, rural municipalities forced to integrate already had fewer daycare spots per capita in 2010, meaning that their integration further increased the gap

[38] The seven départements are in different parts of the country: Aisne (in the north of France), Finistère (west), Drôme (southeast), Gironde and Dordogne (southwest), and Essonne and Val d'Oise (center north). They make up 9 percent of the municipalities in the main sample of analysis.

[39] See Appendix Figure A10 for the impact on the full sample.

with rural control municipalities. On the other, they had more public libraries per capita, meaning that their integration brought them closer to rural control municipalities.[40]

Figure 4: Impact on daycare and public libraries



Notes: These graphs plots the estimates and 95-percent-confidence intervals from the leads-and-lags regression (see Equation (2)). In panel A, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population). In panel B, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population) and the sample is restricted to the 7 départements for which data are available starting in 2009. The graphs on the left-hand side includes only rural municipalities, while the graphs on the right-hand side include only urban municipalities.

The decrease in public services remains significant and the effect size is of similar magnitude when dividing the outcomes by the number of households in year t rather than by the 2010

[40] In 2010, rural resisting municipalities had on average 4.0 daycare spots per 10,000 inhabitants, compared to 6.5 for rural control municipalities, while having the same average number of children (see Table A1). In contrast, rural resisting municipalities had on average 6.7 public libraries per 10,000 inhabitants, compared to 4.5 for rural control municipalities.

population and when controlling for municipalities' size and residents' revenues (Appendix Table A7). This suggests that the impact is not driven by a differential change in the population of rural treated and control municipalities after 2010. Instead, these results are consistent with the loss of public services being driven by decisions made at the IC level to concentrate resources in denser municipalities, explaining rural municipalities' resistance to integration in the first place.

To provide further support for this interpretation, I assess the impact of integration on public schools. If the impact on daycare and libraries is driven by the loss of control over public services, we should not find the same pattern for public services that are unlikely to be directly affected by integration. I look at the number of preschools (*maternelles*) and primary schools in the municipality and then at the number of higher-level schools, including secondary schools, high schools, and universities. The former are managed both at the national and municipal levels, but municipalities' role is limited (see Section 2.1). The latter are managed at the national and departmental or regional levels. Unlike the case of daycare and public libraries, integration does not lead to a decrease in the number of public schools in rural municipalities forced to integrate and the point estimates are close to 0 (Appendix Figure A12).

Table 4: Impact on daycare and public libraries

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Daycare spots			Public libraries		
	All	Rural	Urban	All	Rural	Urban
Treatment	-0.616	-1.001**	1.366	-0.913**	-1.248**	-0.039
	(0.461)	(0.463)	(1.292)	(0.460)	(0.620)	(0.305)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	196,752	153,216	43,536	14,290	11,020	3,270
Mean	10.620	3.617	37.947	4.374	5.277	2.032
Sd	38.600	31.900	48.954	9.574	10.983	3.089

Notes: In Columns 1 to 3, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population); the period of analysis is 2007-2018. In Columns 4 to 6, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population); the period of analysis is 2009–2018 and the sample is restricted to the 7 départements for which data are available starting in 2009. Columns 2 and 5 include only rural municipalities, while Column 3 and 6 include only urban municipalities. Other notes as in Table 1.

7 Impact on public transports and fiscal revenues

I now turn to two last additional aspects of consolidation that should entail benefits without specific local costs. If municipalities resisted integration to avoid the local costs, we should see that they enjoyed similar benefits as municipalities that voluntary integrated (Prediction 5). This analysis also enables us to see what resisting municipalities were ready to give up by not integrating.

7.1 Impact on public transport

By enhancing cooperation and enabling municipalities to pool resources, integration is likely to help neighboring municipalities build larger and more efficient public transport networks. Moreover, the transfer of public transport networks to the IC level was made with the explicit goal of connecting as many municipalities as possible inside the IC perimeter (BANATIC, 2014). As a result, joining an IC might increase a municipality's probability of being connected. I estimate the impact of integration on an indicator variable equal to 1 if the municipality is connected to a public transport network at some point during the year (there is at least one public transport route operating in the municipality, which, in the vast majority of the cases, is a bus line) and 0 if the municipality is not connected (there is no public transport route). This analysis excludes municipalities in Île-de-France, for which data on public transport are not available.

Figure 5 shows the impact for municipalities forced to integrate. The decreasing pre-trend suggests that before 2010, access to public transport increased more rapidly for control municipalities than for treated municipalities. One plausible explanation is that coordination inside ICs helped control municipalities develop transport networks more rapidly. In contrast, the large increasing trend after 2010 shows that, after the law, access to public transport increased more rapidly for municipalities forced to join an IC. In line with the presence of pre-trends, the coefficients associated with the placebo reforms before 2010 are significant but small and negative, contrasting with the large and positive effect of the true reform (Appendix Figure C4).

Table 5 indicates that resisting municipalities' probability of being connected to a public transport network is 3.2 percentage points higher in the average year after 2010, an effect significant at the 1-percent level. Given that only 2.4 percent of treated municipalities had access to public transport before 2010, their entry into an IC more than doubled their probability of being connected. In 2017, the effect reaches 6.8 percentage points, a nearly three-fold increase. The point estimate is larger for urban than for rural municipalities (10.6 vs. 2.0, Columns 3 and 2), but the magnitude relative to the pre-treatment mean is similar and both estimates are significant at the 1-percent level. The impact is robust in magnitude and statistical significance to using propensity score matching or to adding time-varying controls (Appendix Tables A8 and C3.5).

I then measure the impact of integration on public transport for municipalities that voluntarily

joined an IC before 2010, using the same estimation methods as in Section 5.2 (Appendix Figure A13). The results obtained with the regular staggered adoption design suggest that, on average, the probability of having access to public transport is 2 percentage points higher in the years following their integration. Using de Chaisemartin and D'Haultfoeuille (2020)'s method, I find that the impact increases over time and reaches 4 percentage points four years after integration. This is a sizable effect, given that only 3.4 percent of the municipalities that integrated between 2000 and 2010 had access to public transport in 1999. Hence, integration had a large positive effect on public transport for both municipalities that voluntarily joined an IC and those that resisted integration. Moreover, as the share of residents using their cars was the same in both types of municipality in 1999—before any of them integrated—we can conclude that they benefited equally from this increase in public transport.[41]

7.2 Impact on fiscal revenues

I study the effect of integration on the resources available to finance municipalities' public services. Integration is likely to lead to an increase in fiscal revenues through two main channels. First, while a municipality's own state transfers do not change after integration, it does benefit from the additional state transfers allotted each year to the IC. Second, tax collection at the IC level might decrease tax competition and lead to higher tax revenues compared to what the municipality was collecting on its own.

I compute municipalities' resources as follows. If a municipality is isolated, its fiscal revenues are made up of its own tax revenues and state transfers. If a municipality is part of an IC, I compute its fiscal revenues as the sum of its own fiscal revenues and the total IC's fiscal revenues scaled by the municipality's share of the total IC population.[42] I then divide the total fiscal revenues by the municipal population in 2010. This outcome measures the resources available per resident, assuming that every resident benefits equally from the IC's spending. While the results of Section 6 suggest otherwise, comparing the impact for municipalities forced to integrate and those that voluntarily integrated enables me to test whether resisting municipalities entered ICs generating as much extra revenues.

As shown in Figure 5, contrasting with the small decreasing trend before 2010, municipalities forced to integrate had a large increase in fiscal revenues after integration. As with public transport, the estimates associated with placebo reforms are small and negative, contrasting with the large and positive effect of the true reform (Appendix Figure C4). Table 5 indicates that, on average,

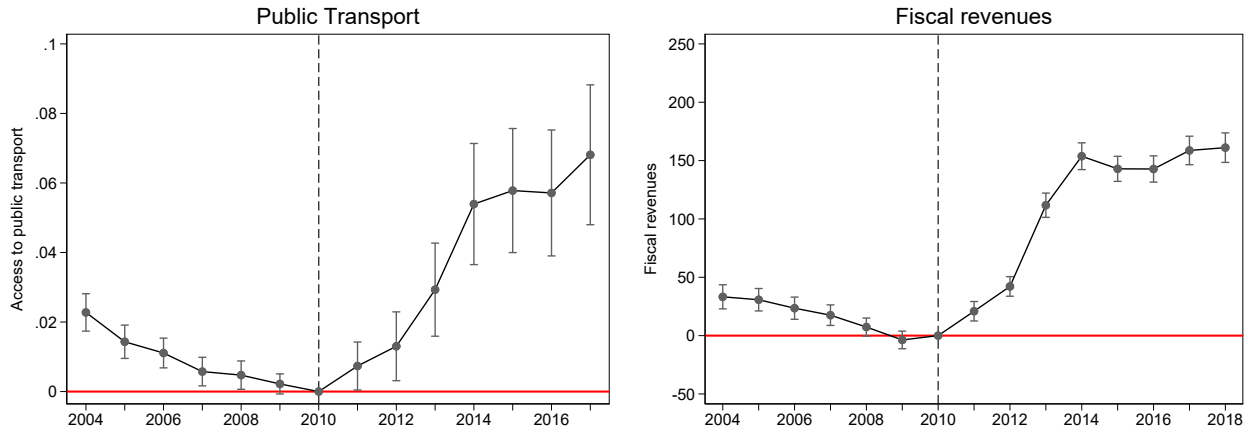
[41] Statistics from the 1999 census indicate that 75.3 percent of resisting municipalities' residents were using their cars to go to work compared with 76.7 percent for municipalities that voluntarily integrated between 2000 and 2010.

[42] If the IC gives direct transfers to one of the member municipalities—for instance, as part of the "solidarity transfers" to poorer municipalities in the community—I consider these transfers as part of the revenues of that municipality only, but not as part of the total IC revenues shared among all members.

integration increased fiscal revenues by 101.2 euros per capita per year in resisting municipalities, an effect significant at the 1-percent level. This effect corresponds to a 14.5-percent yearly increase and is of similar magnitude for rural and urban municipalities (15.1 and 13.3 percent, Columns 2 and 3). Moreover, the increase in fiscal revenues can be interpreted as an increase per current capita, given that the magnitude is similar when dividing by the number of households in year t and when controlling for municipalities' size (Appendix Table A9). I further show that state transfers and tax revenues contribute equally to the overall impact, as they increased in similar proportion compared to the pre-treatment means (Appendix Figure A14 and Table A10).

Turning to municipalities that decided to join an IC voluntarily prior to the law, Appendix Figure A15 (Panel A) shows that their fiscal revenues increased by about 80 to 100 euros per capita per year after their integration, or 14.1 to 17.6 percent per year.[43] As for municipalities forced to integrate, the increase comes from both additional state transfers and higher fiscal revenues (Appendix Figure A15, Panels B and C). The impact is thus similar for resisting municipalities and for those that voluntarily integrated.

Figure 5: Impact on public transport and fiscal revenues



Notes: These graphs plots the estimates and 95-percent-confidence intervals from the leads-and-lags regression (see Equation (2)). On the left-hand side graph, the outcome is an indicator variable equal to 1 if the municipality has access to public transport and I exclude municipalities in the Parisian region of Île-de-France, for which the data are not available. On the right-hand side graph, the outcome is the municipality's total fiscal revenues per capita and I exclude from the analysis the few municipalities for which revenues data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample).

[43] Data on municipalities' and IC's fiscal revenues are available only starting in 2002. I therefore focus the analysis on municipalities that voluntarily integrated between 2003 and 2010. For the regular staggered adoption design, the period of analysis goes from 2002 to 2018, while in using de Chaisemartin and D'Haultfoeuille (2020)'s method the period of analysis goes from 2002 to 2009 (see Section 5.2). To compute the magnitude of the effect, I compare the increase of 80 and 100 euros to the average fiscal revenues in 2002 for municipalities not already integrated.

Table 5: Impact on public transport and fiscal revenues

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Access to public transport			Fiscal revenues per capita		
	All	Rural	Urban	All	Rural	Urban
Treatment	0.032***	0.020***	0.106***	101.2***	93.1***	134.7***
	(0.007)	(0.006)	(0.024)	(4.8)	(5.1)	(12.2)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221,368	173,894	47,474	244,965	190,965	54,000
Mean DepVar	0.024	0.013	0.078	698.6	617.7	1,015
Sd DepVar	0.152	0.115	0.269	430.0	329.1	598.6

Notes: In columns 1-3, the outcome is an indicator variable equal to 1 if the municipality has access to public transport and I exclude municipalities in the Parisian region of Île-de-France, for which the data are not available. In columns 4-6, the outcome is the municipality's total fiscal revenues per capita and I exclude from the analysis the few municipalities for which revenues data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample). Columns 2 and 5 includes only rural municipalities, while Column 3 and 6 includes only urban municipalities. Other notes as in Table 1.

8 Interpretation

While municipalities forced to integrate experienced benefits of integration similar to those of municipalities that voluntarily integrated, they faced an increase in construction and a loss of local public services that the other municipalities did not experience, at least regarding construction. Following the conceptual framework outline in Section 2.3, I interpret those results as evidence that municipalities resisted integration to avoid those very costs.

This interpretation assumes that municipalities correctly anticipated the consequences of integration and that they would have experienced the same costs had they integrated earlier. Alternatively, one could argue that resisting municipalities faced such effects *because* they integrated later on and were forced to do so. They might have entered ICs better organized and more able to impose costs on them. Moreover, already-integrated municipalities might have decided to punish them for having resisted so long. This would imply that resisting municipalities would not have experienced the same costs had they voluntarily integrated earlier, casting doubt on whether the impacts observed can explain their resistance.

If this alternative interpretation is correct, we should see that the costs of integration are borne by treated municipalities that entered existing ICs after the law, as opposed to treated municipalities that created new ICs after 2010. Similarly, when joining existing communities, we would expect

the costs to be lower for those that joined more recently created ICs. Finally, if municipalities are punished for not having integrated the community earlier, we would expect the impact to be lower for resisting municipalities that shared borders with several ICs at the time of the law, as it would be less clear which IC they were avoiding. I assess the impact of integration in each of these situations, focusing on municipalities that are part of an urban area with respect to building permits and focusing on rural municipalities with respect to daycare and public libraries.

As shown in Appendix Table A11, compared to the effect for the full sample (10.5, Column 1), the impact on construction remains large and significant at the 1 percent level for treated municipalities that joined a new IC (8.8, Column 1), joined a recently created one (15.6, Column 4), or had a choice of at least two ICs at the time of the law (6.9, Column 5). Turning to public services (Appendix Table A12), the impact for rural municipalities remains negative across all three cases and only loses statistical significance for recent IC (resp. new IC) for daycare services (resp. libraries).

When entering a new or a recently created IC, the integration process of resisting municipalities resembles that of municipalities that integrated early on. Finding similar patterns in such cases supports the view that municipalities that opposed integration would have experienced the same had they integrated earlier and thus that the consequences they faced help explain their resistance.

9 Conclusion

This paper provides new evidence on the factors explaining municipalities' opposition to consolidation. Exploiting a 2010 reform in France that forced non-integrated municipalities to enter an intermunicipal community, I measure the causal impact of integration on resisting municipalities. Comparing what they faced to the experience of municipalities that had chosen to integrate prior to the law, I provide evidence that resistance is driven by the local costs of integration, beyond ideological or political considerations.

Using a difference-in-differences strategy, I first find that municipalities forced to integrate experienced an increase of 12.5 percent in the number of building permits delivered per year. On the contrary, municipalities that joined an IC voluntarily had no such significant increase in their housing supply after integration. Only municipalities that did not want to integrate faced a large increase in construction, supporting the view that they refused to integrate to avoid an increase in housing supply. Further heterogeneity analyses show that the impact is mainly driven by high-demand and densely built municipalities, consistent with ICs imposing new construction in municipalities that were using local regulations to avoid further increases in density. The effect is also larger for resisting municipalities that lost the most bargaining power, showing that consolidation is particularly effective at overcoming local regulatory capture when the places restricting housing development

have a relatively low bargaining power inside their new jurisdiction.

I then assess the impact of integration on local public services. I gathered data at the local level on two different public services transferred to the community level after integration: daycare and public libraries. I find that rural municipalities forced to enter an IC ended up with 20 to 30 percent fewer daycare spots and public libraries after the law, implying that integration increased the distance to public services for their residents. These results show that different types of municipalities face different costs of integration: while high-demand locations face an unwanted rise in density, low-density places face a loss of local public services.

Finally, I explore the impact of integration on two additional aspects of consolidation that should mainly entail benefits. I find that municipalities that were forced to enter an intermunicipal community became twice as likely to have access to public transport and experienced a 14.5-percent-per-year increase in the fiscal resources available per resident. Municipalities that voluntarily integrated experienced very similar gains. Hence, while resisting municipalities experienced costs other municipalities did not face, they enjoyed similar benefits, suggesting that they did not resist due to lower benefits, but to avoid the costs of integration.

All together, these results suggest that municipalities resisted integration knowing that they would not be able to prevent their neighbors from imposing new construction or decreasing the availability of public services on their territory. These results help explain why the vast majority of French municipalities recently opposed new laws aimed at increasing the size of intermunicipal communities. Such reforms would lead many municipalities to lose bargaining power and thus to suffer from costs they have been able to avoid so far.[44] Moreover, as most forms of cooperation among local jurisdictions imply sharing urban planning policies and public services, these findings can help explain resistance to integration beyond the case of French municipalities, as long as the decision process involves some jurisdictions losing more power than others.

By identifying the local consequences of integration, this paper opens the avenue to a comprehensive welfare analysis of consolidation reforms that would take both sides of the tradeoff into account. To assess the overall welfare effect of integration policies, one would need to compare the aggregate benefits coming from the rationalization of public services and from the increase in construction to the local costs borne by the residents of the low-density municipalities losing public services and by the residents of the high-demand municipalities facing increases in density.

[44] In 2015, a new law passed requiring that ICs should have at least 15,000 inhabitants by 2017 and requiring municipalities to share more public services. There was widespread complaint from mayors, leading to multiple revisions of the law with additional room for exceptions (e.g., AMF-Cevipof/SciencesPo 2018). In the face of such resistance, the French president announced he might reconsider the law (see for instance: https://www.amf.asso.fr/documents-vers-une-revision-la-loi-notre-/39240).

