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Corentin Trevien

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INSTITUT D'ÉTUDES POLITIQUES DE PARIS
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Doctorat en Sciences économiques

Four Essays in Empirical Urban Economics

Evaluation of French Regional Policies

Corentin Trevien

Thèse dirigée par Thierry Mayer

Soutenue le 19 mai 2016

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Abstract

Chapter 1: Introduction. This chapter describes the three main questions of the thesis: the link between railroads and the spatial distribution of jobs, the elasticity of land supply and the spatial segregation in cities, especially issues related to spatial mismatch and sorting based on social status. Besides, this introductory chapter details the empirical evaluation methods used in the thesis.

Chapter 2 – The Impacts of Urban Public Transportation: Evidence from the Paris Region. This chapter was co-authored with Thierry Mayer. It estimates the impact of the Regional Express Rail (here after RER) on the distribution of jobs and firms within the Paris Region. Evaluating the impact of transport infrastructure always meets a major challenge since rail lines are not randomly located. The opening and progressive extension of the RER between the 1970s and the 2000s offers a natural experiment to address this usual endogeneity issue.

In the outskirts of the metropolitan area, on which this paper focuses, RER development was mainly an improvement of the existing suburban rail infrastructure. We take advantage of this feature to provide two identification strategies focusing on two samples of municipalities initially connected to the suburban train network. In some of them, the suburban train station has been upgraded and integrated in the RER network, these municipalities constitute the treatment group. Conversely, the control group includes municipalities that did not benefit from this enhancement. Each identification strategy exploits a specific subsample within which the treatment can be reasonably considered as exogenous.

The first identification strategy relies on the fact that the RER network specifically aimed at connecting the city of Paris, namely the historical city center of the metropolitan area, to new subcenters in the outskirts, namely the two international airports, the business district of *La Défense* and five new towns. By doing so, the RER network also happens to serve intermediate municipalities, located in-between previously stated economic centers. We use these municipalities as treatment group, given that they are not explicitly targeted by the policy and benefit fortuitously from the RER system.

The second identification strategy is based on the important differences between the 1965 masterplan that launched the RER project and the actual network. Instead of the ambitious project that envisioned the construction of hundreds kilometers of new rail tracks across the Paris region, the actual RER system was built in a more economical way by, as stated before, upgrading the existing suburban train network. This program change appears to be due to a combination of tougher budgetary

constraints and a political changeover. Using this dissimilarities, we include in the treatment group municipalities that are linked to the RER network but that should not have been, according to the 1965 masterplan.

Afterwards, we estimate the effect of the treatment with a simple difference-in-differences method and we add some control variables to take into account some initial differences between control and treatment groups. Running the model on the time period preceding the RER construction yields no significant differences between the two groups with both strategies, which gives support to our identification strategy. The last important point of our empirical method is the computation of a continuous treatment variable, namely the variation in the mean travel time to the city center (i.e. the City of Paris). Such a precise measure of the improvement in the transport service is necessary, given that the RER network is not a fully new infrastructure but an evolution of the suburban rail system.

We find that the employment increases by 5.9% when travel time to the city center decreases by one minute between 1975 and 1990. It corresponds to the creation or the relocation of 800 jobs in each municipality connected to the RER network, compared to the level that would have prevailed in the absence of this transport system improvement. Assuming our estimate can be extended to the whole suburbs of the Paris region, it would imply that the RER caused the location of 66,200 jobs in connected municipalities. By way of comparison, it represents a tenth of the total number of jobs decentralized from the city center to the outskirts.¹

The RER effect is explained by a better access to the metropolitan labor market. Workers live further from their workplace in treated municipalities. Besides, we find a positive impact on firm location, except on those that are foreign-owned, probably more responsive to the improvement of public transportation. We notice no clear shift in the distribution of employment across industries. On the opposite, there may be no effect of the RER on the overall population growth. Our results suggest however that the commissioning of the RER may have increased the competition for land since highly-skilled households are more likely to locate in the vicinity of RER stations.

Chapter 3 – Is High-Speed Rail a Business Communication Technology? Evidence from French Multi-Plant Businesses. This chapter was co-authored with Pauline Charnoz and Claire Lelarge. We document the impact of travel time between affiliates and headquarters of geographically dispersed corporate groups on the management of such business organizations. Theory suggests that the easier circulation of managers might facilitate the transmission of information between production plants and headquarters. This would thus foster the growth and the functional specialization on production activities at remote affiliates and decrease operational costs at the group level. We test these predictions on French corporate groups, using the expansion of the High Speed Rail (hereafter HSR) network as an arguably exogenous shock on internal travel times.

The French high-speed rail service has been introduced in 1981 and is still ex-

¹Employment growth in treated municipalities may also be due to relocation of firms coming from other suburban areas and other agglomerations, as well as to the creation of economic activities that would not have existed otherwise.

panding. It combines the construction of new high-speed railway lines, the modernization of regular railway lines, the introduction of new business practices and the commissioning of new high-speed trains. The first high speed line linked Paris to Lyon and Marseille, which are the three main French cities. The network has been then expanded toward many other French cities and regions: the Atlantic coast in 1989-1990, Lille in 1993, London in 1994, Strasbourg and Frankfurt in 2007.