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Appendix to "Better Alone? Evidence on the Costs of Intermunicipal Cooperation"

ONLINE APPENDIX 1

Appendix A. Additional figures and tables 1

ADDITIONAL SUPPLEMENT - not intended for publication 1

Appendix B. Supplementary tables 1

Appendix C. Additional robustness checks 16

Appendix D. Housing supply: Heterogeneity analysis 34

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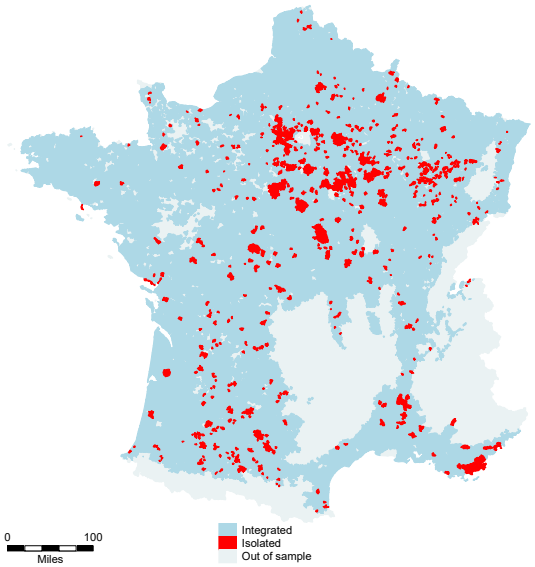
Online Appendix

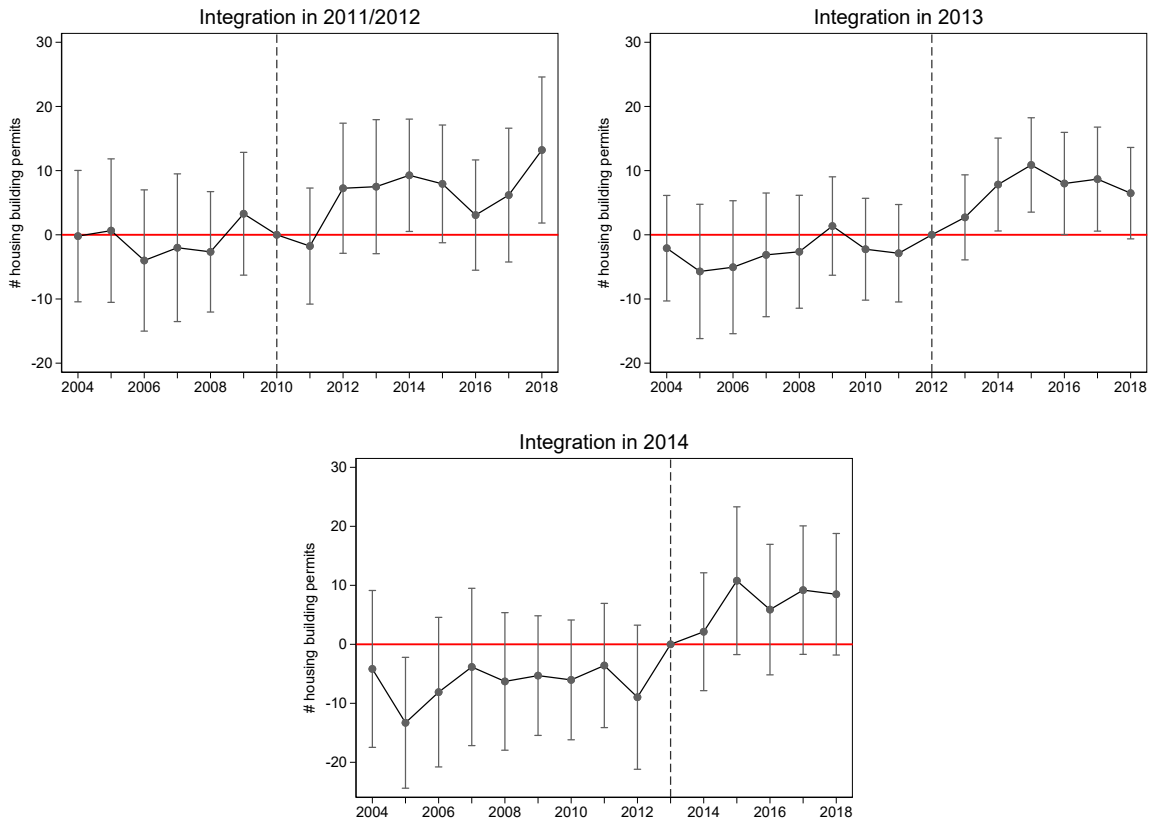
Appendix A. Additional figures and tables

Figure A1: French municipalities depending on their integration status in 2010



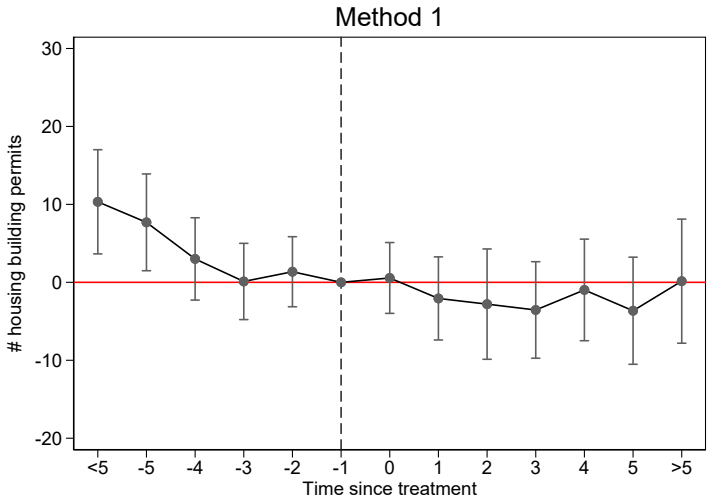
Notes: Municipalities in red were not part of an intermunicipal community in 2010. Municipalities in blue were already integrated. Grey areas represent municipalities excluded from the sample of analysis, as explained in Section 4.1.

Figure A2: Impact on housing depending on the exact year of integration



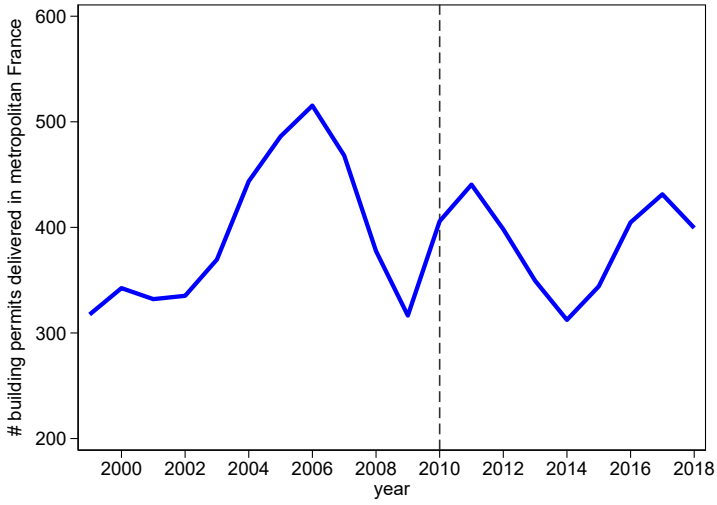
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression, using as outcome the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The first graph includes only treated municipalities that entered an IC in 2011 or 2012. The second (resp. third) graph includes only treated municipalities that entered an IC in 2013 (resp. 2014).

Figure A3: Impact on housing for municipalities that voluntarily integrated between 2004 and 2010



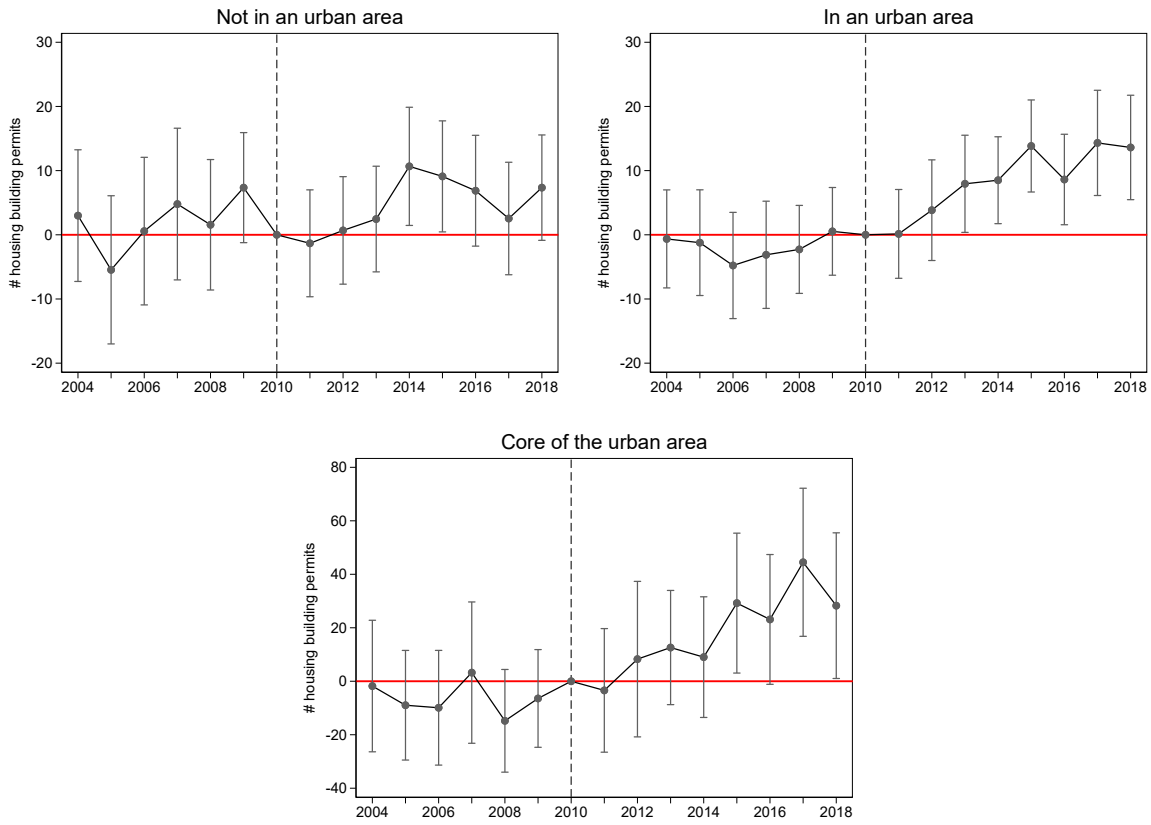
Notes: The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample is made up of municipalities that voluntarily integrated between 2004 and 2010. The graph plots the estimates and 95-percent-confidence intervals from a regular staggered adoption design (method 1). The period of analysis goes from 1999 to 2018. More information in Section 5.2.

Figure A4: Number of building permits delivered over time



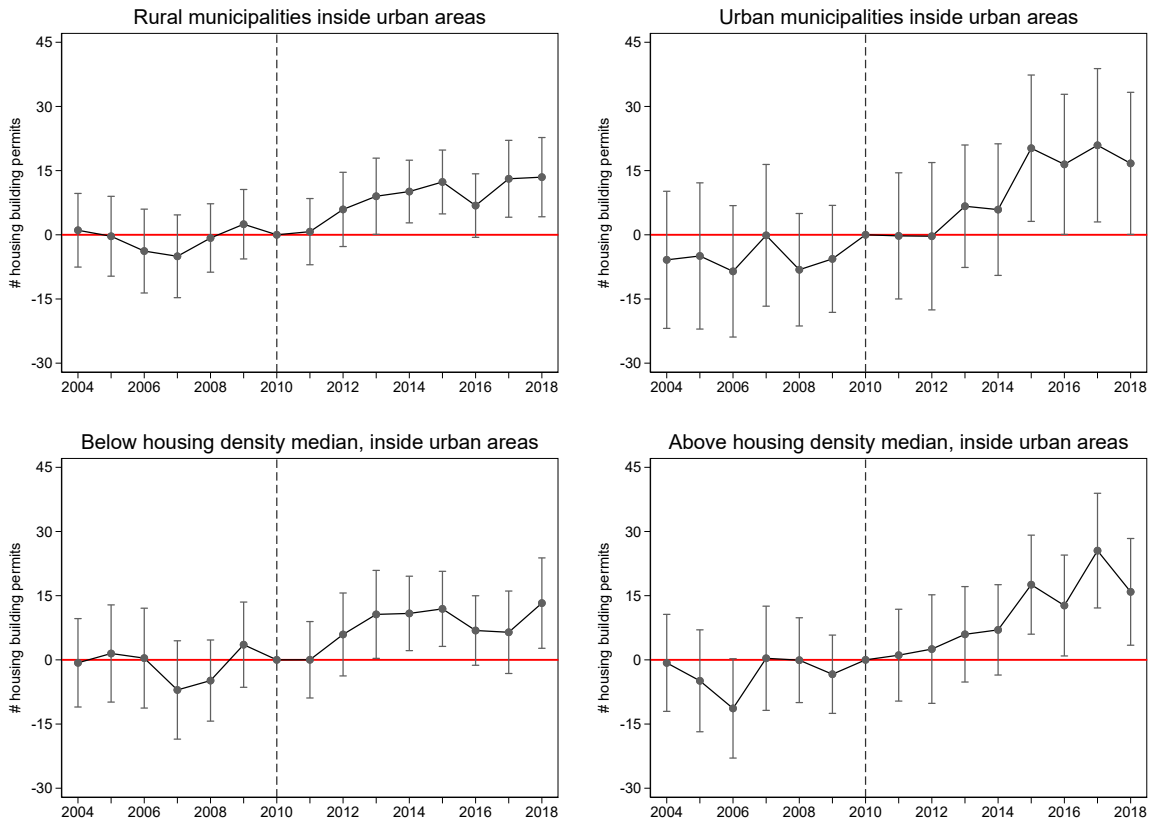
Notes: This figure plots the total number of building permits delivered in metropolitan France from 1999 to 2018, expressed in thousands.

Figure A5: Impact on housing: Urban area



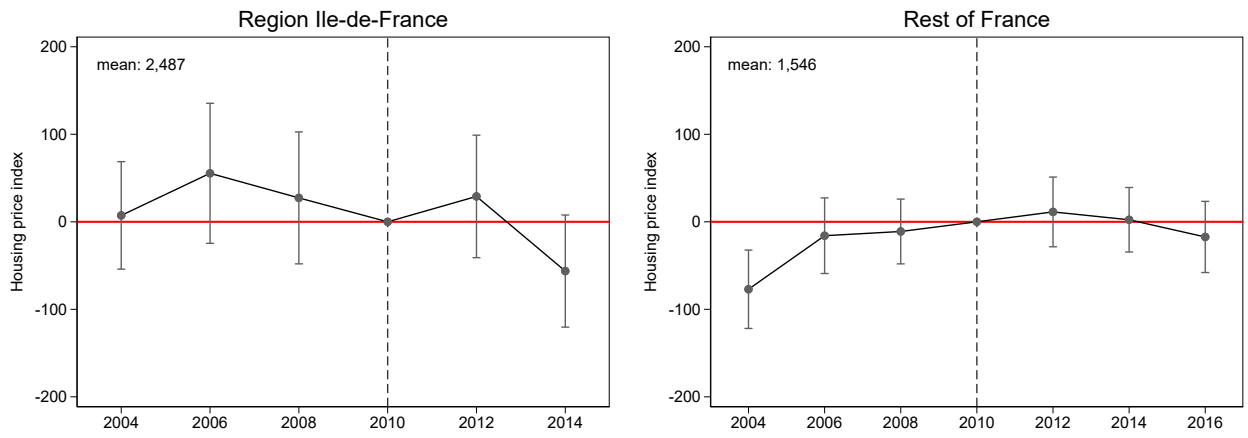
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression, using as outcome the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population) The first graph includes only municipalities that are not part of an urban area. The second graph includes only municipalities that are part of an urban area. The third graph includes only municipalities in the core of the urban area (using a different scale given the magnitude of the results).

Figure A6: Impact on housing inside urban areas: Housing density



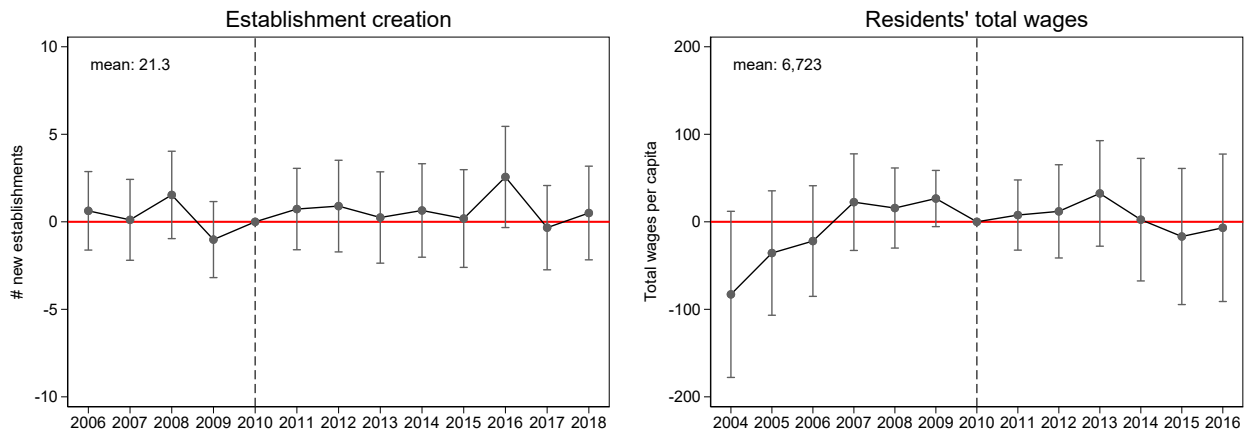
Notes: All graphs focus on municipalities inside an urban area. These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression, using as outcome the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The first (resp. second) graph includes only rural (resp. urban) municipalities. The third (resp. fourth) graph includes only municipalities with a housing density in 2010 below (resp. above) the median.

Figure A7: Impact on housing prices



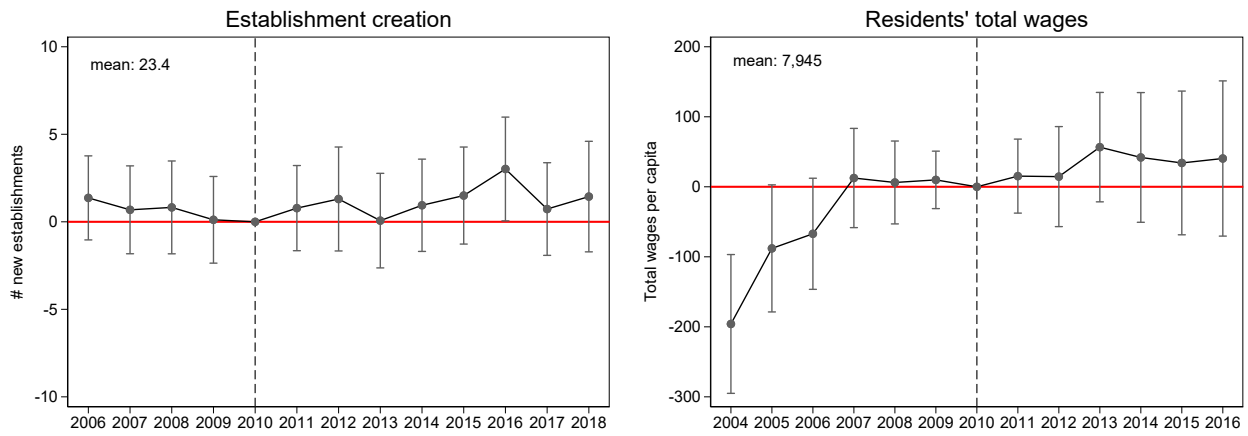
Notes: This graph plots the estimates and 95-percent-confidence intervals from the leads-and-lags regression (see Equation (2)). The outcome is the municipality housing price index giving the price per square meter of a reference dwelling. The sample includes only municipalities in which at least one housing transaction took place each even year over the period studied. The graph on the left-hand side includes only municipalities in the Parisian region of Île-de-France, while the graph on the right-hand side includes all the other municipalities. On each graph, the average price per square meter in the treatment group before 2010 is displayed on the top left corner.

Figure A8: Impact on economic activity



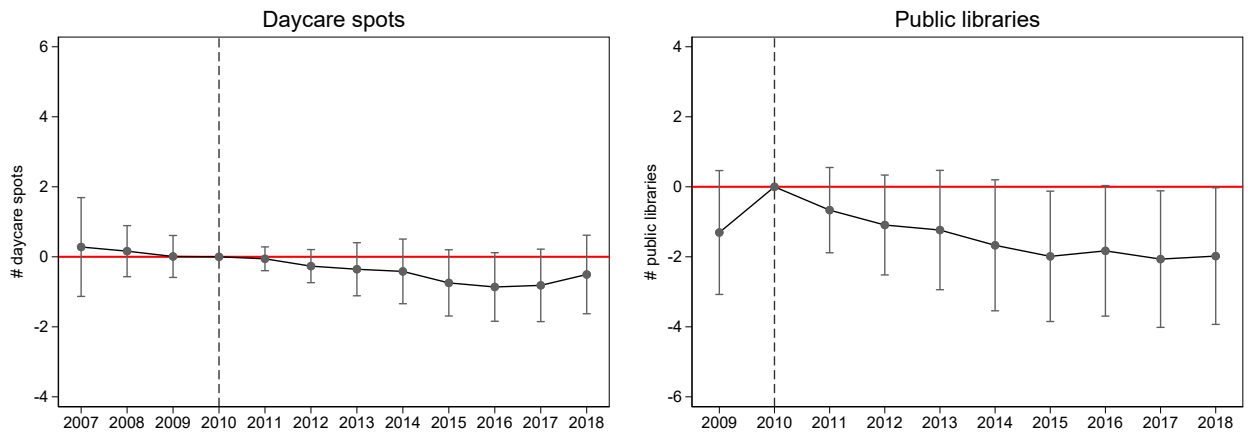
Notes: These graphs plots the estimates and 95-percent-confidence intervals from the leads-and-lags regression (see Equation (2)). On the left-hand graph, the outcome is the number of establishments created in a given year, per 10,000 inhabitants (using the 2010 population). The agricultural sector and the establishments created by individual entrepreneurs are excluded. On the right-hand graph, the outcome is the yearly total wages received by residents, divided by the 2010 population. The total wage computation includes only full-time employed residents. It excludes self-employed workers as well as the agricultural and public sectors. It is missing for the 311 smallest municipalities (2 percent of the sample). On both graphs, the average value of the outcome in the treatment group before 2010 is displayed on the top left corner.

Figure A9: Impact on economic activity: Urban area



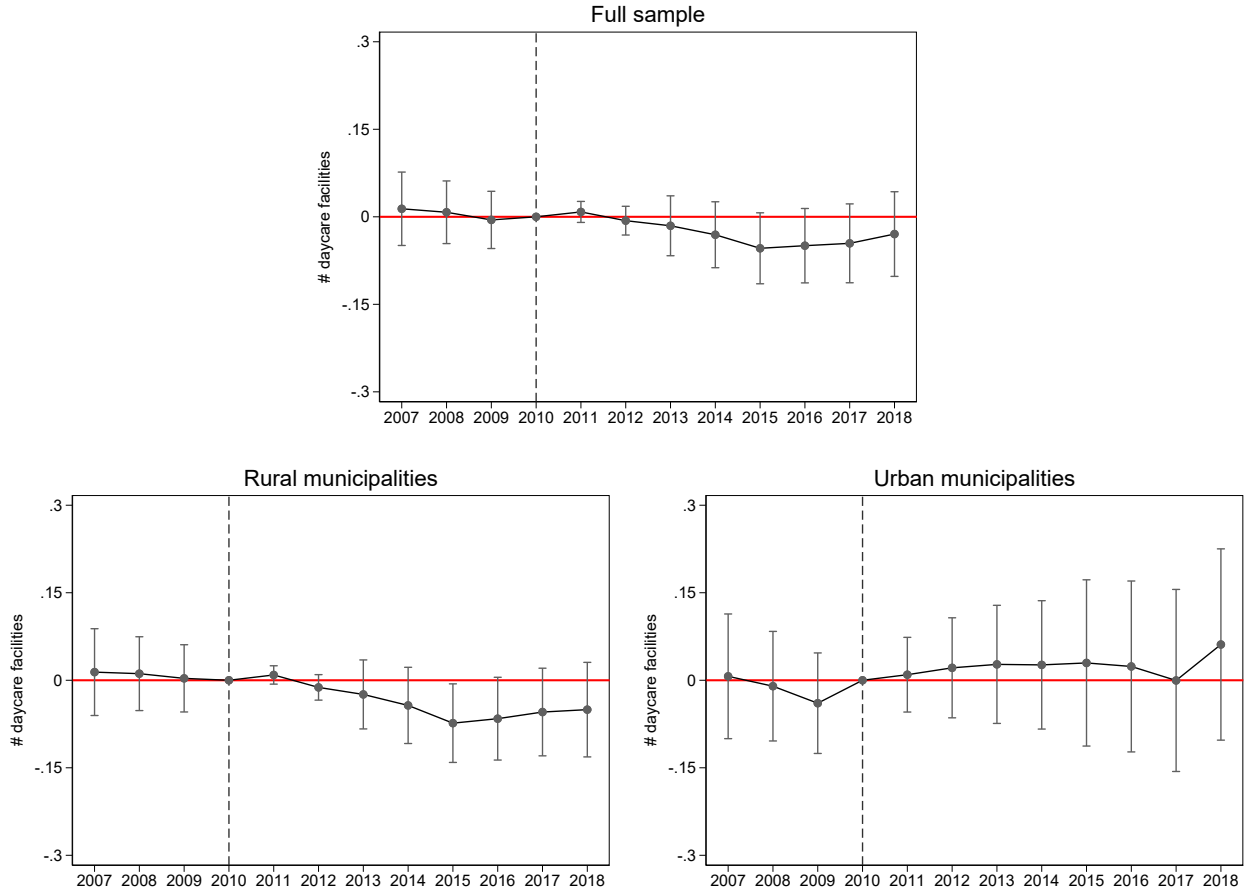
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The two graphs focus on municipalities inside an urban area. On the left-hand graph, the outcome is the number of establishments created in a given year, per 10,000 inhabitants (using the 2010 population). The agricultural sector and establishments created by individual entrepreneurs are excluded. On the right-hand graph, the outcome is the yearly total wages received by residents, divided by the 2010 population. The total wage computation includes only full-time employed residents. It excludes self-employed workers as well as the agricultural and public sectors. It is missing for the 311 smallest municipalities (2 percent). On both graphs, the average value of the outcome in the treatment group before 2010 is displayed on the top left corner.

Figure A10: Impact on public services: Full sample



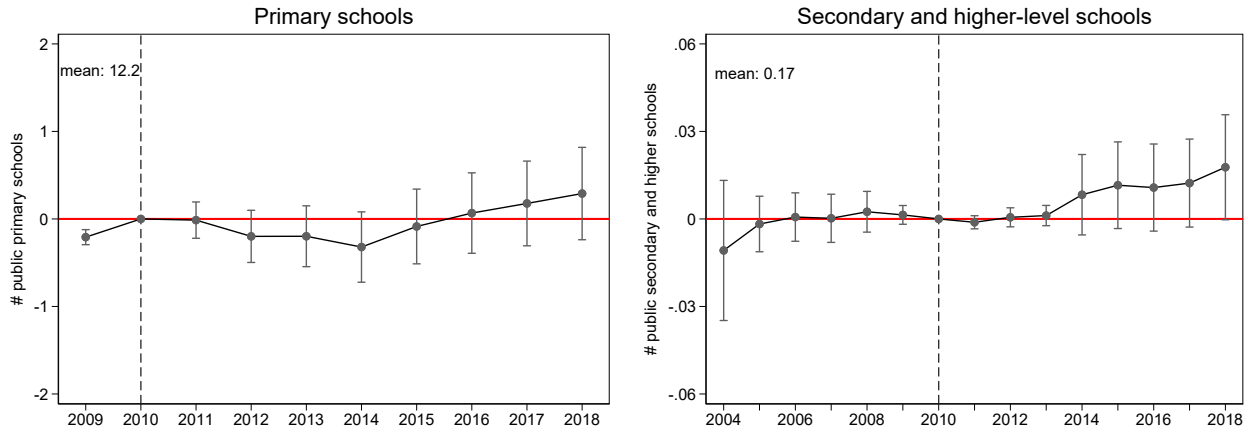
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. In the left-hand graph, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population). In the right-hand graph, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), and the sample is restricted to the 7 départements for which data are available starting in 2009.

Figure A11: Impact on the number of daycare facilities



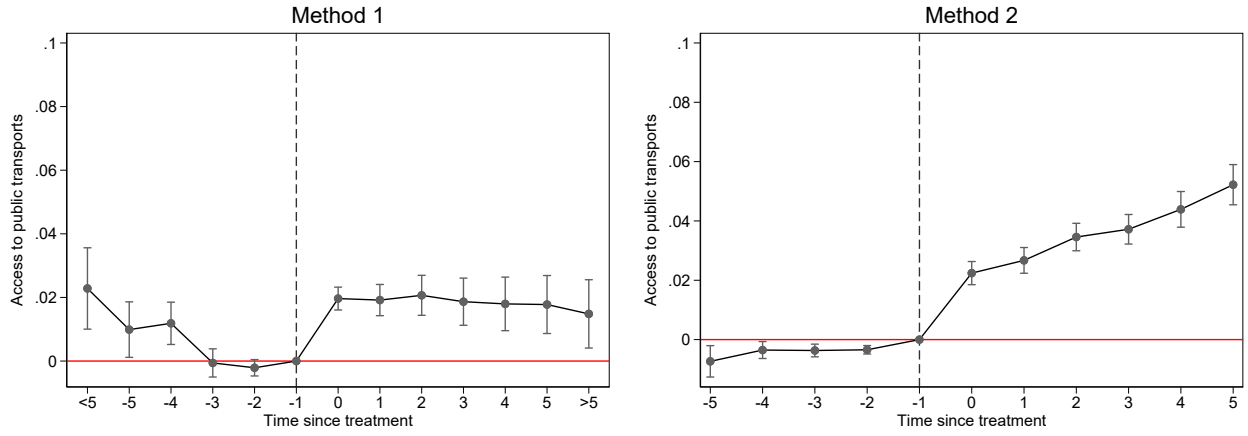
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The outcome is the number of daycare facilities in the municipality per 10,000 inhabitants (using the 2010 population). The graph on the bottom left-hand side includes only rural municipalities, while the graph on the bottom right-hand side includes only urban municipalities.

Figure A12: Impact on public schools: Rural municipalities



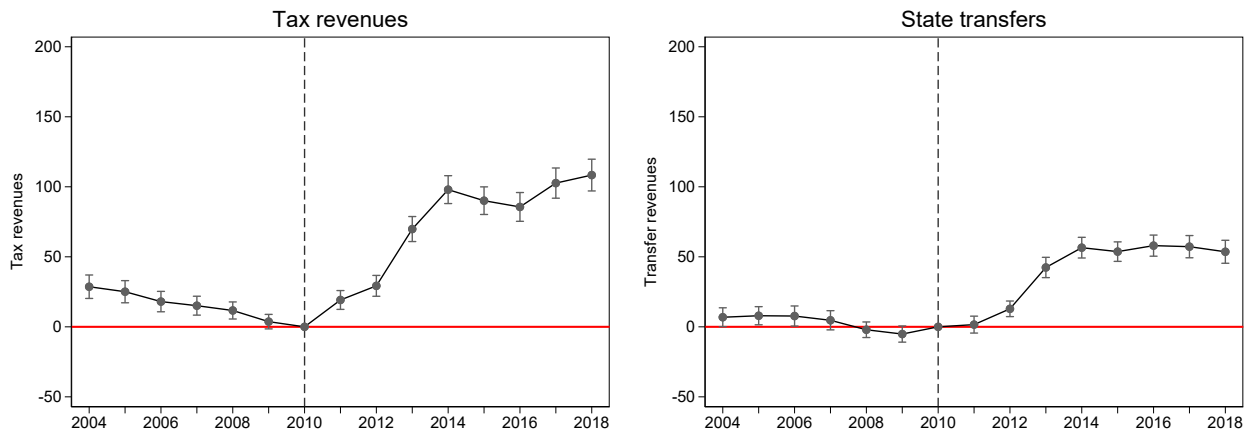
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The sample includes only rural municipalities. On the left-hand graph, the outcome is the number of public preschools and primary schools in the municipality, per 10,000 inhabitants (using the 2010 population). On the right-hand graph, the outcome is the number of secondary schools, high schools, and universities in the municipality, per 10,000 inhabitants (using the 2010 population). It includes both fully public schools and private but publicly subsidized schools. On both graphs, the average value of the outcome in the treatment group before 2010 is displayed on the top left corner.

Figure A13: Comparison with municipalities that voluntarily integrated: Transport



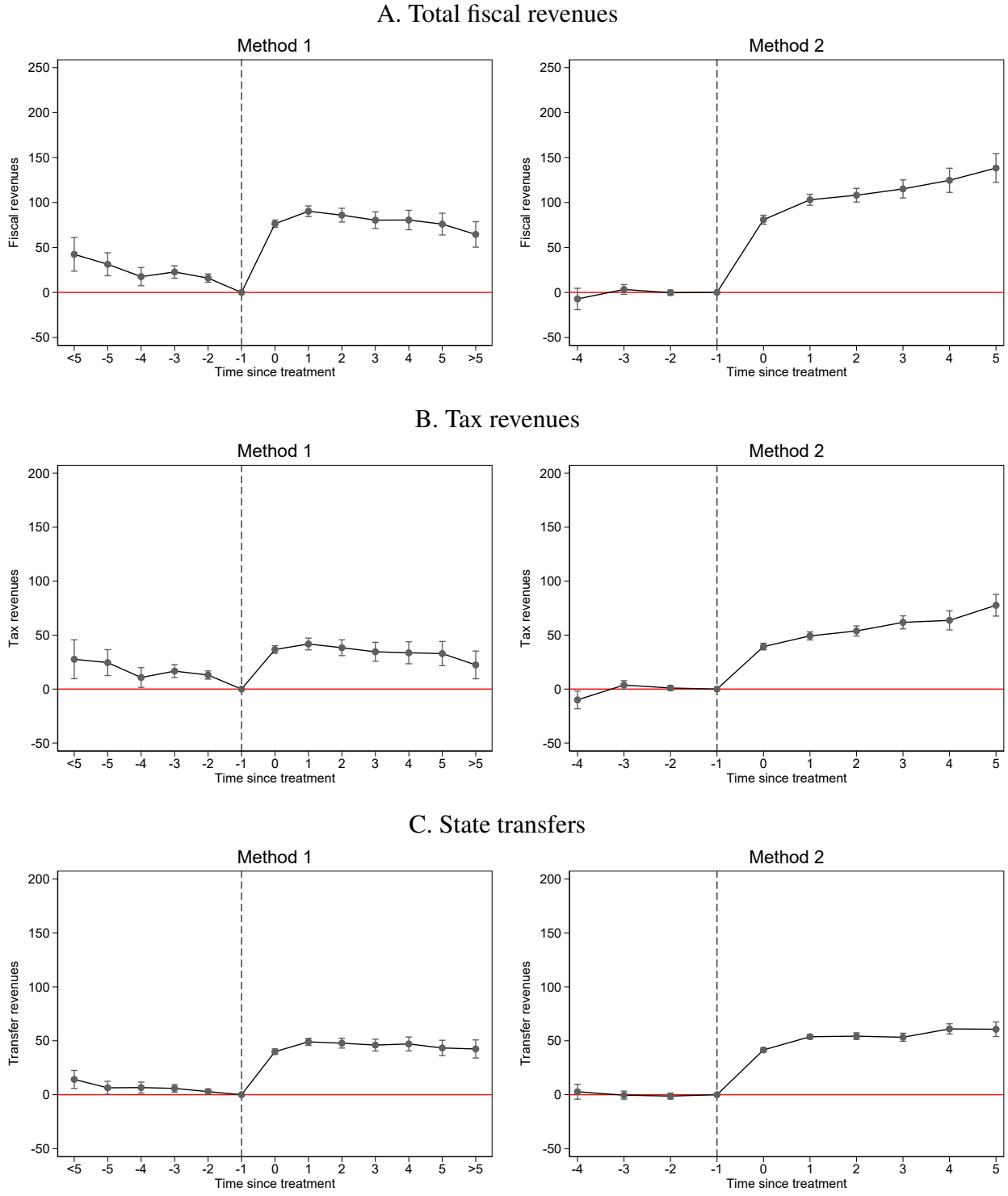
Notes: The outcome is an indicator variable equal to 1 if the municipality has access to public transport. The sample includes only municipalities that voluntarily integrated between 2000 and 2010 and excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. The left-hand graph plot the estimates and 95-percent-confidence intervals from a regular staggered adoption design (method 1). The right-hand graph uses de Chaisemartin and D'Haultfoeuille (2020)'s method, implemented using the Stata command did_multiplegt, available on SSC repository (method 2). In method 1, the period of analysis goes from 1999 to 2017, whereas, in method 2, it goes from 1999 to 2009. More information on the two methods in Section 5.2.