Our main identifications strategy takes advantage of the dyadic nature of the data, meaning affiliate-headquarters couples. Using a large set of fixed effects, we quantify the link between the travel time to the headquarters and the employment structure of the affiliate. To assess the effect of the variation of travel time, which is clearly less endogenous than its level, we include affiliate fixed effects. We also add commuting zone \times time fixed effects. This takes into account that the location of HSR lines may be driven by the local economic situation and that the commissioning of a high-speed line could boost the local growth. In the end, we compare affiliates located in the same commuting zones, controlled by distant headquarters located in different places. Consequently, affiliates are not affected in the same extent by the treatment and experience different changes in the travel time required to reach the head office.

In addition, we propose an alternative identification strategy based on the 1991 high-speed rail masterplan. Back then, the Government envisioned a very ambitious high-speed system and ranked each projected line, according to its expected profitability. In practice, this ranking was not fully followed to select projects to be carried out. Taking advantage of this setting, we run our model keeping only affiliate-headquarters couples that should have been affected by the 1991 masterplan, after excluding all unrealized projects which did not reach the profitability level of the least profitable realized line. We argue that this reduced control group is more similar to our treatment group, and thus limits the potential endogeneity problem.

For data availability reasons, we focus on the 1993-2011 period. Results are more pronounced in the service industries, where information to be transmitted is softer. We estimate that HSR induced the creation of one production job for the average affiliate in these industries (against 0.2 job in retail, trade or manufacturing industries), and the shift of around one managerial job from affiliate to headquarters. These results are robust to alternative identification strategies addressing the problem of the endogenous placement of the HSR infrastructure. We find similar estimates focusing on affiliates and headquarters only partially linked by high speed tracks. We also highlight a similar impact using the strategy based upon the 1991 high-speed rail masterplan. At the group level, descriptive regressions suggest that the impact on the operational profit margin is around 0.5 percentage points in most industries.

Chapter 4 – A Long-Term Evaluation of Enterprise Zones: The First Generation of the “*Zones Franches Urbaines*”. This chapter was co-authored with Pauline Givord and Simon Quantin. It evaluates the impacts of the French enterprise zone initiative (“*Zones Franches Urbaine*”, ZFU hereafter) on firm location and local employment. This program grants very generous tax breaks to small

and medium size firms which accept to locate in one of the 44 selected deprived neighborhoods. The rationale behind this policy is to bring job opportunities to unemployed people living in disadvantaged areas. In contrast to previous works which mainly evaluate the impact of this policy after 2004, we focus on the first generation, implemented in 1997 in France. It allows us to follow the effects of the program over a period of nearly two decades, which is an important contribution of this chapter.

Our identification strategy relies on two exogenous features of the zone selection process. First, ZFU designation was supposed to be based on a deprivation index summarizing the socio-economic characteristics of eligible areas. In facts, the index had been only partially followed. Some very disadvantaged areas should have been involved in the program given their level of deprivation but were left apart. Such zones offer a good control group and have been widely used in previous evaluation works of the ZFU program. Of course, this index still influences the probability of ZFU selection, even if not in deterministic way. We use a matching method to take into account differences between control and treatment groups.

The first ZFU wave provides another useful source of exogeneity, since program eligibility is limited to zones inhabited by more than 10,000 people. Unlike the deprivation index, this second criterion have been almost strictly followed. Given that eligible zones were delineated years before the implementation of the policy, it is unlikely that this population threshold could have been manipulated to influence the program selection process. We compute a specific propensity score that takes into account the two selection criteria. We also present a RDD regression as a robustness test, using only the population threshold criterion.

Our results validate a strong and positive impact of the ZFU program on both employment and plant location, robust to several identification strategies. After five years, the whole caused the location of 11,000 plants, employing 50,500 workers. This corresponds to 250 new firms and 1,400 new jobs in each zone. After 2002, meaning beyond the first five years of the program, the number of plants located in ZFU no longer increases, due to a higher rate of business closure. This questions the ability of the policy to initiate a self-sustainable economic development in deprived neighborhoods. We also find that the treatment impact is stronger on firms belonging to “footloose” industries (for example business services) and on firm relocation, which evidences a clear windfall effect.

The policy seems to fall short of its main goal, namely helping unemployed people plagued by urban segregation to find a job, because of insufficient targeting. In fact, the treatment effect is not higher for unskilled workers and previously unemployment people and is even weaker for residents of municipalities in which ZFU are located. Besides, the impact of program is smaller on firms depending on the local demand (for example trade).

Finally, our findings are much larger than previous results on the French case. We argue this difference is due to the specific context of the first wave of the program: land availability was greater compared to zones selected for subsequent generation; the level of payroll tax was higher at the national level, strengthening the comparative fiscal advantage of enterprise zones; the “local employment clause”, which requires a minimum share of local residents in the workforce, was poorly enforced

before 2002, which could have allowed firms to get around the shortage in the local supply of skilled workers.

Chapter 5 – The Impact of Housing Subsidies on the Rental Sector: the French Example. This chapter was co-authored with Céline Grislain-Letrémy. It evaluates the effect of subsidies on prices and quantities in the housing market. Housing allowance to tenants is a main tool for housing policy in France and accounted for 0.8% of the public spending in 2014. It aims to limit the budget share of housing for eligible tenants and to improve their housing conditions for a given budget share. Despite the increasing spending allocated to housing subsidies since the end of the 1970s, the proportion of household expenditure that low-income tenants devote to rent has continued to increase. This policy may have missed its goal due to the adverse effect of housing subsidies on the level of private rents, empirically proven in the United-States and in several European countries. This phenomenon is the consequence of a subsidized demand in the context of an inelastic housing supply.

The contribution of this paper is to provide a new assessment of the upward effect of housing subsidies on rents, using an identification strategy based on the geographical variation in the amount of the allowance. In fact, the housing subsidies are 15 to 40 euros higher per month in cities with more than 100,000 inhabitants and some other smaller metropolitan areas with a specific housing market,² all other things being equal. Zone designation cannot therefore be considered as exogenous, when studying the level of rents. However, the population threshold, which has not been updated after the delineation of the three zones in the late 1970s, offers a suitable instrumental variable for treatment evaluation.

We assess the impact of housing subsidies on price, quality and quantity in the private rental sector from the early 1980s to the early 2010s. We show that housing subsidies had an upward impact on rents in the 1990s and the 2000s. Rents are 4% higher in the second zone, namely 20 euros per month, where housing subsidies are higher. This result is robust to several tests: the effect is higher when the tenant benefits from the allowance; there is no similar discontinuity in the level of rents depending on the population of the metropolitan area; the impact is insignificant in the 1980s, when the housing benefits were lower.

Higher subsidies seem to have almost no effect on housing quality, meaning that higher rents are not explained by bigger or more comfortable dwellings in treated areas. Finally, we provide evidence of the inelasticity of rental housing supply in France. We notice that the impact of housing subsidies on the rents is significant only in metropolitan areas with a higher population growth. Additionally, we find no difference in the size and characteristics of the private rental sector, in relation to the level of housing subsidies. The only exception to this statement is that the proportion of one-room dwellings in the rental housing stock, which are likely to be occupied by allowance recipients, slightly increases where subsidies are higher.

²Especially tight, such as costal or border zones, or especially depressed, such as areas affected by industrial decline.

Chapter 1

Introduction

This thesis raises three research questions, that are addressed in the four composite chapters. First, chapters 2 and 3, on transportation, take up the question of the impact of railroads on the spatial distribution of jobs, in urban and regional contexts. Second, chapters 2, 4 and 5 (respectively on RER, ZFU and housing subsidy) highlight the differential elasticity of commercial and housing land supply. Third, chapters 2 and 4 question the characteristics of spatial segregation in cities, on the specific points of spatial mismatch and sorting based on social status. In this introduction, I also describe the variety of empirical methods used in this thesis, which leads me to discuss their limits in quantifying displacement effects.

1.1 Rail and the spatial distribution of jobs

Both chapters 2 and 3 focus on passenger rail infrastructure, within Paris metropolitan region in the first case and between French cities in the second case, and highlight interesting facts on the spatial distribution of employment. Unlike the majority of transport infrastructure, RER and HSR lines are mostly served by passenger trains¹. These framings allow to disentangle the differential impact of transportation on people and goods. In the chapter 2, we focus on commuting patterns. In the chapter 3, we study the impact of long distance transportation costs on the circulation of information.

In chapter 2, we notice a clear job decentralization phenomenon in the Paris region between the 1960s and the 1990s. As similarly described by Glaeser and Kahn (2004) for the United-States, we find that job decentralization occurred after population decentralization. We also highlight that jobs remain more concentrated in the city centers than households and that car ownership probably played a major role. Indeed, car ownership widely expand during this period. In 1990, 77% of French households owned at least one automobile, up from 21% in 1953 (Méot, 2009). Additionally, in the same time, the share of employees working locally seems

¹In the case of the French HSR, four trains are dedicated to mail shipment, which is marginal compared to the 500 passenger trains operating on the high speed rail network. In the case of RER, the existing tracks are partially used by freight trains before and after the development of the RER network, but this is not case of the new tracks, which caused the most significant improvements in the mass transit system.

to have fallen, suggesting that firms moving to the outskirts do not seek to follow workers increasingly living away from the city center.

Chapter 2 highlights the important role of public transportation in job decentralization. Firms face an intricate trade-off when considering the opportunity of relocating from the city center to the outskirts (Fujita and Ogawa, 1982). While the land is cheaper in the suburbs, agglomeration spillovers may be weaker because of more distant interactions with other firms. Commuting costs also play a role, considering that firm should compensate for longer commuting trip by paying higher wages. One could argue that moving to the outskirts would reduce commuting costs as population is less concentrated in city centers than firms. In contrast to this idea, Duranton and Puga (2015) document the existence of “wasteful” commuting patterns, namely the fact that workers do not necessarily commute to the closest workplaces. This occurs due to the presence of preference for specific amenities, location choice of two-earner couples or costs of a move when changing job.