Figure A14: Impact on tax revenues and state transfers



Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. On the left-hand (resp. right-hand) side graph, the outcome is the municipality's tax revenues (resp. state transfers) per capita (using the 2010 population). The outcome construction is described in Section 7.2. I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample).

Figure A15: Comparison with municipalities that voluntarily integrated: Fiscal revenues



Notes: The sample is made up of municipalities that voluntarily integrated between 2000 and 2010. The outcome in panel A (resp. B, C) is the municipality's total fiscal revenue (resp. tax revenue, state transfers) per capita, as described in Section 7.2. Data on fiscal revenues are only available starting in 2002. The analysis is thus restricted to municipalities integrating between 2003 and 2010. I exclude from the analysis a few municipalities for which the data are missing for at least one year over the period 2002–2018 (3.5 percent of the sample). Left-hand graphs plot the estimates and 95-percent-confidence intervals from a regular staggered adoption design (method 1). Right-hand graphs use de Chaisemartin and D'Haultfoeuille (2020)'s method (method 2). In method 1, the period of analysis goes from 2002 to 2018, whereas, in method 2, it goes from 2002 to 2009. The second method goes only back to year -4 for fiscal outcomes. To compute the impact in year -5, we would need to observe some municipalities both 4 years before and after treatment, which is not possible given the lack of data prior to 2002.

Table A1: Descriptive statistics – 2010 (Panel A)

	Resisting municipalities (N=1,299)				Integrated municipalities (N=26,991)			
	mean	sd	min	max	**mean**	sd	min	max
Panel A	*Socio-demographic characteristics*							
population	**1,640**	4,692	16	72,939	**1,711**	9,606	3	851,420
Δ population	**0.102**	0.160	-0.397	1.385	**0.100**	0.152	-0.500	2.692
density	**162.1**	541.6	2.11	6,884	**140.9**	422.4	0.161	9,976
urban	**0.204**	0.403	0.000	1.000	**0.209**	0.406	0.000	1.000
urban area	**0.637**	0.481	0.000	1.000	**0.610**	0.488	0.000	1.000
core urban area	**0.091**	0.287	0.000	1.000	**0.108**	0.310	0.000	1.000
immigrants	**0.044**	0.040	0.000	0.333	**0.038**	0.039	0.000	0.667
unemployed	**0.082**	0.039	0.000	0.571	**0.087**	0.040	0.000	0.429
below 5 y/o	**0.060**	0.020	0.000	0.172	**0.061**	0.021	0.000	0.206
above 65 y/o	**0.170**	0.066	0.000	0.522	**0.178**	0.065	0.000	0.667
av. # children	**0.907**	0.245	0.000	2.000	**0.892**	0.228	0.000	3.000
farmers	**0.036**	0.053	0.000	0.444	**0.038**	0.052	0.000	1.000
executives	**0.070**	0.065	0.000	0.429	**0.053**	0.045	0.000	0.563
workers	**0.152**	0.080	0.000	1.000	**0.166**	0.071	0.000	1.000
retired	**0.280**	0.101	0.000	0.800	**0.288**	0.098	0.000	1.000
no diploma	**0.172**	0.069	0.000	0.463	**0.183**	0.067	0.000	0.674
baccalaureate	**0.156**	0.039	0.000	0.324	**0.153**	0.037	0.000	0.557
high education	**0.088**	0.071	0.000	0.507	**0.073**	0.046	0.000	0.542
commuters	**0.758**	0.154	0.000	1.000	**0.758**	0.147	0.000	1.000
homeowners	**0.803**	0.109	0.077	1.000	**0.787**	0.107	0.000	1.000
residents' income	**14,064**	4,362	5,495	59,093	**12,621**	2,908	3,273	65,756

Notes: Socio-demographic variables come from the 2008 census, which applies to the year 2010. The variation in the population (line 2) is computed by comparing the 1999 and 2008 censuses. Indicator variables for whether the municipality is urban, part of an urban area, or in the urban core are based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality.

Table A1 (continued): Descriptive statistics – 2010 (Panels B and C)

	Resisting municipalities (N=1,299)				Integrated municipalities (N=26,991)			
	mean	sd	min	max	**mean**	sd	min	max
Panel B	*Land-use and housing price characteristics*							
share built land (%)	**0.742**	1.416	0.024	15.882	**0.725**	1.330	0.004	25.987
average height	**1.561**	0.216	1.048	4.727	**1.560**	0.187	1.000	4.391
FAR (p30)	**0.110**	0.076	0.001	0.901	**0.106**	0.078	0.000	1.465
FAR (p50)	**0.171**	0.116	0.003	1.812	**0.169**	0.118	0.003	1.901
housing price/income	**0.116**	0.043	0.012	0.410	**0.112**	0.037	0.013	0.634
Panel C	*Political characteristics*							
turnout municipal	**0.763**	0.096	0.431	1.000	**0.779**	0.090	0.367	1.000
turnout presidential	**0.874**	0.040	0.655	1.000	**0.873**	0.040	0.000	1.000
voteshare right	**0.602**	0.107	0.152	1.000	**0.563**	0.105	0.106	1.000
voteshare far-right	**0.138**	0.057	0.000	0.467	**0.130**	0.054	0.000	0.556
right-wing mayor	**0.620**	0.486	0.000	1.000	**0.559**	0.496	0.000	1.000
left-wing mayor	**0.267**	0.443	0.000	1.000	**0.337**	0.473	0.000	1.000
NC mayor	**0.112**	0.316	0.000	1.000	**0.103**	0.304	0.000	1.000
woman mayor	**0.161**	0.368	0.000	1.000	**0.142**	0.349	0.000	1.000
age mayor	**57.1**	9.1	25.0	87.0	**56.1**	8.7	18.0	88.0
incumbent mayor	**0.661**	0.474	0.000	1.000	**0.624**	0.484	0.000	1.000
change orientations	**0.210**	0.407	0.000	1.000	**0.221**	0.415	0.000	1.000

Notes: Panel B: Land-use characteristics come from Combes et al. (2021) and each variable is built considering only construction before 2010. The share of built land is computed considering all construction, whereas the average height and floor-to-area ratio (FAR) are computed considering only housing construction. The average height gives the average number of housing stories. To measure the FAR stringency, I follow Combes et al. (2021) and take the 30th percentile of the distribution of realized floor-to-area ratios of all housing buildings in the municipality (FAR p(30)). I also show the statistics using the median (FAR p(50)). The last variable of Panel B measures the ratio of the housing price index over the average income per capita in 2010. To measure housing prices, I built indices based on official housing transactions, which can be interpreted as the price per square meter of a reference dwelling (see Appendix E). The data on housing prices are restricted to municipalities in which at least one transaction took place every even year over the period from 2000 to 2010.
Panel C: The municipal turnout rate and mayor's characteristics are based on the results of the 2008 municipal elections. The presidential turnout rate and far-right vote share come from the results of the first round of the 2007 presidential elections. The right-wing vote share comes from the results of the second round of the 2007 presidential elections. NC stands for "non classified".

Table A2: Impact on housing: Dividing by current population and adding controls

	(1)	(2)	(3)	(4)
Outcome	Number of housing building permits			
	2010 population		Current population	
Treatment	8.141***	7.702***	15.980***	15.617***
	(1.517)	(1.521)	(2.878)	(2.883)
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	245,940	228,326	228,858	228,326
Mean DepVar	64.836	65.006	121.936	122.265
Sd DepVar	90.844	89.454	175.032	175.094

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants. In Columns 1 and 2 (resp. 3 and 4), the outcome is normalized by dividing by the 2010 population (resp. by the number of households in the municipality at year t). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) and should be used to compare the magnitude of the effect across the two measures. Controls included in Columns 2 and 4 are the number of households in the municipality and the average household's annual taxable income in year t. When using the number of households (resp. household's annual taxable income), the sample is restricted to a balanced panel of municipalities for which fiscal data are not missing during the period of analysis, excluding 49 (resp. 87) small municipalities. Household tax declaration data are available up to 2017 so Columns 2, 3, and 4 exclude the year 2018 from the analysis.

Table A3: Impact on housing: Residents' income

	(1)	(2)	(3)
Outcome	Number of housing building permits		
	All	Residents' income median	
		Below	Above
Treatment	8.141***	4.518**	8.114***
	(1.517)	(2.173)	(2.024)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	245,940	122,850	122,850
Mean DepVar	64.836	66.216	81.021
Sd DepVar	90.844	87.332	102.097

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 2 (resp. 3) includes only municipalities below (resp. above) the median of the per capita residents' annual taxable income in 2010. Data on taxable income in 2010 are missing for the 16 smallest municipalities.

Table A4: Impact on housing inside urban areas: Housing density

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of housing building permits per 10,000 inhabitants				
	All	Rural	Urban	Housing density median	
				Below	Above
Treatment	10.494***	9.852***	15.538***	9.258***	13.895***
	(1.812)	(1.967)	(3.755)	(2.230)	(2.759)
Mun. FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Obs.	151,500	105,345	46,155	75,750	75,750
Mean	63.660	62.963	65.558	64.714	62.425
Sd	85.197	82.124	93.059	85.671	84.638

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample includes only municipalities inside an urban area. Columns 2 to 5 add further restrictions: Column 2 (resp. 3) includes only rural (resp. urban) municipalities and Column 4 (resp. 5) includes only municipalities with a housing density in 2010 below (resp. above) the median.

Table A5: Impact on housing: Share of homeowners

Outcome	(1)	(2)	(3)
	Number of housing building permits		
	All	Median % homeowners	
		Below	Above
Treatment	8.141***	9.263***	7.650***
	(1.517)	(2.645)	(1.798)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	245,940	122,985	122,955
Mean DepVar	64.836	70.803	60.789
Sd DepVar	90.844	97.331	85.940

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 4 (resp. 5) includes only municipalities below (resp. above) the median value of the share of homeowners in 2010.

Table A6: Impact on the number of daycare facilities

Outcome	(1)	(2)	(3)
	Number of daycare facilities		
	All	Rural	Urban
Treatment	-0.032	-0.046	0.036
	(0.030)	(0.034)	(0.060)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	196,752	153,216	43,536
Mean DepVar	0.402	0.169	1.310
Sd DepVar	1.453	1.267	1.749

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of daycare facilities in the municipality per 10,000 inhabitants (using the 2010 population), and the period of analysis is 2007–2018. Column 2 (resp.3) include only rural (resp. urban) municipalities.

Table A7: Impact on public services (rural municipalities): Dividing by current population and adding controls

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Daycare spots				Public Libraries			
	2010 population		Current population		2010 population		Current population	
Treatment	-1.001**	-0.946**	-1.854**	-1.824**	-1.248**	-1.180**	-2.089*	-2.107*
	(0.463)	(0.451)	(0.760)	(0.762)	(0.620)	(0.600)	(1.072)	(1.086)
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	153,216	139,502	139,909	139,502	11,020	9,819	9,864	9,819
Mean	3.617	3.673	6.991	7.025	4.374	5.342	9.657	9.775
Sd	31.900	32.147	63.000	63.152	9.574	11.034	20.282	20.377

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The sample includes only rural municipalities. In Columns 1 to 3, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Columns 4 to 6, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), the period of analysis is 2009–2018, and the sample is restricted to the 7 départements for which data are available starting in 2009. In Columns 1, 2, 5, and 6 (resp. 3, 4, 7, and 8) the outcome is normalized by dividing by the 2010 population (resp. by the number of households in the municipality at year t). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) and should be used to compare the magnitude of the effect across the two measures. Controls included in Columns 2, 4, 6, and 8 are the number of households in the municipality and the average household's annual taxable income in year t. When using the number of households (resp. household's annual taxable income), the sample is restricted to a balanced panel of municipalities for which fiscal data are not missing during the period of analysis, excluding 49 (resp. 87) small municipalities. Household tax declaration data are available up to 2017, so all columns except Columns 1 and 5 exclude the year 2018 from the analysis.

Table A8: Impact on public transport: Adding controls

	(1)	(2)
Outcome	Public transport	
Treatment	0.032***	0.033***
	(0.007)	(0.007)
Municipality FE	Yes	Yes
Time FE	Yes	Yes
Controls	No	Yes
Observations	221,368	220,178
Mean DepVar	0.024	0.024
Sd DepVar	0.152	0.153

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is an indicator variable equal to 1 if the municipality has access to public transport. The sample excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. Controls included in Column 2 are the number of households in the municipality and the average household's annual taxable income in year t. In Column 2, the sample is restricted to a balanced panel of municipalities for which fiscal data are not missing during the period of analysis, excluding 49 (resp. 87) small municipalities.

Table A9: Impact on fiscal revenues: Dividing by current population and adding controls

	(1)	(2)	(3)	(4)
Outcome	Fiscal revenues per capita			
	2010 population		Current population	
Treatment	101.2***	93.8***	186.2***	180.6***
	(4.813)	(4.674)	(8.296)	(8.041)
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	244,965	227,416	227,948	227,416
Mean DepVar	698.6	694.9	1,282	1,279
Sd DepVar	430.0	423.7	731.5	721.4

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The outcome is the municipality's total fiscal revenues per capita, as described in Section 6.2. I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample). In Columns 1 and 2 (resp. 3 and 4) the outcome is normalized by dividing by the 2010 population (resp. by the number of households in the municipality at year t). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) and should be used to compare the magnitude of the effect across the two measures. Controls included in Columns 2 and 4 are the number of households in the municipality and the average household's annual taxable income in year t. When using the number of households (resp. household's annual taxable income), the sample is restricted to a balanced panel of municipalities for which fiscal data are not missing during the period of analysis, excluding 49 (resp. 87) small municipalities. Household tax declaration data are available up to 2018 so Columns 2, 3, and 4 exclude the year 2018 from the analysis.

Table A10: Impact on tax revenues and state transfers

Outcome	(1)	(2)	(3)
	Fiscal revenues		
	All	Tax	Transfer
Treatment	101.2***	60.8***	39.1***
	(4.813)	(4.929)	(2.410)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	244,965	244,965	244,965
Mean DepVar	698.6	424.4	274.2
Sd DepVar	430.0	349.2	150.0

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). In Column 1, the outcome is the municipality's total fiscal revenues per capita. In Column 2 (resp. 3), the outcome is the municipality's tax revenues (resp. state transfers), per capita. The construction of the outcomes is described in Section 7.2. I exclude from the analysis few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample).

Table A11: Impact on housing depending on the integration process: Urban areas

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of building permits per 10,000 inhabitants				
	All	New IC	Existing IC		
			All	Recent	≥ 2 choices
Treatment	10.494***	8.751***	11.298***	15.634***	6.879***
	(1.812)	(3.207)	(2.116)	(3.472)	(2.549)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Observations	151,500	143,010	147,585	142,125	144,585
Mean DepVar	63.660	63.138	63.900	58.509	67.357
Sd DepVar	85.197	83.922	85.788	85.666	86.968

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample includes only municipalities part of an urban area. Columns 2 (resp. 3) includes only treated municipalities that created a new IC (resp. that joined an existing IC) after the 2010 law. Column 4 includes only treated municipalities that entered a recently created IC (in which all other members integrated after 2000). Column 5 includes only treated municipalities that entered an existing IC and that had the choice between at least two of them in 2010.

Table A12: Impact on public services depending on the integration process: Rural municipalities

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Daycare spots					Public libraries				
	All	New IC	Existing IC			All	New IC	Existing IC		
			All	Recent	≥ 2 choices			All	Recent	≥ 2 choices
Treatment	-1.001**	-1.265***	-0.908	-0.394	-1.672***	-1.248**	-0.516	-1.349*	-1.627*	-2.273**
	(0.463)	(0.341)	(0.569)	(0.887)	(0.514)	(0.620)	(0.366)	(0.698)	(0.968)	(1.064)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	153,216	144,060	149,964	144,012	146,940	11,020	10,290	10,920	10,630	10,610
Mean	3.617	1.951	4.208	6.976	2.551	5.277	6.600	5.096	5.655	5.588
Sd	31.900	13.115	36.288	52.146	22.776	10.983	5.644	11.524	12.621	12.381

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The sample includes only rural municipalities. In Columns 1 to 5, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Columns 6 to 10, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), the period of analysis is 2009–2018, and the sample is restricted to the 7 départements for which data are available starting in 2009. Columns 2 and 7 (resp. 3 and 8) include only treated municipalities that created a new IC (resp. that joined an existing IC) after the 2010 law. Results on public libraries should be interpreted with caution for this restriction, as it includes only 10 rural treated municipalities. Columns 4 and 9 include only treated municipalities that entered an existing IC which had recently been created (in which all other members integrated after 2000). Columns 5 and 10 include only treated municipalities that entered an existing IC and that had the choice between at least two of them in 2010.