We find that the RER modifies this trade-off. First, the average commuting distance of workers increases in RER municipalities, which confirms a diminution of commuting costs that allows firms to locate further from worker’s home. Second, manufacturing firms did not locate more frequently in the vicinity of a RER station than other firms. This is not in line with the results found in the United-States for highways as reported Duranton and Puga (2015). This probably reflects that highway proximity decreases transportation costs of goods, while passenger rail does obviously not.

Chapter 3, on HSR, is also related to the question of employment distribution between regions, as multi-plan businesses employ a large share of the workforce. In the United-States, Lindley and Machin (2014) notice rising spatial inequalities in skill distribution. Indeed, between 1980 and 2010, the supply in high-skilled workers rose faster in places where it was initially higher. They also notice a stronger low-skilled employment growth in places where highly educated people are increasingly concentrated. This is the spatial representation of the well-known job polarization phenomenon (Autor et al., 2006). This means that the demand in both high-skilled workers (professional and managerial) and low-skilled workers (personal services) increases, while the demand in medium-skilled workers (routine office jobs) drops, also resulting in a polarization of wages. According to Autor et al. (2013), job polarization has been mainly caused by a skilled-biased technological change (and not by job offshoring). The development of information and communication technologies lead to the automation of routine office jobs, that were previously occupied by medium-skilled workers. On the opposite, low-skilled tasks in personal services cannot be done by computers.

For France, similar results hold. The demand for medium-skilled workers declined (Goos et al., 2009), due to skilled-biased technological change which is nevertheless not associated with an increase in wage inequalities (Charnoz et al., 2014). Finally, Charnoz and Orand (2016) confirm a growing spatial polarization, explained by the automation of medium-skilled occupations.

Our work on HSR proposes an alternative mechanism to explain the spatial concentration of high-skilled workers. Our argumentation is based on the spatial organization of firms and, more precisely, on their ability to separate headquarters

from production activities. We follow Duranton and Puga (2005), which specify the trade-off between single-location versus multiple-location organization. In fact, if headquarters and production activities are integrated in the same place, management costs are lowered. However, firms may gain to separate their activities if cities offer different opportunities, it would then be more effective to locate headquarters in places with a large variety of business services (Davis and Henderson, 2008) and production plants close to intermediate good suppliers. At the equilibrium, headquarters end up being concentrated in few large cities where they share business services suppliers. Given that agglomeration economies are weaker in production activities, firms choose to split among different locations. Consequently, manufacturing plants are placed in smaller cities, with low congestion costs and specialized in production.

On this question, the contribution of the HSR chapter is to show that lower transportation costs facilitate remote management. Given that headquarters are concentrated in big cities (and especially Paris), HSR may lead to a specialization of affiliate in production tasks and reinforce the concentration of skilled workers in big cities.

1.2 Supply of land

The various chapters bring to light some specific topics related to the issue of land use and land supply. Chapter 5 directly highlights the capitalization of housing subsidies in rents and the inelasticity of housing supply. This capitalization effect has been already proved in the French case on both rents (Fack, 2006, Laferrère and Blanc, 2004) and housing prices (Bono and Trannoy, 2012). Similar results have been also found in the UK (Gibbons and Manning, 2006) and in the US (Hilber and Turner, 2013, Susin, 2002). Furthermore, the very low impact of RER infrastructure on population growth, pointed out by chapter 2, is striking. Indeed, the Alonso monocentric model predicts that a drop in commuting costs should result in a higher population density in the outskirts and a lower density in the city center. We also show that high-educated people move more frequently nearby RER stations, suggesting a capitalization of the better accessibility in housing prices. Besides, the positive and large impact of RER on employment suggests a different elasticity of commercial and housing land supply. Finally, the chapter 4 highlights a stronger impact of tax cuts on firm location where the land is more available. This is in line with the results of Poulhès (2015). She shows that subsidies granted to firms located in ZFU were partially capitalized in commercial building prices but to a lesser extent where land was more available before the policy implementation.

The central issue is therefore to understand whether low land supply is due to a lack of available land or to restrictions in land use. In the American case, there is a strong empirical link between the low elasticity of housing supply and land use regulation. Glaeser and Ward (2009) show that the simultaneous rise in home prices and decrease in dwelling construction in the Greater Boston is not due to a lack of land but to stricter land use policies. More directly related to housing policy impacts, Hilber and Turner (2013) highlight that the impact of home-owner subsidies is closely linked to land use regulation. Mortgage interest deduction raises the share

of home-owners where land use regulation is lax while it should be capitalized in housing prices where land use regulation is strict.