Additional Supplement - not intended for publication

Appendix B. Supplementary tables

Table B1: Descriptive statistics using the control group only – 2010 (Panel A)

	Resisting municipalities (N=1,299)				Control municipalities (N=15,097)			
	mean	sd	min	max	**mean**	sd	min	max
Panel A	*Socio-demographic characteristics*							
population	**1,640**	4,692	16	72,939	**1,959**	11,686	7	851,420
w/out largest	**1,640**	4,692	16	72,939	**1,574**	4,139	7	66,095
Δ population	**0.102**	0.160	-0.397	1.385	**0.100**	0.149	-0.500	2.692
density	**162.1**	541.6	2.11	6,884	**156.3**	450.1	0.60	9,976
urban	**0.204**	0.403	0.000	1.000	**0.223**	0.416	0.000	1.000
urban area	**0.637**	0.481	0.000	1.000	**0.614**	0.487	0.000	1.000
core urban area	**0.091**	0.287	0.000	1.000	**0.119**	0.324	0.000	1.000
immigrants	**0.044**	0.040	0.000	0.333	**0.036**	0.039	0.000	0.665
unemployed	**0.082**	0.039	0.000	0.571	**0.088**	0.040	0.000	0.417
below 5 y/o	**0.060**	0.020	0.000	0.172	**0.062**	0.020	0.000	0.206
above 65 y/o	**0.170**	0.066	0.000	0.522	**0.176**	0.063	0.000	0.666
av. # children	**0.907**	0.245	0.000	2.000	**0.904**	0.220	0.000	2.500
farmers	**0.036**	0.053	0.000	0.444	**0.038**	0.050	0.000	0.600
executives	**0.070**	0.065	0.000	0.429	**0.052**	0.043	0.000	0.563
workers	**0.152**	0.080	0.000	1.000	**0.169**	0.069	0.000	0.667
retired	**0.280**	0.101	0.000	0.800	**0.286**	0.095	0.000	1.000
no diploma	**0.172**	0.069	0.000	0.463	**0.185**	0.067	0.000	0.614
baccalaureate	**0.156**	0.039	0.000	0.324	**0.153**	0.036	0.000	0.557
high education	**0.088**	0.071	0.000	0.507	**0.072**	0.045	0.000	0.542
commuters	**0.758**	0.154	0.000	1.000	**0.759**	0.143	0.000	1.000
homeowners	**0.803**	0.109	0.077	1.000	**0.782**	0.109	0.000	1.000
residents' income	**14,064**	4,362	5,495	59,093	**12,488**	2,863	3,273	65,758

Notes: Socio-demographic variables come from the 2008 census, which applies to the year 2010. Line 2 removes the 31 municipalities with a population higher than that of the largest resisting municipality from the control group. The variation in the population (line 3) is computed by comparing the 1999 and 2008 censuses. Indicator variables for whether the municipality is urban, part of an urban area, or in the urban core are based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality.

Table B1 (continued): Descriptive statistics using the control group only – 2010 (Panels B and C)

	Resisting municipalities (N=1,299)				Control municipalities (N=15,097)			
	mean	sd	min	max	**mean**	sd	min	max
Panel B	*Land-use and housing price characteristics*							
share built land (%)	**0.742**	1.416	0.024	15.882	**0.791**	1.411	0.006	21.892
average height	**1.561**	0.216	1.048	4.727	**1.569**	0.191	1.000	4.391
FAR (p30)	**0.110**	0.076	0.001	0.901	**0.109**	0.080	0.001	0.895
FAR (p50)	**0.171**	0.116	0.003	1.812	**0.173**	0.121	0.004	1.901
housing price/income	**0.116**	0.043	0.012	0.410	**0.111**	0.036	0.013	0.495
Panel C	*Political characteristics*							
turnout municipal	**0.763**	0.096	0.431	1.000	**0.774**	0.090	0.367	1.000
turnout presidential	**0.874**	0.040	0.655	1.000	**0.872**	0.039	0.600	1.000
voteshare right	**0.602**	0.107	0.152	1.000	**0.563**	0.105	0.143	1.000
voteshare far-right	**0.138**	0.057	0.000	0.467	**0.128**	0.054	0.000	0.519
right-wing mayor	**0.620**	0.486	0.000	1.000	**0.568**	0.495	0.000	1.000
left-wing mayor	**0.267**	0.443	0.000	1.000	**0.336**	0.472	0.000	1.000
NC mayor	**0.112**	0.316	0.000	1.000	**0.094**	0.292	0.000	1.000
woman mayor	**0.161**	0.368	0.000	1.000	**0.138**	0.345	0.000	1.000
age mayor	**57.1**	9.1	25.0	87.0	**56.1**	8.6	18.0	85.0
incumbent mayor	**0.661**	0.474	0.000	1.000	**0.626**	0.484	0.000	1.000
change orientations	**0.210**	0.407	0.000	1.000	**0.212**	0.409	0.000	1.000

Notes: Panel B: Land-use characteristics come from Combes et al. (2021) and each variable is built considering only construction before 2010. The share of built land is computed considering all construction, whereas the average height and floor-to-area ratio (FAR) are computed considering only housing construction. The average height gives the average number of housing stories. To measure the FAR stringency, I follow Combes et al. (2021) and take the 30th percentile of the distribution of realized floor-to-area ratios of all housing buildings in the municipality (FAR p(30)). I also show the statistics using the median (FAR p(50)). The last variable of Panel B measures the ratio of the housing price index over the average income per capita in 2010. To measure housing prices, I built indices based on official housing transactions, which can be interpreted as the price per square meter of a reference dwelling (see Appendix E). The data on housing prices are restricted to municipalities in which at least one transaction took place every even year over the period from 2000 to 2010.
Panel C: The municipal turnout rate and mayor's characteristics are based on the results of the 2008 municipal elections. The presidential turnout rate and far-right vote share come from the results of the first round of the 2007 presidential elections. The right-wing vote share comes from the results of the second round of the 2007 presidential elections. NC stands for "non classified".

Table B2: Descriptive statistics using municipalities' characteristics in 1999

	Resisting municipalities (N=1,299)				Integrated municipalities (N=26,991)			
	mean	sd	min	max	**mean**	sd	min	max
population	**1,511**	4,427	18	67,406	**1,615**	9,170	2	797,491
density	**150.4**	518.9	1.20	6,629	**133.3**	413.1	0.17	10,153
urban	**0.179**	0.384	0.000	1.000	**0.174**	0.379	0.000	1.000
urban area	**0.574**	0.495	0.000	1.000	**0.542**	0.498	0.000	1.000
core urban area	**0.078**	0.268	0.000	1.000	**0.093**	0.291	0.000	1.000
unemployed	**0.101**	0.046	0.000	0.345	**0.105**	0.048	0.000	0.500
below 5 y/o	**0.048**	0.018	0.000	0.152	**0.049**	0.017	0.000	0.250
above 65 y/o	**0.180**	0.073	0.025	0.514	**0.189**	0.071	0.000	0.778
av. # children	**0.949**	0.308	0.000	3.500	**0.947**	0.286	0.000	5.000
farmers	**0.051**	0.062	0.000	0.500	**0.052**	0.059	0.000	1.000
executives	**0.053**	0.056	0.000	0.429	**0.038**	0.036	0.000	0.500
workers	**0.159**	0.072	0.000	0.600	**0.171**	0.066	0.000	0.667
retired	**0.250**	0.091	0.000	0.750	**0.258**	0.086	0.000	1.000
no diploma	**0.200**	0.080	0.029	0.682	**0.219**	0.078	0.000	0.692
baccalaureate	**0.115**	0.039	0.000	0.286	**0.110**	0.035	0.000	0.500
high education	**0.060**	0.057	0.000	0.413	**0.046**	0.035	0.000	0.518
commuters	**0.733**	0.155	0.000	1.000	**0.723**	0.149	0.000	1.000
homeowners	**0.777**	0.113	0.000	1.000	**0.760**	0.110	0.000	1.000
residents' income	**8,434**	3,199	2,739	30,590	**7,252**	1,929	1,937	38,509

Notes: Data on the municipal population, age, education, and occupational composition come from the 1999 census. The share of immigrants is not available for this census year. Residents' income corresponds to the 2000 taxable income data. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality.

Table B3: Descriptive statistics – 2010: Urban municipalities (Panel A)

	Resisting municipalities (N=265)				Integrated municipalities (N=5,628)			
	mean	sd	min	max	**mean**	sd	min	max
Panel A	*Socio-demographic characteristics*							
population	**6,199**	9,001	137	72,939	**6,302**	20,376	77	85,1420
Δ population	**0.106**	0.160	-0.167	1.385	**0.086**	0.129	-0.257	2.070
density	**617.9**	1,081.7	26.2	6,883.5	**496.5**	826.6	12.4	9,976.5
urban area	**0.838**	0.369	0.000	1.000	**0.852**	0.355	0.000	1.000
core urban area	**0.445**	0.498	0.000	1.000	**0.518**	0.500	0.000	1.000
immigrants	**0.070**	0.047	0.005	0.281	**0.050**	0.044	0.000	0.395
unemployed	**0.092**	0.036	0.013	0.237	**0.099**	0.041	0.000	0.296
below 5 y/o	**0.059**	0.013	0.030	0.091	**0.059**	0.013	0.014	0.132
above 65 y/o	**0.165**	0.063	0.037	0.406	**0.169**	0.054	0.021	0.431
av. # children	**0.954**	0.187	0.416	1.471	**0.924**	0.158	0.376	1.833
farmers	**0.007**	0.012	0.000	0.103	**0.009**	0.013	0.000	0.190
executives	**0.099**	0.069	0.000	0.319	**0.070**	0.044	0.000	0.340
workers	**0.125**	0.057	0.016	0.305	**0.148**	0.052	0.000	0.400
retired	**0.265**	0.081	0.070	0.578	**0.274**	0.069	0.029	0.617
no diploma	**0.163**	0.064	0.018	0.359	**0.176**	0.064	0.025	0.557
baccalaureate	**0.164**	0.027	0.090	0.251	**0.155**	0.026	0.032	0.362
high education	**0.125**	0.092	0.025	0.507	**0.093**	0.055	0.000	0.542
commuters	**0.735**	0.154	0.172	0.949	**0.745**	0.150	0.123	1.000
homeowners	**0.711**	0.131	0.371	0.924	**0.701**	0.142	0.139	0.978
residents' income	**16,197**	4,867	8,963	40,218	**13,792**	3,229	6,164	57,126

Notes: The sample includes only urban municipalities. Socio-demographic variables come from the 2008 census, which applies to the year 2010. The variation in the population (line 2) is computed by comparing the 1999 and 2008 censuses. Indicator variables for whether the municipality is part of an urban area, or in the urban core are based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality.

Table B3 (continued): Descriptive statistics – 2010: Urban municipalities (Panels B and C)

	Resisting municipalities (N=265)				Integrated municipalities (N=5,628)			
	mean	sd	min	max	**mean**	sd	min	max
Panel B	*Land-use and housing price characteristics*							
share built land (%)	**2.377**	2.474	0.247	15.882	**2.183**	2.274	0.122	23.943
average height	**1.678**	0.317	1.135	4.727	**1.635**	0.233	1.061	4.391
FAR (p30)	**0.170**	0.085	0.030	0.493	**0.170**	0.089	0.008	1.465
FAR (p50)	**0.245**	0.146	0.061	1.812	**0.244**	0.126	0.027	1.901
housing price/income	**0.141**	0.046	0.060	0.315	**0.122**	0.035	0.020	0.586
Panel C	*Political characteristics*							
turnout municipal	**0.677**	0.086	0.431	0.883	**0.696**	0.084	0.367	0.985
turnout presidential	**0.869**	0.032	0.761	0.943	**0.863**	0.036	0.724	0.993
voteshare right	**0.592**	0.093	0.340	0.828	**0.541**	0.089	0.276	0.924
voteshare far-right	**0.115**	0.039	0.034	0.224	**0.119**	0.043	0.020	0.380
right-wing mayor	**0.589**	0.493	0.000	1.000	**0.492**	0.500	0.000	1.000
left-wing mayor	**0.343**	0.476	0.000	1.000	**0.444**	0.497	0.000	1.000
NC mayor	**0.068**	0.252	0.000	1.000	**0.063**	0.243	0.000	1.000
woman mayor	**0.140**	0.347	0.000	1.000	**0.109**	0.312	0.000	1.000
age mayor	**57.7**	8.9	30.0	81.0	**56.4**	8.6	23.0	83.0
incumbent mayor	**0.701**	0.459	0.000	1.000	**0.623**	0.485	0.000	1.000
change orientations	**0.170**	0.377	0.000	1.000	**0.203**	0.403	0.000	1.000

Notes: Panel B: Land-use characteristics come from Combes et al. (2021) and each variable is built considering only construction before 2010. The share of built land is computed considering all construction, whereas the average height and floor-to-area ratio (FAR) are computed considering only housing construction. The average height gives the average number of housing stories. To measure the FAR stringency, I follow Combes et al. (2021) and take the 30th percentile of the distribution of realized floor-to-area ratios of all housing buildings in the municipality (FAR p(30)). I also show the statistics using the median (FAR p(50)). The last variable of Panel B measures the ratio of the housing price index over the average income per capita in 2010. To measure housing prices, I built indices based on official housing transactions, which can be interpreted as the price per square meter of a reference dwelling (see Appendix E). The data on housing prices are restricted to municipalities in which at least one transaction took place every even year over the period from 2000 to 2010.
Panel C: The sample includes only urban municipalities. The municipal turnout rate and mayor's characteristics are based on the results of the 2008 municipal elections. The presidential turnout rate and far-right vote share come from the results of the first round of the 2007 presidential elections. The right-wing vote share comes from the results of the second round of the 2007 presidential elections. NC stands for "non classified".

Table B4: Descriptive statistics – 2010: Rural municipalities (Panel A)

	Resisting municipalities (N=1,034)				Integrated municipalities (N=21,363)			
	mean	sd	min	max	**mean**	sd	min	max
Panel A	*Socio-demographic characteristics*							
population	**472**	469	16	3,391	**501**	491	3	5,438
Δ population	**0.101**	0.160	-0.397	1.279	**0.104**	0.157	-0.500	2.692
density	**45.2**	50.3	2.1	525.1	**47.3**	57.8	0.2	2,750
urban area	**0.585**	0.493	0.000	1.000	**0.546**	0.498	0.000	1.000
immigrants	**0.038**	0.034	0.000	0.333	**0.034**	0.036	0.000	0.667
unemployed	**0.080**	0.040	0.000	0.571	**0.084**	0.040	0.000	0.429
below 5 y/o	**0.061**	0.022	0.000	0.172	**0.062**	0.022	0.000	0.206
above 65 y/o	**0.171**	0.067	0.000	0.522	**0.180**	0.067	0.000	0.667
av. # children	**0.895**	0.257	0.000	2.000	**0.884**	0.242	0.000	3.000
farmers	**0.044**	0.057	0.000	0.444	**0.046**	0.055	0.000	1.000
executives	**0.063**	0.062	0.000	0.429	**0.048**	0.044	0.000	0.563
workers	**0.159**	0.083	0.000	1.000	**0.170**	0.074	0.000	1.000
retired	**0.283**	0.106	0.000	0.800	**0.291**	0.104	0.000	1.000
no diploma	**0.175**	0.071	0.000	0.463	**0.184**	0.068	0.000	0.674
baccalaureate	**0.154**	0.042	0.000	0.324	**0.152**	0.039	0.000	0.557
high education	**0.079**	0.061	0.000	0.500	**0.068**	0.041	0.000	0.435
commuters	**0.763**	**0.154**	0.000	1.000	**0.761**	0.146	0.000	1.000
homeowners	**0.827**	**0.089**	0.077	1.000	**0.810**	0.082	0.000	1.000
residents' income	**13,515**	4,048	5,495	59,093	**12,312**	2,735	3,273	65,758

Notes: The sample includes only rural municipalities. Socio-demographic variables come from the 2008 census, which applies to the year 2010. The variation in the population (line 2) is computed by comparing the 1999 and 2008 censuses. The indicator variable for whether the municipality is part of an urban area is based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality.

Table B4 (continued): Descriptive statistics – 2010: Rural municipalities (Panels B and C)

	Resisting municipalities (N=1,034)				Integrated municipalities (N=21,363)			
	mean	sd	min	max	**mean**	sd	min	max
Panel B	*Land-use characteristics*							
share built land (%)	**0.323**	0.308	0.024	3.631	**0.341**	0.408	0.004	25.987
average height	**1.531**	0.169	1.048	2.325	**1.540**	0.168	1.000	3.000
FAR (p30)	**0.095**	0.065	0.001	0.901	**0.090**	0.065	0.000	0.965
FAR (p50)	**0.152**	0.099	0.003	1.614	**0.150**	0.108	0.003	1.790
housing price/income	**0.107**	0.037	0.012	0.410	**0.108**	0.038	0.013	0.634
Panel C	*Political characteristics*							
turnout municipal	**0.785**	0.086	0.499	1.000	**0.800**	0.078	0.464	1.000
turnout presidential	**0.876**	0.042	0.655	1.000	**0.876**	0.040	0.000	1.000
voteshare right	**0.604**	0.111	0.152	1.000	**0.569**	0.108	0.106	1.000
voteshare far-right	**0.144**	0.059	0.000	0.467	**0.133**	0.056	0.000	0.556
right-wing mayor	**0.628**	0.484	0.000	1.000	**0.577**	0.494	0.000	1.000
left-wing mayor	**0.248**	0.432	0.000	1.000	**0.309**	0.462	0.000	1.000
NC mayor	**0.124**	0.330	0.000	1.000	**0.113**	0.317	0.000	1.000
woman mayor	**0.166**	0.373	0.000	1.000	**0.151**	0.358	0.000	1.000
age mayor	**57.0**	9.1	25.0	87.0	**56.0**	8.8	18.0	88.0
incumbent mayor	**0.651**	0.477	0.000	1.000	**0.624**	0.484	0.000	1.000
change orientations	**0.220**	0.414	0.000	1.000	**0.226**	0.418	0.000	1.000

Notes: Panel B: Land-use characteristics come from Combes et al. (2021) and each variable is built considering only construction before 2010. The share of built land is computed considering all construction, whereas the average height and floor-to-area ratio (FAR) are computed considering only housing construction. The average height gives the average number of housing stories. To measure the FAR stringency, I follow Combes et al. (2021) and take the 30th percentile of the distribution of realized floor-to-area ratios of all housing buildings in the municipality (FAR p(30)). I also show the statistics using the median (FAR p(50)). The last variable of Panel B measures the ratio of the housing price index over the average income per capita in 2010. To measure housing prices, I built indices based on official housing transactions, which can be interpreted as the price per square meter of a reference dwelling (see Appendix E). The data on housing prices are restricted to municipalities in which at least one transaction took place every even year over the period from 2000 to 2010.
Panel C: The sample includes only rural municipalities. The municipal turnout rate and mayor's characteristics are based on the results of the 2008 municipal elections. The presidential turnout rate and far-right vote share come from the results of the first round of the 2007 presidential elections. The right-wing vote share comes from the results of the second round of the 2007 presidential elections. NC stands for "non classified".

Table B5: Impact on housing by municipality size

	(1)	(2)	(3)	(4)
Outcome	Number of housing building permits			
	All	$< 3,500$	$< 1,000$	< 500
Treatment	8.141***	7.280***	6.774***	6.819***
	(1.517)	(1.531)	(1.687)	(1.964)
Mun. FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs.	245,940	223,380	174,030	123,555
Mean	64.836	64.256	62.557	61.712
Sd	90.844	90.398	91.500	95.589

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 2 (resp. 3, 4) includes only municipalities with less than 3,500 (resp. 1,000, 500) inhabitants in 2010.

Table B6: Impact on housing inside urban areas: Residents' income

	(1)	(2)	(3)
Outcome	Number of housing building permits		
	All	Residents' income median	
		Below	Above
Treatment	10.494***	5.265*	9.418***
	(1.812)	(2.907)	(2.333)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	151,500	75,735	75,720
Mean DepVar	63.660	70.743	80.285
Sd DepVar	85.197	83.777	102.510

Notes: The sample includes only municipalities inside an urban area. Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 2 (resp. 3) includes only municipalities below (resp. above) the median of the per capita residents' annual taxable income in 2010. Data on taxable income in 2010 are missing for the 3 smallest municipalities.

Table B7: Impact on housing inside urban areas: CA and CU ICs

	(1)	(2)	(3)
Outcome	Number of housing building permits		
	All	Rural	Urban
Treatment	8.853**	7.831	16.928***
	(4.171)	(5.371)	(6.146)
Mun. FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Obs.	39,180	16,260	22,920
Mean	66.201	67.800	64.264
Sd	89.274	80.366	99.046

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample includes only municipalities inside an urban area and that are part of a "CA" or "CU" IC in 2014 (standing for *Communauté d'Agglomération* and *Communauté Urbaine*). Column 2 (resp. 3) includes only rural (resp. urban) municipalities.

Table B8: Impact on housing supply: Neighbors' characteristics

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of housing building permits per 10,000 inhabitants						
	All	Median income ratio		Median immigrants ratio		Median unemployed ratio	
		Below	Above	Below	Above	Below	Above
Treatment	8.141***	7.983***	7.413***	6.548***	9.745***	3.695*	12.634***
	(1.517)	(2.144)	(2.090)	(1.883)	(2.405)	(2.102)	(2.190)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	245,940	122,850	122,850	122,970	122,970	122,970	122,970
Mean DepVar	64.836	60.787	67.524	59.883	70.129	67.563	61.949
Sd DepVar	90.844	83.960	94.371	84.459	96.934	95.575	85.466

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). For each municipality, the ratio divides the value of the heterogeneity variable in the municipality by the average value in the other municipalities from the same département, weighted by their population. Column 2 (resp. 3) includes only municipalities below (resp. above) the median value of the ratio using the per capita residents' annual taxable income. Data on taxable income in 2010 are missing for the 16 smallest municipalities. Column 4 (resp. 5) includes only municipalities below (resp. above) the median value of the ratio using the share of immigrants in 2010. Columns 6 and 7 repeat the same exercise using the share of unemployed workers in 2010.

Interpretation:

The larger the income ratio, the richer the municipality is compared to its neighbors.

The larger the immigrants or unemployed ratio, the larger the share of immigrants or unemployed workers the municipality has compared to its neighbors.

Table B9: Impact on housing: Direct neighbors' characteristics

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of housing building permits per 10,000 inhabitants						
	All	Median income ratio		Median immigrants ratio		Median unemployed ratio	
		Below	Above	Below	Above	Below	Above
Treatment	8.141***	7.249***	9.348***	6.893***	9.224***	4.362**	12.079***
	(1.517)	(2.126)	(2.128)	(2.118)	(2.171)	(2.174)	(2.103)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	245,940	122,850	122,850	122,970	122,970	122,970	122,970
Mean DepVar	64.836	60.737	68.200	63.472	66.330	67.458	61.938
Sd DepVar	90.844	84.897	94.558	87.958	93.890	95.820	84.922

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). For each municipality, the ratio divides the value of the heterogeneity variable in the municipality by the average value for the neighboring municipalities—defined as municipalities sharing a common border—weighted by their population. Column 2 (resp. 3) includes only municipalities below (resp. above) the median value of the ratio using the per capita residents' annual taxable income. Data on taxable income in 2010 are missing for the 16 smallest municipalities. Column 4 (resp. 5) includes only municipalities below (resp. above) the median value of the ratio using the share of immigrants in 2010. Columns 6 and 7 repeat the same exercise using the share of unemployed workers in 2010.
Interpretation:
The larger the income ratio, the richer the municipality is compared to its neighbors.
The larger the immigrants or unemployed ratio, the larger the share of immigrants or unemployed workers the municipality has compared to its neighbors.

Table B10: Impact on housing: Neighbors' share of non-European immigrants

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of housing building permits				
	All	Median non-European immigrants ratio			
		Département		Direct neighbors	
		Below	Above	Below	Above
Treatment	8.141***	8.359***	8.159***	4.898**	11.150***
	(1.517)	(1.778)	(2.531)	(2.062)	(2.260)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Observations	245,940	122,970	122,970	121,440	121,425
Mean DepVar	64.836	58.932	71.609	63.189	67.397
Sd DepVar	90.844	84.465	97.219	88.437	92.985

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). For each municipality, the ratio divides the share of non-European immigrants in the municipality by the average share in surrounding municipalities, weighted by their population. Surrounding municipalities are defined either as all the other municipalities from the same département (Columns 2 and 3) or as municipalities sharing a border (Columns 4 and 5). Columns 2 and 4 (resp. 3 and 5) includes only municipalities below (resp. above) the median value of the ratio. The ratio considering direct neighbors (Columns 4 and 5) is missing for 205 municipalities whose neighboring municipalities had zero non-European immigrants.

Interpretation:

The larger the non-European immigrants ratio, the larger the share of non-European immigrants the municipality has compared to its neighbors.

Table B11: Impact on housing: Political alignment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Number of housing building permits per 10,000 inhabitants						
	All	Same orientation as district official		Vote-share-distance median			
				Département		Direct neighbors	
		Yes	No	Below	Above	Below	Above
Treatment	8.141***	8.699***	7.239***	5.962***	9.951***	7.936***	8.331***
	(1.517)	(1.964)	(2.380)	(2.195)	(2.092)	(2.203)	(2.093)
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	245,940	141,885	103,080	122,970	122,970	122,970	122,970
Mean	64.836	64.925	64.685	69.661	60.908	69.458	60.524
Sd	90.844	91.436	89.837	94.704	87.388	94.093	87.494

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 2 (resp. 3) includes only municipalities whose mayor had the same orientation (resp. a different orientation) as the member of parliament of their district in 2010. Column 4 (resp. 5) includes only municipalities below (resp. above) the median value of the absolute difference in the right-wing vote share in the 2007 presidential election between the municipality and the other municipalities from the same département. In Columns 6 and 7, surrounding municipalities are defined as direct neighbors.

Table B12: Impact on housing: Share of homeowners: Urban municipalities

	(1)	(2)	(3)
Outcome	Number of housing building permits		
	All	Median % homeowners	
		Below	Above
Treatment	13.774***	13.139**	14.364***
	(3.691)	(5.548)	(4.883)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	54,420	27,210	27,210
Mean DepVar	69.875	74.602	65.253
Sd DepVar	93.921	100.447	86.876

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The sample includes only urban municipalities. The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 4 (resp. 5) includes only municipalities below (resp. above) the median value of the share of homeowners in 2010.

Table B13: Impact on housing depending on mayors' incumbency status: Urban areas

	(1)	(2)	(3)
Outcome	Number of building permits		
	All	Incumbent	Not incumbent
Treatment	10.494***	10.883***	10.622***
	(1.812)	(2.347)	(2.851)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	151,500	93,315	55,605
Mean DepVar	63.660	64.866	61.206
Sd DepVar	85.197	84.970	85.156

Notes: The sample includes only municipalities part of an urban area. Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 2 (resp. 3) includes only municipalities whose mayor was the incumbent in 2010 and had thus been in place since at least 2001 (resp. was not the incumbent and was thus newly elected in 2008).

Table B14: Impact on public services depending on mayors' incumbency status: Rural municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Daycare spots			Public libraries		
	All	Incumbent	Not incumbent	All	Incumbent	Not incumbent
Treatment	-1.001**	-0.994	-0.893	-1.248**	-1.582*	-0.490
	(0.463)	(0.650)	(0.603)	(0.620)	(0.898)	(0.417)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	153,216	94,536	55,800	11,020	6,750	4,060
Mean	3.617	4.183	2.713	5.277	5.381	4.147
Sd	31.900	35.382	24.956	10.983	11.754	7.704

Notes: The sample includes only rural municipalities. Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). In Columns 1 to 3, the outcome is the number of child daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Columns 4 to 6, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), the period of analysis is 2009–2018, and the sample is restricted to the 7 départements for which data are available starting in 2009. Columns 2 and 5 (resp. 3 and 6) include only municipalities whose mayor was the incumbent in 2010 and had thus been in place since at least 2001 (resp. was not the incumbent and was thus newly elected in 2008).

Appendix C. Additional robustness checks

In this section, I describe and present additional robustness tests to support the identification strategy. As the negative impact on public services is significant only for rural municipalities, I present the robustness tests on public services for rural municipalities only.

C1. Impact depending on the latest date of integration of the control municipalities

For each outcome, I test the robustness of the results to varying the control group depending on the latest date of integration of the control municipalities. In each Table Celow, the first column gives the baseline estimate restricting the control group to municipalities integrated since 1999. The next columns provide the estimates obtained by considering instead all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively. The last column includes all municipalities already integrated in 2010.

Table C1.1: Housing supply

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of building permits per 10,000 inhabitants					
Latest integration	1999	2002	2004	2006	2008	2010
Treatment	8.141***	7.865***	7.505***	7.306***	7.318***	7.396***
	(1.517)	(1.496)	(1.490)	(1.489)	(1.488)	(1.487)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	245,940	335,685	389,325	407,010	415,815	426,495
Mean DepVar	64.836	64.836	64.836	64.836	64.836	64.836
Sd DepVar	90.844	90.844	90.844	90.844	90.844	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Column 1 reproduces the baseline estimate as reported in the paper, using as control group municipalities integrated since 1999. The next columns include in the control group all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively.

Table C1.2: Daycare: Rural municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of daycare spots per 10,000 inhabitants					
Latest integration	1999	2002	2004	2006	2008	2010
Treatment	-1.001**	-0.969**	-1.027**	-1.091**	-1.059**	-1.019**
	(0.463)	(0.437)	(0.437)	(0.434)	(0.431)	(0.430)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	153,216	210,816	245,412	256,680	262,704	270,180
Mean DepVar	3.617	3.617	3.617	3.617	3.617	3.617
Sd DepVar	31.900	31.900	31.900	31.900	31.900	31.900

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The sample includes only rural municipalities. The outcome is the number of daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. Column 1 reproduces the baseline estimate as reported in the paper, using as a control group municipalities integrated since 1999. The next columns include in the control group all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively.

Table C1.3: Public libraries: Rural municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of public libraries per 10,000 inhabitants					
Latest integration	1999	2002	2004	2006	2008	2010
Treatment	-1.248**	-1.340**	-1.475**	-1.501**	-1.500**	-1.483**
	(0.620)	(0.617)	(0.615)	(0.614)	(0.614)	(0.614)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,020	14,820	18,170	18,960	19,030	19,580
Mean DepVar	5.277	5.277	5.277	5.277	5.277	5.277
Sd DepVar	10.983	10.983	10.983	10.983	10.983	10.983

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) The sample includes only rural municipalities. The outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population). The period of analysis is 2009–2018 and the sample is restricted to the 7 départements for which data are available starting in 2009. Column 1 reproduces the baseline estimate as reported in the paper, using as a control group municipalities integrated since 1999. The next columns include in the control group all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively.

Table C1.4: Public transport

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Access to public transport					
Latest integration	1999	2002	2004	2006	2008	2010
Treatment	0.032***	0.031***	0.031***	0.031***	0.030***	0.030***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221,368	304,178	350,938	364,994	372,918	382,088
Mean DepVar	0.024	0.024	0.024	0.024	0.024	0.024
Sd DepVar	0.152	0.152	0.152	0.152	0.152	0.152

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) The outcome is an indicator variable equal to one if the municipality has access to public transport. The sample excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. Column 1 reproduces the baseline estimate as reported in the paper, using as a control group municipalities integrated since 1999. The next columns include in the control group all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively.

Table C1.5: Fiscal revenues

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Fiscal revenues per capita					
Latest integration	1999	2002	2004	2006	2008	2010
Treatment	101.2***	102.6***	101.0***	101.1***	100.0***	98.2***
	(4.8)	(4.8)	(4.7)	(4.7)	(4.7)	(4.7)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	244,965	334,335	387,630	405,195	413,970	424,650
Mean DepVar	698.6	698.6	698.6	698.6	698.6	698.6
Sd DepVar	430.0	430.0	430.0	430.0	430.0	430.0

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010) The outcome is the municipality's total fiscal revenues per capita, as defined in Section 7.2. I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample). Column 1 reproduces the baseline estimate as reported in the paper, using as a control group municipalities integrated since 1999. The next columns include in the control group all municipalities integrated since 2002, 2004, 2006, 2008, and 2010, respectively.

C2. Clusters at the IC level

Table C2.1: Housing supply

	(1)	(2)	(3)
Outcome	Number of building permits		
Cluster	Municipality	IC 2014	IC 2018
Treatment	8.141***	8.141***	8.141***
	(1.517)	(2.250)	(2.344)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	245,940	245,940	245,940
Mean DepVar	64.836	64.836	64.836
Sd DepVar	90.844	90.844	90.844

Notes: Standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. In Column 1, standard errors are clustered at the municipality level. In Column 2 (resp. 3), standard errors are clustered at the IC level, considering the IC the municipality belongs to in 2014 (resp. 2018). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population).

Table C2.2: Local public services: Rural municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Daycare spots			Public libraries		
Cluster	Municipality	IC 2014	IC 2018	Municipality	IC 2014	IC 2018
Treatment	-1.001**	-1.001*	-1.001*	-1.248**	-1.248**	-1.248**
	(0.463)	(0.531)	(0.533)	(0.620)	(0.474)	(0.478)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	153,216	153,216	153,216	11,020	11,020	11,020
Mean	3.617	3.617	3.617	5.277	5.277	5.277
Sd	31.900	31.900	31.900	10.983	10.983	10.983

Notes: Standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. In Column 1, standard errors are clustered at the municipality level. In Column 2 (resp. 3), standard errors are clustered at the IC level, considering the IC the municipality belongs to in 2014 (resp. 2018). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). In Columns 1 to 3, the outcome is the number of daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Columns 4 to 6, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), the period of analysis is 2009–2018, and the sample is restricted to the 7 départements for which data are available starting in 2009.

Table C2.3: Public transport and fiscal revenues

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Access to public transport			Fiscal revenues per capita		
Cluster	Municipality	IC 2014	IC 2018	Municipality	IC 2014	IC 2018
Treatment	0.032***	0.032**	0.032***	101.2***	101.2***	101.2***
	(0.007)	(0.013)	(0.012)	(4.813)	(9.073)	(9.027)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	221,368	221,368	221,368	244,965	244,965	244,965
Mean	0.024	0.024	0.024	698.6	698.6	698.6
Sd	0.152	0.152	0.152	430.0	430.0	430.0

Notes: Standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. In Column 1, standard errors are clustered at the municipality level. In Column 2 (resp. 3), standard errors are clustered at the IC level, considering the IC the municipality belongs to in 2014 (resp. 2018). The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). In Columns 1 to 3, the outcome is an indicator variable equal to 1 if the municipality has access to public transport and the sample excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. In Columns 4 to 6, the outcome is the municipality's total fiscal revenues per capita, as defined in Section 7.2, and I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample).

C3. Matching

I test the robustness of the main results to using kernel-based propensity score matching. I use the Stata "diff" package to perform the matching (Villa, 2016) and I match control and treated municipalities based on the socio-demographic characteristics available in the 2008 census, which applies to the year 2010. The variables used are the following: the number of inhabitants; population growth since 1999; population density; whether the municipality is urban; whether it belongs to an urban area; whether it belongs to the core of the urban area; the share of immigrants; the share of unemployed workers; the share of students; the share of the population included in the labor force; the share of the population below 5 years old, between 15 and 64 years old, and above 65 years old; the average number of children per family; the share of the active population being farmers, craftsperson, executives, temporary employed, full-time employed, workers, retired, or others; the share of the population with no diploma, holding a primary school certificate (CEP), holding a secondary education diploma (BEPC), holding a certificate of vocational aptitude (CAP or BEP), holding the baccalaureate, who completed two years after the baccalaureate, or with higher education; the share of commuters and homeowners; and the per capita residents' annual taxable income.