The assumption was made that homeowners should be strong supporters of land use regulation, in order to protect the value of their housing by preserving the local amenities. However, empirical evidence are less clear (Gyourko and Molloy, 2015). Considering that the homeownership rate is slightly lower in France (58%) than in the United-States (65%), homeownership impact on land use regulation could thus apply to France.

Another mechanism is that small jurisdictions are more likely to restrict land use. Residents may benefit from positive externalities of urban development in neighboring jurisdictions without suffering from negative externalities of new residents in their own place of living. This mechanism could also be valid for France given that municipalities are especially small and play a prominent role in land use regulation. In addition to this, Verdugo (2011) finds that social housing supply in a given metropolitan area decreases with its fractionalization in independent municipalities.

Gyourko and Molloy (2015) also states that land use regulation could be an attempt to sort households across municipalities. We lack evidence to prove the link between residential sorting and restrictive land use regulation in the French case. Nevertheless, the controversial sociology work of Pinçon and Pinçon-Charlot (2007) suggests that the rich would lobby to limit a greater social diversity in wealthy neighborhood. They argue that some of the richer municipalities in the Paris Region prefer to pay the penalty imposed by the SRU law in case of shortage of social housing,² in order to prevent low income households from settling down. Gobillon and Vignolles (2016) however show the effectiveness of such intensives. The implementation of the SRU law caused a faster growth of social housing construction, especially in municipalities where the initial stock was scarce.

Lastly, land use regulation could be linked to fiscal situation, local authorities are more likely to impose strict land use regulation if they do not need to increase their fiscal income by attracting new residents. This mechanism could fit the French case as local authorities do not depend substantially from household taxes. Conversely, before recent reforms, the corporate tax was considered as a key factor of local authority income. Even if the median resident tax revenue was higher,³ the dispersion of corporate tax revenue was greater.⁴ This heterogeneity could perhaps signify that, for a given municipality, attracting firms would be a better way to increase financial resources. In fact, this could partially explain why there was no population growth in municipalities linked to the RER network while employment significantly increased. It was fiscally more advantageous to attract firms rather than residents.

²According to the 2000 SRU law (*Loi relative à la solidarité et au renouvellement urbains*) urban municipalities should reach a 20 percent share of social housing in the total housing stock and must pay a penalty if they do not.

³In 2002 in the Paris region, the medium resident tax revenue per inhabitant (*taxe d'habitation*) was 106 euros to 62 euros for local corporate tax (*taxe professionnelle*), across municipalities.

⁴Concerning corporate tax, the first quantile is equal to 25 euros and the third quantile is equal 163 euros while respectively 71 and 152 euros for resident tax. Same results hold for the total tax revenue.

1.3 Spatial segregation in cities

Finally, this thesis also examines the effect of urban policies on residential segregation. Chapters 2 and 4, on RER and ZFU program, provide insights on two different questions: spatial mismatch and income sorting.

As presented in the chapter 4, the ultimate goal of the French enterprise zone program is to help unemployed workers living in deprived neighborhoods to find a job. The rationale of this policy is a spatial mismatch hypothesis. Residents of deprived neighborhoods are unemployed because there are not enough job opportunities close by. Empirical evidence supports this assessment for the Paris region (Gobillon and Selod, 2007), plausibly explained by redlining and weak social networks.⁵

While we find a very impressive impact of ZFU on firm location, the effect on unemployed resident is much lower, as confirmed by Gobillon et al. (2011) and Charnoz (2015). The program suffers from a clear targeting problem. As stated before, the treatment effect is similar on skilled and unskilled workers, on workers and unemployment people, and even lower on ZFU municipalities' residents compared to other municipalities' residents. Manning and Petrongolo (2011) offer an interesting explanation for this dilution effect, they show that in Britain: "as labor markets overlap, a local stimulus or transport improvements have modest effects on local outcomes, because ripple effects in job applications dilute their impact across a series of overlapping market". Even more problematic, the ZFU impact seems weaker from the moment the "local employment clause", requiring a minimum share of local residents in the workforce, was actually enforced. It may signify that firms located in deprived areas encounter a skill mismatch issue, due to the fact that unemployed persons living nearby do not have appropriate qualifications to fill the available positions.

On question of spatial sorting of population based on income or skills, chapter 4 offers no evidence. However, it should be mentioned that Charnoz (2015) finds that the ZFU policy caused an increase in the share of highly-educated residents. This underlines that impacts of urban policies, even aiming at helping the poor, can often be weakened by the migration of better-off people, wishing to benefit from the policy advantages.

Chapter 2 gives support to the idea that public transportation infrastructures may induce spatial sorting based on social status, since our findings suggest that the proportion of highly-skilled residents increase in connected municipalities. Baum-Snow and Kahn (2000) also finds that the development new mass transit projects lead to higher housing prices in the vicinity of new stations. Having noted that wealthier households live in the city center in Paris while they live in the suburbs in Detroit, Brueckner et al. (1999) explain this difference by the presence of amenities (such as historical monuments, restaurants, museums) in the city center. Even if better-off households value more such amenities, they also have higher housing consumption and may thus be attracted by low housing price in the suburbs. In

⁵On the American case, Bayer et al. (2008) show the importance of very local social interactions on labor market outcomes, especially for low-skilled workers. Besides, L'Horty et al. (2012), using a testing method on three municipalities of the Paris region, confirm the spatial segregation hypothesis, but only for women.