In Tables C3.1 and C3.2, I report the differences between the control and treatment groups along with the t-tests for each variable, with and without using matching, respectively. The next tables provide the estimates. The first column gives the baseline estimate, the second column the estimate obtained using kernel-based propensity score matching, and the third column the estimate using matching on the common support of the propensity score. Given the very small sample for which library data are available, the analysis for public services is restricted to daycare.

Table C3.1: T-tests without matching

	Mean treatment	Mean control	Difference (T-C)	P-value
population	1,640	1,959	-319	0.048**
Δ population	0.102	0.100	0.002	0.596
density	162.0	156.3	5.8	0.709
urban mun	0.204	0.223	-0.019	0.108
urban area	0.637	0.614	0.022	0.107
core urban area	0.091	0.119	-0.028	0.001***
immigrants	0.044	0.036	0.008	0.000***
unemployed	0.082	0.088	-0.005	0.000***
students	0.077	0.078	-0.001	0.243
labor force	0.738	0.731	0.008	0.000***
below 5 y/o	0.060	0.062	-0.002	0.001***
15-64 y/o	0.639	0.633	0.006	0.000***
above 65 y/o	0.170	0.176	-0.006	0.002***
av. # children	0.904	0.907	0.003	0.685
farmers	0.036	0.038	-0.001	0.328
craftsperson	0.041	0.037	0.004	0.000***
executives	0.070	0.052	0.019	0.000***
part-time employed	0.133	0.125	0.008	0.000***
full-time employed	0.153	0.155	-0.002	0.154
workers	0.152	0.169	-0.017	0.000***
retired	0.280	0.286	-0.006	0.036*
others	0.134	0.139	-0.004	0.010**
no diploma	0.172	0.185	-0.013	0.000***
CEP certificate	0.139	0.146	-0.007	0.000***
BEPC	0.061	0.060	0.001	0.158
CAP or BEP	0.271	0.279	-0.007	0.000***
baccalaureate	0.156	0.153	0.004	0.002***
bac + 2 years	0.112	0.107	0.005	0.000***
high education	0.088	0.072	0.017	0.000***
commuters	0.758	0.759	-0.001	0.809
homeowners	0.782	0.803	0.022	0.000***
residents' income	14,064	12,488	1,575	0.000***

Notes: Data on the municipal population, age, education, and occupational composition come from the 2008 census, which applies to the year 2010. The variation in the population (line 2) is computed as the variation in the number of inhabitants between the 1999 and 2008 censuses. Indicator variables for whether the municipality is urban, part of an urban area, or located in the urban core are based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality. ***, **, and * indicate significance at 1%, 5%, and 10% respectively.

Table C3.2: T-tests with matching

	Mean treatment	Mean control	Difference (T-C)	P-value
population	1,640	1,579	61	0.668
Δ population	0.102	0.104	-0.002	0.731
density	162.0	146.9	15.1	0.331
urban mun	0.204	0.200	0.004	0.748
urban area	0.637	0.625	0.011	0.426
core urban area	0.091	0.092	-0.001	0.913
immigrants	0.044	0.044	0.001	0.556
unemployed	0.082	0.084	-0.002	0.160
students	0.077	0.077	0.001	0.388
labor force	0.738	0.737	0.002	0.356
below 5 y/o	0.060	0.061	-0.000	0.506
15-64 y/o	0.639	0.638	0.001	0.326
above 65 y/o	0.170	0.172	-0.002	0.315
av. # children	0.900	0.907	0.007	0.341
farmers	0.036	0.037	-0.001	0.440
craftsperson	0.041	0.040	0.001	0.195
executives	0.070	0.064	0.006	0.001***
part-time employed	0.133	0.131	0.002	0.231
full-time employed	0.153	0.154	-0.001	0.559
workers	0.152	0.157	-0.005	0.022**
retired	0.280	0.281	-0.002	0.554
others	0.134	0.135	-0.001	0.686
no diploma	0.172	0.177	-0.004	0.041**
CEP certificate	0.139	0.141	-0.002	0.176
BEPC	0.061	0.060	0.000	0.680
CAP or BEP	0.271	0.275	-0.003	0.070*
baccalaureate	0.156	0.155	0.001	0.367
bac + 2 years	0.112	0.110	0.002	0.088*
high education	0.088	0.082	0.006	0.004***
commuters	0.758	0.759	-0.001	0.830
homeowners	0.803	0.798	0.005	0.135
residents' income	14,064	13,669	395	0.012***

Notes: Data on the municipal population, age, education, and occupational composition come from the 2008 census, which applies to the year 2010. The variation in the population (line 2) is computed as the variation in the number of inhabitants between the 1999 and 2008 censuses. Indicator variables for whether the municipality is urban, part of an urban area, or located in the urban core are based on the INSEE 2010 classification. The share of commuters is computed as the share of employed residents working in a municipality different from their home municipality. ***, **, and * indicate significance at 1%, 5%, and 10% respectively.

Table C3.3: Housing

	(1)	(2)	(3)
Outcome	Number of building permits		
Matching	No	Yes	Common support
Treatment	8.141***	7.261***	7.417***
	(1.517)	(1.676)	(1.670)
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	245,940	245,685	245,160
Mean DepVar	64.836	64.836	64.836
Sd DepVar	90.844	90.844	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the full treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). In Column 2, the analysis used propensity score matching. Column 3 repeats the same exercise on the common support of the propensity score.

Table C3.4: Daycare: Rural municipalities

	(1)	(2)	(3)
Outcome	Daycare spots		
Matching	No	Yes	Common support
Treatment	-1.001**	-0.743	-0.743
	(0.463)	(0.482)	(0.483)
Mun FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Obs.	153,216	153,024	152,736
Mean	3.617	3.617	3.617
Sd	31.900	31.900	31.900

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the full treatment group during the pre-reform period (before 2010). The sample includes only rural municipalities. The outcome is the number of daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Column 2, the analysis uses propensity score matching. Column 3 repeats the same exercise on the common support of the propensity score.

Table C3.5: Public transport and fiscal revenues

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Access to public transport			Fiscal revenues per capita		
Matching	No	Yes	+ common support	No	Yes	+ common support
Treatment	0.032***	0.032***	0.032***	101.2***	95.03***	94.85***
	(0.007)	(0.007)	(0.007)	(4.813)	(5.161)	(5.168)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	221,368	221,116	219,940	244,965	244,725	244,215
Mean	0.024	0.024	0.024	698.6	698.6	698.6
Sd	0.152	0.152	0.152	430.0	430.0	430.0

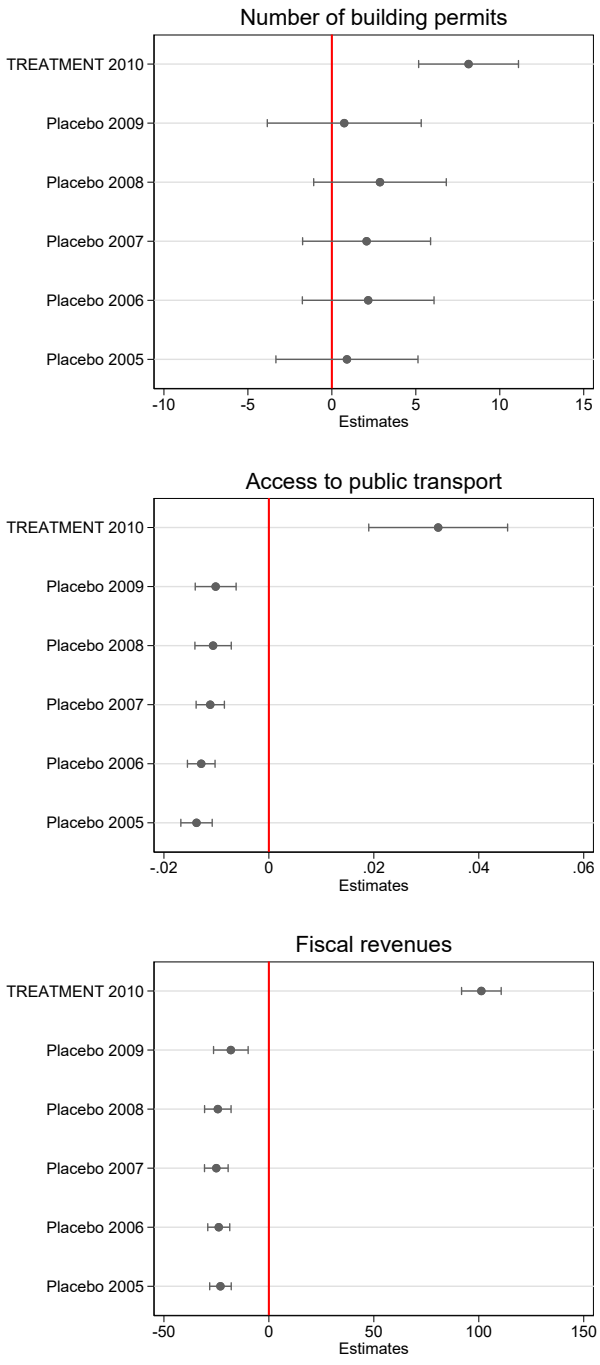
Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the full treatment group during the pre-reform period (before 2010). In Columns 1 to 3, the outcome is an indicator variable equal to 1 if the municipality has access to public transport and the sample excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. In Columns 4 to 6, the outcome is the municipality's total fiscal revenues per capita, as defined in Section 7.2, and I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample). In Column 2, the analysis uses propensity score matching. Column 3 repeats the same exercise on the common support of the propensity score.

C4. Placebo tests

In this section, I test the impact of a series a placebo reforms. I consider only the pre-treatment period from 2004 to 2010 and I run the same specification as the one described in Section 4.2, pretending that the law passed in 2005, 2006, 2007, 2008, or 2009. The graph below provides the estimates obtained for each placebo reform, along with the impact of the true reform (first coefficient on each graph). Unfortunately, I cannot run these placebo tests on daycare and public libraries, given the few pre-treatment periods I have in the data. This analysis is thus restricted to the number of building permits, access to public transport, and fiscal revenues.

As seen Figure C4, no coefficient associated with placebo reforms is significant at the standard level for the number of building permits. For public transport and fiscal revenues, consistent with the presence of decreasing pre-trends (see Section 7), the placebo estimates are significant but negative, which contrasts with the positive effect of the true reform. All in all, these results support the fact that the main results are capturing the impact of the 2010 law rather than the impact of factors that systematically affect treated and control municipalities differently.

Table C4: Placebo tests



Notes: The figure shows the impact of a series of placebo reforms on the number of building permits, the probability of access to public transport, and fiscal revenues. In each graph, the first coefficient refers to the impact of the 2010 law, while the other estimates give the impact of a placebo reform implemented in 2005, 2006, 2007, 2008, and 2009, respectively. When estimating the impact of the placebo reforms, I include only the pre-treatment period from 2004 to 2010. Horizontal lines are 95-percent-confidence intervals.

C5. Alternative control groups

Table C5.1: Housing supply

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of building permits per 10,000 inhabitants				
Control group	Group 1	Group 2	Group 3	Group 4	Group 5
Treatment	8.141***	8.547***	11.249***	8.511***	11.557***
	(1.517)	(1.537)	(1.637)	(1.524)	(1.638)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Observations	245,940	190,410	75,825	224,310	74,040
Mean DepVar	64.836	64.836	64.836	64.836	64.836
Sd DepVar	90.844	90.844	90.844	90.844	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). In Column 1, the control group is the one used in the baseline estimation, including all municipalities integrated since 1999 (Group 1). In Column 2, I exclude from the control group all municipalities that were part of an IC that received a treated municipality as a result of the 2010 law (Group 2). In Column 3, I exclude more generally all control municipalities whose IC had changed since 1999 and until 2014, as a result of the 2010 law (Group 3). In Column 4, I exclude control municipalities that share a border with a treated municipality (Group 4). In Column 5, I exclude both control municipalities whose ICs changed and neighbors (Group 5).

Table C5.2: Local public services: Rural municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Daycare spots					Public libraries				
Control	Group 1	Group 2	Group 3	Group 4	Group 5	Group 1	Group 2	Group 3	Group 4	Group 5
Treat.	-1.001**	-0.781	-1.048*	-0.922*	-1.127*	-1.248**	-1.418**	-1.417**	-1.276**	-1.423**
	(0.463)	(0.497)	(0.581)	(0.472)	(0.591)	(0.620)	(0.635)	(0.635)	(0.623)	(0.636)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	153,216	117,996	49,200	139,788	48,132	11,020	7,970	4,100	10,140	4,030
Mean	3.617	3.617	3.617	3.617	3.617	5.277	5.277	5.277	5.277	5.277
Sd	31.900	31.900	31.900	31.900	31.900	10.983	10.983	10.983	10.983	10.983

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The sample includes only rural municipalities. In Columns 1 to 3, the outcome is the number of daycare spots in the municipality per 10,000 inhabitants (using the 2010 population) and the period of analysis is 2007–2018. In Columns 4 to 6, the outcome is the number of public libraries in the municipality per 10,000 inhabitants (using the 2010 population), the period of analysis is 2009-2018, and the sample is restricted to the 7 départements for which data are available starting in 2009. In Column 1, the control group is the one used in the baseline estimation, including all municipalities integrated since 1999 (Group 1). In Column 2, I exclude from the control group all municipalities that were part of an IC that received a treated municipality as a result of the 2010 law (Group 2). In Column 3, I exclude more generally all control municipalities whose IC had changed since 1999 and until 2014, as a result of the 2010 law (Group 3). In Column 4, I exclude control municipalities that share a border with a treated municipality (Group 4). In Column 5, I exclude both control municipalities whose ICs changed and neighbors (Group 5).

Table C5.3: Public transport and fiscal revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Access to public transport					Fiscal revenues per capita				
Control	Group 1	Group 2	Group 3	Group 4	Group 5	Group 1	Group 2	Group 3	Group 4	Group 5
Treat.	0.032***	0.036***	0.058***	0.033***	0.058***	101.2***	98.16***	101.2***	101.0***	101.7***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(4.813)	(4.829)	(5.248)	(4.824)	(5.262)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	221,368	171,276	66,304	202,552	64,848	244,965	189,975	75,510	223,440	73,740
Mean	0.024	0.024	0.024	0.024	0.024	698.6	698.6	698.6	698.6	698.6
Sd	0.152	0.152	0.152	0.152	0.152	430.0	430.0	430.0	430.0	430.0

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). In Columns 1 to 5, the outcome is an indicator variable equal to 1 if the municipality has access to public transport and the sample excludes municipalities in the Parisian region of Île-de-France, for which the data are not available. In Columns 6 to 10, the outcome is the municipality's total fiscal revenues per capita, as defined in Section 6.2, and I exclude from the analysis the few municipalities for which the data are missing for at least one year over the period 2004–2018 (0.4 percent of the sample). In Column 1, the control group is the one used in the baseline estimation, including all municipalities integrated since 1999 (Group 1). In Column 2, I exclude from the control group all municipalities that were part of an IC that received a treated municipality as a result of the 2010 law (Group 2). In Column 3, I exclude more generally all control municipalities whose IC had changed since 1999 and until 2014, as a result of the 2010 law (Group 3). In Column 4, I exclude control municipalities that share a border with a treated municipality (Group 4). In Column 5, I exclude both control municipalities whose ICs changed and neighbors (Group 5).

Appendix D. Housing supply: Heterogeneity analysis

In the main text and in Appendix A, I report the impact on housing supply obtained by splitting the full sample according to the median value of the heterogeneity variable I consider. In this section, I explore the heterogeneity of the treatment impact by adding an interaction term with the continuous heterogeneity variable. I estimate the following equation:

$$Y_{mt} = \alpha + \beta 1_{\{t>2010\}} 1_{\{treated_m=1\}} + \gamma Z_m 1_{\{treated_m=1\}} + \eta Z_m 1_{\{t>2010\}} + \psi Z_m 1_{\{t>2010\}} 1_{\{treated_m=1\}} + \delta_t + \theta_m + \varepsilon_{mt},$$

where m stands for the municipality and t for the year. $1_{\{t>2010\}}$ is an indicator variable equal to 1 for years after the reform, starting in 2011. $1_{\{treated_m=1\}}$ is an indicator variable equal to 1 for municipalities that were isolated in 2010 and thus forced to join an IC (treatment group), and 0 for municipalities already integrated since 1999 (control group). δ_t and θ_m are time and municipality fixed effects, respectively. $Z_m$is the heterogeneity variable measured in 2010. I standardize each heterogeneity variable, subtracting its mean in the treatment group and dividing it by its standard error. As a result, in the tables below, the impact of the treatment β can be interpreted as the impact for a treated municipality with an average value of Z_m and ψ can be interpreted as the change in the treatment effect due to a one-standard-deviation increase in Z_m.

Table D1: Distance to the core of the urban area

	(1)	(2)
Outcome	Number of housing building permits	
Heterogeneity	Distance to urban core	
	Ratio	Log ratio
Treatment	9.473***	10.434***
	(1.913)	(1.918)
Interaction	-3.635*	-6.663***
	(1.906)	(2.011)
Municipality FE	Yes	Yes
Time FE	Yes	Yes
Observations	121,560	121,350
Mean DepVar	63.195	63.195
Sd DepVar	85.954	85.954

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The sample includes only municipalities that are part of one urban area. It excludes municipalities outside of any urban areas or that are part of several urban areas, such that we cannot identify the core to which they are linked. The heterogeneity variable is the ratio between the municipality's Euclidean distance to the core divided by the average distance to the core of the other municipalities from the same urban area. The coordinates of the core are computed as the average coordinates of the different municipalities composing the core, weighted by their population. In Column 2, I consider the log of the ratio, thus excluding 14 municipalities in the control group that constitute the core of their urban area and for which the distance is thus 0.

Table D2: Residents' income

	(1)
Outcome	Number of housing building permits
Heterogeneity	Residents' income
Treatment	5.407***
	(1.560)
Interaction	3.734**
	(1.856)
Municipality FE	Yes
Time FE	Yes
Observations	245,700
Mean DepVar	64.780
Sd DepVar	90.330

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The heterogeneity variable is the per capita residents' annual taxable income in 2010. Data on taxable income in 2010 are missing for the 16 municipalities with the smallest populations.