Paris the first effect is stronger, which could explain why we find more high-skilled residents when the accessibility to city center is better. However, we find no clear evidence of a reduction in the share of poor households, as we should expect from this amenity based explanation. This could be explained by an alternative mechanism proposed by Glaeser et al. (2008). They argue that the income elasticity of the demand for land in the US is too low to fully explain why wealthier household are over represented in suburban areas. The reason why poor household do not move in the outskirts is that car-based mobility is too expansive. Consequently, they are constrained to stay in the city center, where public transportation is available.⁶ In the French context, we should also add to the picture the role of social housing which is overrepresented in the inner ring and strongly influence the location choices of poor households.

1.4 Empirical methods

The four chapters of this thesis are challenged by the issue of selection related endogeneity. The designation of beneficiaries is specific to each evaluated policy: ZFU are especially disadvantaged neighborhood, transportation infrastructures are never located independently of local economic prospects, subsidized households often occupy dwellings with specific characteristics. We implement a variety of empirical methods: difference-in-differences, regression on discontinuities, propensity score matching, fixed effect methods and instrumental variables.⁷

Regression discontinuity design, or instrumental variable methods using discontinuities, are used in chapters 4 and 5. Unlike many papers in empirical spatial economics, we do not compare areas located on one side or the other of a boundary (see for example Black, 1999). In the case of housing subsidies, we are limited by the availability of data, meaning that we do not have enough observations close to the border between the two zones. In addition, given that boundaries cross agglomerations, we cannot exclude general equilibrium effects of the treatment from either side. In the case of the ZFU program, using the areas surrounding the Enterprise zone would lead to overestimate the treatment, due to displacement effects (Givord et al., 2013). In both chapters, we therefore use non spatial thresholds, which limit the potential contamination effect of the control group by the treatment. An other important issue of the regression on discontinuity method is to ensure the threshold has not been manipulated. In our cases, thresholds are based on “round numbers”, less likely to be endogenous. Even though, there could be a concern for potential endogenous ZFU delineation, but the contours of the zones were fixed several years before the treatment implementation.

In both chapters 2 and 3, on transportation, identification strategies are based on past masterplans, similarly to Duranton and Turner (2012) or Donaldson (2016).

⁶This explanation could also be valid in France. Even if a four households on five possess at least one car, only a quarter possesses two cars or more, that are essential for a two-earner couples, according to census results.

⁷Note that the instrumental variable method is not *per se* used, but chapters 2 and 3 rely on subsampling methods that are very similar to the existing literature using past transportation plans as instrumental variables.

More precisely, we use past projects to choose a subsample among which treatment can be considered as (more) exogenous. We use the French 1991 masterplan for high-speed rail. Considering that, among feasible abandoned projects, line selection has not been driven by economic reasons, we restrict our control group to couples of affiliate-headquarters that should have been connected faster by rail, according to the 1991 plan. In the RER chapter, we use the deep differences between then 1965 RER master plan and the actual network. They both link the economic centers of the Paris region, that is to say places intentionally connected by the infrastructure, but they use different itineraries to reach the same points, because of budgetary constrains. Using this setting, we keep only stations that should not have been connected to the RER network, according to the initial plan, but that actually are.

Related to the general question of the external validity of our findings, a common problem of interpretation is whether urban public policies create new economic activities or simply reorganize the spatial distribution of jobs and people, as pointed at by Redding and Turner (2015) for transportation infrastructures and Neumark and Simpson (2015) for place-based policies. Clearly, the identification strategies of all chapters aim primarily to quantify the differences between control and treatment groups that are attributable to the treatment.

These methods are ill-equipped to measure general equilibrium effects, except possibly in chapter 5, on housing subsidies. In that latter case, we are less concerned by potential displacement effects. Even if inter-regional migrations are supposed to equalize utilities across locations, we think that it is unlikely to happen in our case because the difference in housing subsidies between zones is too low (around 15 to 40 euros per month), and certainly far below the fixed costs associated with a move.

In chapter 4, we show that the impact of tax cuts is especially strong on firm relocations, which sometimes happen over long distances. The estimated effect is also high on firm creation, but we cannot rule out that such firms would have been created elsewhere in the absence of the treatment. Consequently, our results clearly indicate that a certain part of the enterprise zone effect is due to a displacement effect, but we are unable to quantify this proportion.

The case of the chapter 2 is even more undetermined concerning the respective share of growth and displacement due to urban public transportation. We know that both phenomena are likely to occur, empirical papers on the United-States have for example shown that highways can simultaneously redistribute residents within a city (Baum-Snow, 2007) and increase its total population (Duranton and Turner, 2012). In our work, we do not know if the RER effect is either due to the attraction of new businesses in the Paris region or to a decentralization from the city center to the outskirts or even to a displacement effect from control to treatment group. We can only assert there is a difference between our two groups because of the treatment. Finally, in the chapter 3, we try to quantify the HSR effect on the whole enterprise, even if our identification strategy does not allow for a robust estimation at this level.

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Chapter 2

The Impacts of Urban Public Transportation: Evidence from the Paris Region

This chapter is cowritten with Thierry Mayer (Sciences Po, Banque de France, CEPII and CEPR).

2.1 Introduction

Urban public transit is considered as a key policy to ease urban congestion and promote environmentally-friendly transportation. In Europe as in the United States, multiple projects demonstrate the belief of policy makers in the efficiency of public spending for transportation. Mention may be made of the “Crossrail”¹ project in London, the “Grand Paris Express”² in France or the plan for high speed rail in California.³ Besides, spending for inland transport infrastructure is significant: 0.7 percent of the GDP in North America and 0.8 in Western Europe and even 0.9 in France (OECD, 2011). Empirical evaluations are needed to assess the return of costly infrastructure investments. Following this idea, this paper offers evidence of the way urban rail transit can shape urban development. To do so, we use the natural experiment offered by the improvement of the Paris commuter rail system from the 1970s to the 1990s. During this period, Paris metropolitan region spread and the population rose from 9.2 millions of inhabitants in 1968 to 11.5 millions of inhabitants in 2006 (INSEE, Census).

This growth was accompanied by the improvement of the commuter rail system and the commissioning of the so-called Regional Express Rail (RER thereafter). While this policy mainly led to the improvement of the existing network, it also

¹<http://www.crossrail.co.uk/>

²<http://www.societedugrandparis.fr/english-version>

³<http://www.hsr.ca.gov/>

encompassed the construction of new stations and lines. The improvement of the suburban rail system into a fast and high capacity network offers an interesting identification strategy to estimate the causal impact of public mass transit on firm, employment and population location across the metropolitan area. In fact, we estimate the effect of a one-minute reduction in travel time by public transport on various indicators at the municipality level.⁴ We implement a difference-in-differences design, using a continuous treatment variable (instead of a binary measure), on a specific subsample chosen to address the endogeneity bias (explained below). We find that the number of jobs grew by 5.9% in a municipality when travel time decreases by one minute on the 1975-1990 period. We obtain similar results for firm location. However, we find no robust impact on the whole population growth.

The literature gives support to the idea that transportation plays a key role in the economics of cities. There are both empirical and theoretical evidence that transport infrastructures influence the location of people and economic activities within cities, and also between cities (which is not covered in our work). The standard monocentric city model predicts that a decrease in transportation costs should increase the share of the population who lives in the suburbs (Alonso, 1960). According to this model, it will also cause a rise in the global size of the city due to lower congestion costs. Empirical results support this theoretical prediction. Baum-Snow (2007) studies the effect of highways on the shape of US cities. He shows that roads explain one third of the observed population displacement from city centers to the outskirts. Similar results hold for Spain (García-López et al., 2013). Duranton and Turner (2012) highlight the positive impact of highways on city growth: a 10% increase in the stock of highway causing a 1.5% rise in local employment in the US. However, in line with theoretical models of economic geography, Faber (2014) confirms that transportation could decrease economic activity in some cases. Indeed, he finds that highways caused a reduction in the GDP growth of peripheral counties in China.

Empirical studies on firm location choices also highlight a positive link with transport infrastructures. Coughlin and Segev (2000) show that highways foster foreign-owned manufacturing plant location in US counties. Holl (2004a,b) finds similar results in Portugal and Spain. Strauss-Kahn and Vives (2009) show that the proximity to an airport is a significant factor for headquarter relocation in the United States. More generally, transport infrastructure is positively associated with the productivity of cities. According to Fernald (1999), highway construction in the US increased the productivity of vehicle-intensive industries at a metropolitan level. The contribution of our work is to provide results for firms, employment and population, in order to assess the different impact of transportation depending on the type of economic agent.

Evaluations of public transportation highlight the fact that the development of mass transit has specific effects on cities, compared to other means of transportation. First, commuter rail systems help reduce air pollution in cities. Chen and Whalley (2012) show that the opening of the metro of Taipei reduced measured concentration in carbon monoxide by 5 to 15 percent. Second, rail influences the location of people and jobs in cities, in a different way than other means of trans-

⁴We will use the terms municipality and city interchangeably to design the administrative unit which is our unit of observation (more than 1300 of them in the Paris region).

portation. According to Baum-Snow and Kahn (2000), commuter rail investments cause a slight increase in the local value of properties in five major American cities. It also encourages switching from driving to using public transportation. Besides, Burchfield et al. (2006) show that cities, where public transportation was embedded in the initial urban development plan, are less sprawled than cities built for cars, because of higher commuting costs. Our results emphasize once again that European and American cities work differently (Brueckner et al., 1999). In the Parisian case, we find suggestive evidence of a gentrification effect of commuter train in the inner ring of Paris suburbs. Finally, considering the case of a major European city appears relevant as urban mass transit plays a bigger role in commuting than in Northern America. For example, only 5.3 percent of American workers use public transportation to commute⁵ (McKenzie and Rapino, 2011), while 13.3 percent of French workers do (François, 2010) and even 22.6 percent of Japanese commuters⁶ (Japan Census, 2010). Besides, car is prevalent in the US (90.0%), less prominent in France (72.3%) and even rarer in Japan (46.9%).