Table D3: Housing density

For this heterogeneity test, in the second column, I also include the interaction between the treatment variable and the square value of the housing density. Indeed, even if we expect the impact on housing to be larger in denser municipalities, we might not expect the densest places to experience the largest increase, as they may be too dense for their housing supply to increase as much as elsewhere. As a result, the effect is likely to rise non monotonically with the housing density. The results confirm this hypothesis: while in the first column the interaction is close to zero and not significant, in the second column it is large and significant and the estimate associated to the interaction with the square value is negative and significant.

	(1)	(2)
Outcome	Number of housing building permits	
Heterogeneity	Housing density	
Treatment	10.395***	12.126***
	(1.787)	(1.838)
Interaction	-0.220	9.966***
	(1.896)	(3.636)
Interaction²		-1.435***
		(0.463)
Municipality FE	Yes	Yes
Time FE	Yes	Yes
Observations	151,500	151,500
Mean DepVar	63.660	63.660
Sd DepVar	85.197	85.197

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The heterogeneity variable is the number of housing units per square kilometer in 2010. In Column 2, I also include the interaction between the treatment variable and the square of the housing density.

Table D4: Neighbors' characteristics: Département

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of housing building permits				
Heterogeneity	Ratio income	Ratio immigrants	Ratio non-euro. imm.	Ratio unemployed	Vote-share distance
Treatment	7.466***	7.850***	8.404***	7.993***	8.184***
	(1.517)	(1.513)	(1.514)	(1.518)	(1.517)
Interaction	1.836	-1.344	0.042	1.212	1.155
	(1.590)	(2.210)	(1.713)	(1.666)	(1.889)
Mun. FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Obs.	245,700	245,940	245,940	245,940	245,940
Mean	64.780	64.836	64.836	64.836	64.836
Sd	90.330	90.844	90.844	90.844	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Each ratio (Columns 1 to 3) divides the value of the heterogeneity variable in the municipality by the average value in the other municipalities from the same département, weighted by their population. In Column 1, the heterogeneity variable used to compute the ratio is the residents' average annual taxable income. Data on taxable income in 2010 are missing for the 16 smallest municipalities. In Column 2 (resp. 3, 4), the heterogeneity variable is the share of all immigrants (resp. non-European immigrants, unemployed workers) in 2010. In Column 5, the heterogeneity variable is the absolute difference in the right-wing vote share in the 2007 presidential election, between the municipality and the other municipalities in the same département.

Interpretation for the ratio heterogeneity variables:

The larger the income ratio, the richer the municipality is compared to its neighbors.

The larger the immigrants or unemployed ratio, the larger the share of immigrants or unemployed workers the municipality has compared to its neighbors.

Table D5: Neighbors' characteristics: Direct neighbors

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of housing building permits				
Heterogeneity	Ratio revenues	Ratio immigrants	Ratio non-Euro. imm.	Ratio unemployed	Vote share distance
Treatment	8.322***	8.011***	7.942***	7.991***	8.127***
	(1.514)	(1.517)	(1.527)	(1.518)	(1.523)
Interaction	1.448	1.296	0.551	3.025*	-0.640
	(1.646)	(1.705)	(1.502)	(1.590)	(2.030)
Mun. FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Obs.	245,700	245,940	242,865	245,940	245,940
Mean	64.780	64.836	65.219	64.836	64.836
Sd	90.330	90.844	90.679	90.844	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). Each ratio (Columns 1 to 3) divides the value of the heterogeneity variable in the municipality by the average value in neighboring municipalities (defined as sharing a border), weighted by their population. In Column 1, the heterogeneity variable used to compute the ratio is the residents' average annual taxable income. Data on taxable income in 2010 are missing for the 16 smallest municipalities. In Column 2 (resp. 3, 4), the heterogeneity variable is the share of all immigrants (resp. non-European immigrants, unemployed workers) in 2010. In Column 5, the heterogeneity variable is the absolute difference in the right-wing vote share in the 2007 presidential election, between the municipality and the other municipalities in the same département. The ratio considering the share of non-European immigrants (Column 3) is missing for 205 municipalities for which neighboring municipalities had zero non-European immigrants.
Interpretation for the ratio heterogeneity variables:
The larger the income ratio, the richer the municipality is compared to its neighbors.
The larger the immigrants or unemployed ratio, the larger the share of immigrants or unemployed workers the municipality has compared to its neighbors.

Table D6: Share of homeowners

	(1)
Outcome	Number of housing building permits
Heterogeneity	Share of homeowners
Treatment	8.789***
	(1.509)
Interaction	-1.462
	(1.672)
Municipality FE	Yes
Time FE	Yes
Observations	245,940
Mean DepVar	64.836
Sd DepVar	90.844

Notes: Standard errors are in parentheses and are clustered at the municipality level. ***, **, and * indicate significance at 1%, 5%, and 10% respectively. The mean of the dependent variable gives the average of the outcome variable in the treatment group during the pre-reform period (before 2010). The outcome is the number of housing building permits delivered in the municipality per year per 10,000 inhabitants (using the 2010 population). The heterogeneity variable is the share of homeowners in the municipality in 2010.

Appendix E. Housing price indices

Description of the method

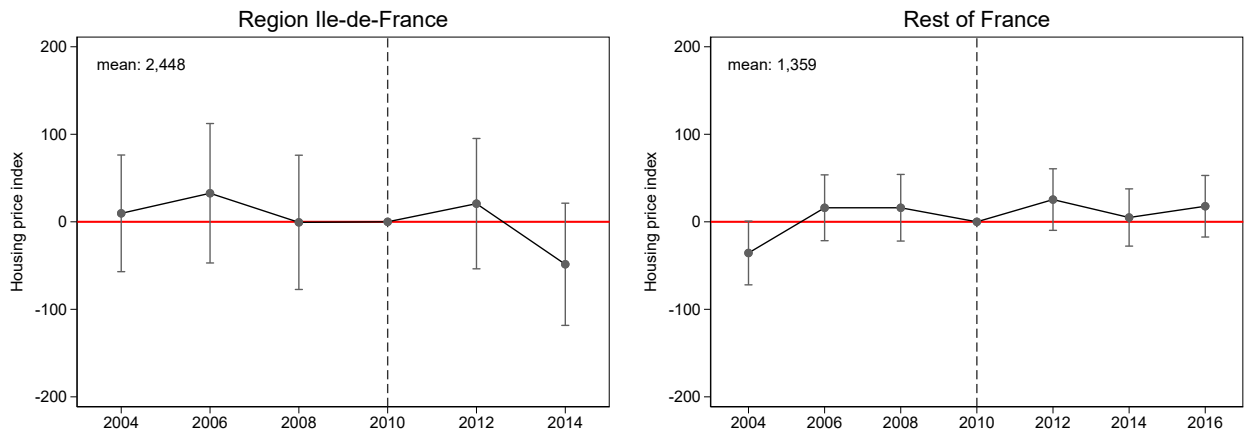
Following Combes et al. (2018), I built the housing price indices using official transaction records. These data come from an annual census conducted by the regional notary associations, which report the transactions of non-new dwellings. Although reporting is voluntary, it covers about 65 percent of all transactions. I built the indices separately for the Parisian region of Île-de-France and for the rest of France, as the two databases come from two distinct notary associations and do not use the same definitions for the dwellings' characteristics. The data are made available by the Ministry of Sustainable Development for every even year since 2000. They are available until 2014 for Île-de-France and 2016 for the rest of France.

I first run hedonic regressions, separately for houses and apartments. Following Gouriéroux and Laferrère (2009)'s and Musiedlak and Vignolles (2016)'s guidelines, I excluded some outliers from the transaction databases. Next, I regressed the log of the price per square meter of the dwelling on several characteristics. To build the baseline index, I regressed the log of the price per square meter on indicator variables for the quarter of the transaction and the construction period (Combes et al., 2018). I built a second index (which I refer to as the "augmented index") using additional characteristics. For houses, I added the floorspace, the size of the land, the number of rooms, bathrooms and floors; and whether the house has parking. For apartments, I added the floorspace, the floor on which it is located, the number of rooms and bathrooms, whether the building has an elevator, and whether the apartment has parking and a cellar.

While the price of the transaction is never missing, the floorspace is missing for 10 percent of the apartments and 36 percent of the houses. To compute the price per square meter, I replaced the missing floorspace by the average floorspace of an apartment or a house with the same number of rooms in the same département. Results are unchanged if I instead drop the transactions for which the floorspace is missing. Regarding the right-hand variables (the dwelling's characteristics), I replaced the missing values by the average of that variable and added an indicator variable equal to 1 if the variable was missing. I also centered all explanatory variables by subtracting the means and dividing by the standard errors.

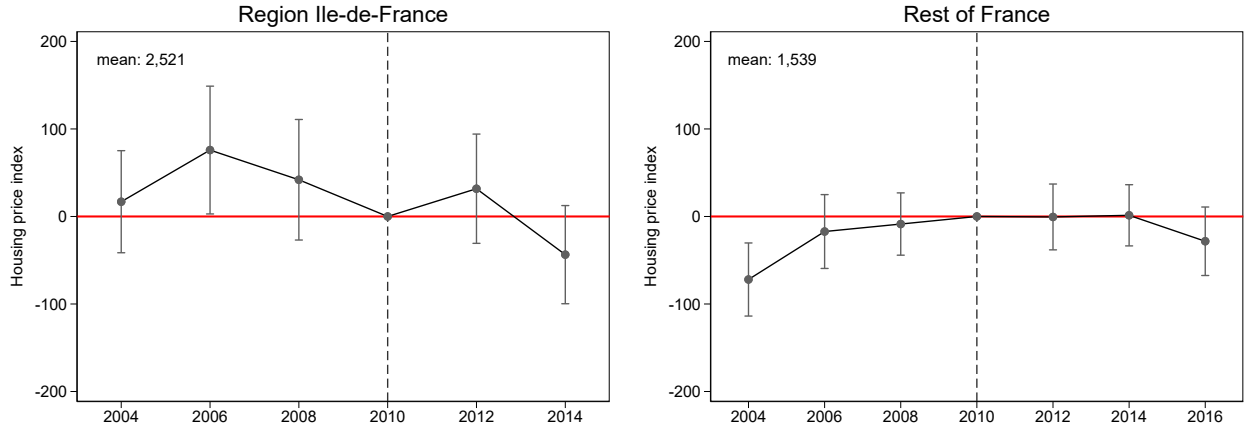
After running the hedonic regressions, I computed the mean of the residuals over both houses and apartments for each year and municipality separately, after having added the regression constant. Since the explanatory variables are centered, we can interpret the resulting indices as the price per square meter of a reference dwelling.

Figure E1: Impact on prices: Unbalanced panel



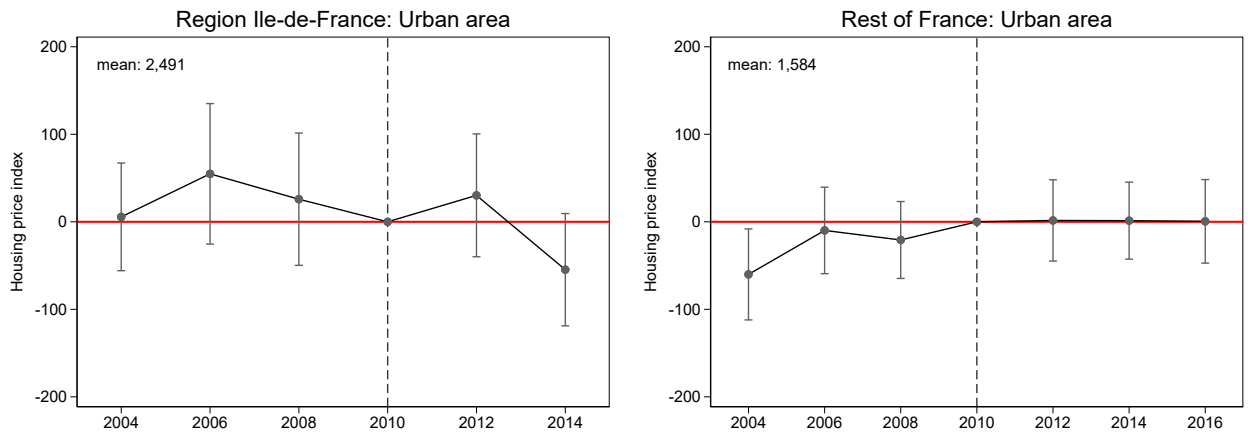
Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The outcome is the municipality housing price index giving the price per square meter of a reference dwelling. The sample includes all municipalities in which at least one housing transaction took place during the period studied. The graph on the left-hand side includes only municipalities in the Parisian region of Île-de-France, while the graph on the right-hand side includes all the other municipalities. On each graph, the average value of the price index in the treatment group before 2010 is displayed on the top left corner.

Figure E2: Impact on prices: Augmented index



Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The outcome is the municipality housing price index giving the price per square meter of a reference dwelling. For these graphs, I use an alternative version of the indices for which I include additional apartment and house characteristics in the hedonic regressions. The sample includes only municipalities in which at least one housing transaction took place each even year over the period studied. The graph on the left-hand side includes only municipalities in the Parisian region of Île-de-France, while the graph on the right-hand side includes all the other municipalities. On each graph, the average value of the price index in the treatment group before 2010 is displayed on the top left corner.

Figure E3: Impact on prices: Urban area



Notes: These graphs plot the estimates and 95-percent-confidence intervals from the leads-and-lags regression. The outcome is the municipality housing price index giving the price per square meter of a reference dwelling. The sample includes only municipalities that are part of an urban area and in which at least one housing transaction took place each even year over the period studied. The graph on the left-hand side includes only municipalities in the Parisian region of Île-de-France, while the graph on the right-hand side includes all the other municipalities. On each graph, the average value of the price index in the treatment group before 2010 is displayed on the top left corner.

Appendix F. Data description

Municipalities' characteristics. The municipality characteristics I use for the descriptive statistics and for the heterogeneity analysis come from various sources. Municipalities' socio-demographic characteristics are available from censuses conducted by the National Institute of Statistics and Economic Studies (INSEE). INSEE also provides a classification of rural and urban municipalities as well as the composition of French urban areas. Municipalities' geographical coordinates are obtained from the National Geographic Institute (IGN). Household taxable income data aggregated at the municipality level are provided by the Ministry of Finance and extracted from income tax declarations. Municipal and presidential electoral results are obtained from the Ministry of the Interior and mayors' characteristics from the National Directory of Elected Officials (RNE). ICs' municipal composition and the list of mountain-zone municipalities are provided by the General Directorate of Local Authorities (DGCL).

Finally, data on municipalities' land use come from Combes et al. (2021). The share of built land is computed considering all construction, whereas the average height and floor-to-area ratio (FAR) are computed considering only housing construction. To measure the FAR stringency, I follow Combes et al. (2021) and take the 30th percentile of the distribution of realized FARs of all housing buildings in the municipality. I also show the statistics using the median.

Housing building permits. Housing building permits data come from the Ministry of Sustainable Development (sit@del2 database). The dataset contains the number of housing building permits delivered every year in each municipality over 1999–2018.[45] More precisely, it provides the number of housing units allowed for construction. Hence, if a building of 10 apartments was approved, the dataset registers 10 authorized housing units, even if only one permit was delivered. The database also indicates whether the unit is a house (single-family home) or an apartment (unit part of a multiple-family home) and whether the construction takes place on empty land or as an extension to an existing building.

Housing prices. Building on Combes et al. (2018) and following INSEE's guidelines (Gouriéroux and Laferrère, 2009; Musiedlak and Vignolles, 2016), I measure housing prices at the municipal level using official transactions records of non-new dwellings and adjusting for housing characteristics. The data are available from the Ministry of Sustainable Development for every even year since 2000, separately for the Parisian region of Île-de-France and for the rest of France. Transaction data are available until 2014 (resp. 2016) for Île-de-France (resp. the rest of the country). To build municipal housing price indices, I regress separately for each year the log of the price per square meter on the characteristics of the dwelling. I then compute the indices as the average of

[45] I focus on ordinary housing, excluding residences providing particular services, such as medical or retirement residences.

the residuals for each municipality and year, after adding the regression constant. Since I center the explanatory variables, the resulting indices can be interpreted as the price per square meter of a reference dwelling. Appendix E provides further details on the construction of the indices.

Economic activity. I measure firm creation using the number of new establishments created in a municipality during a given year. The data are obtained from INSEE and cover 2007–2018. As an alternative proxy for economic activity, I use the total wages of the municipality's residents over 2002–2016, obtained from the annual declaration of social data (DADS).

Local public services. I gathered novel data on two local public services commonly transferred to the IC level: daycare and municipal libraries. Daycare data come from the Family Allowance Agency (CAF), which gives the number of daycare facilities and the total number of daycare spots available in each municipality for every year over 2007–2018. Data on the location of municipal libraries come from a yearly survey run by the Ministry of Culture from 2009 to 2018. While all French départements ended up participating in the survey, only seven were surveyed in 2009. The analysis of public libraries is thus restricted to those départements.

I also gathered data on public schools, a largely centralized public service which is unlikely to be directly affected by intermunicipal cooperation. I obtained data on the number of preschool (*maternelle*) and primary schools in each municipality over 2009–2018 from the Ministry of Education. Data on higher-level schools (including secondary schools, high-schools, and universities) come from the Central Education Database (BCS) made available by the Quetelet network for 2004–2018.

Public transport. Information on municipalities' access to public transport comes from the Center for Studies and Expertise on Risk, the Environment, Mobility and Development (CEREMA) which surveys public transport operators every year in all French regions except Île-de-France. Operators report the list of municipalities served by their network. I thus know for each year which municipalities have access to a public transport network. The database includes all regular public transport networks that are managed at the municipal or intermunicipal level, accounting for about half of French public transport networks and 90 percent of all public transit trips (the other networks are managed at the departmental or regional level). Specific and temporary transports such as school bus services or transport services during market days are not included in the database. Data are available for every year until 2017, when the survey ended.

Fiscal revenues. I use municipalities' and ICs' annual accounts that are made publicly available by the French Ministry of Economy and Finance (DGFiP) from 2010 to 2018. For years prior to 2010, the data come from Fabre (2021).