Because transportation infrastructures are not randomly located, evaluation of its impact faces up to a severe endogeneity issue. The consequence is that a naive evaluation, comparing directly connected to unconnected areas, would certainly be biased. In addition, intentions of policymakers are not clear; they may intend to connect either dynamic or deprived areas depending on the public policy goal at the time of decision. The sign of the endogeneity bias is thus undetermined. The literature offers some examples of identification strategies to address this issue. Such strategies, based on natural experiments or clever instruments, yield the causal impact of new infrastructures. Duranton and Turner (2012) evaluate the impact of the highway network in the United States on the local evolution of employment. They use an instrumental variable strategy, based upon the 1947 plan of the interstate highway system, partially based on military purposes, and on the 1898 network of railroads, to address the endogeneity of the location of highways in 1980. Michaels (2008) also uses the 1947 plan as an exogenous source of variation of roads for evaluating the impact on interstate trade. Donaldson (2013) shows that railway extensions in India led to a decrease in interregional trade costs and increased both incomes and trade. To do so, he uses a natural experiment provided by 40,000 km of planned lines which were never built for arguably exogenous reasons. Banerjee et al. (2012) find a moderate positive effect of transportation access on income growth in China. Their strategy relies on the fact that railroad lines were built in China to connect European concessions on the coast and inland historical cities in the 19th century. They argue that crossed areas, which were located in between these two sorts of cities were “quasi-randomly” linked to the railway network and can be compared to similar unconnected areas.

We provide two identification strategies to solve this endogeneity issue. The first method is inspired by the approach of Banerjee et al. (2012). The RER network was developed with the aim of connecting new economic subcenters to the historical

⁵Without people working at home.

⁶Workers and students over 15 years; public transportation includes company’s or school bus; two modes of transport can be given, in that case, users are shared between the two modes, on a half-half basis.

center of the city. These economic subcenters are located between 15 and 30 km away. Doing so, RER lines happen to cross areas which are located between the historical core and these subcenters. Such stations were “quasi-randomly” included in the RER enhancement program, as there was no “intention to treat” from the policy makers. They can therefore be compared to similar untreated stations to estimate the causal impact of railway improvement.

Our second identification, presented as a robustness test, is inspired by the method of Donaldson (2013), Duranton and Turner (2012), Michaels (2008). An urban plan has been presented in 1965 to improve the Paris suburban train system and envisioned the construction of hundreds kilometers of new lines in the outskirts. The actual RER network differs from this initial proposition, as its development mainly consisted of an upgrade of the existing suburban rail lines. This project modification occurred after a political change and was mainly driven by budgetary reasons. This incidental divergence from the initial project points out the exogenous selection process of some parts of the RER network. We thus use it as an alternative way to identify the causal impact of the treatment.

Lastly, data availability and precision is a key issue when trying to estimate the impact of improved transport systems. Gibbons et al. (2012) insist on the fact that, when networks are already dense, it is indeed not sufficient to observe the fact of being connected to the transportation system, as it should already be the case for a large range of locations, including untreated areas. On the contrary, a precise measure of the improvement of rail service quality is required, to assess to what extent a better public transport system affects the behavior of economic agents. Therefore, we do not use the simple fact of being connected to the RER as treatment. We develop a method to compute the average journey time by train across the Paris metropolitan region, from 1969 to 2006. Of all the lines in the RER network, some saw their travel time drastically reduced, thanks to substantial investment while transportation conditions were hardly improved for others.

This chapter is organized as follows. Section 2.2 relates the history of urban planning in Paris region. The second section explains our estimation strategy. The third section details the econometric model we use and section 2.5 presents the data used in estimation. The fifth section sets out the results and finally, section 2.7 concludes and discusses our findings.

2.2 The development of the Paris region

A massive development of the Paris region⁷ occurred during the second half of the 20th century. While the population rose from 6.6 millions in 1946 to 9.2 in 1968 and 10.5 in 2006, job and population dramatically decentralized from the city center to the outskirts of the city (see Table 2.1). The share of the population who lives in the City of Paris, which precisely corresponds to the historical center of the metropolitan area, decreased from 41% in 1946 to 28% in 1968 and 19% in 2006. Job location followed the same phenomenon but remained unsurprisingly more centralized. The proportion of workers who commutes to the city center dropped for both people

⁷Here, the administrative region Île-de-France.

Table 2.1: Distribution of population and employment in Paris region (Île-de-France) from 1946 to 2006

		<i>Total</i>	Percentage distribution			
			Paris	Suburbs		
				<15km	15-25km	<25km
Surface (sq. km)		<i>12,012</i>	1	5	10	84
Population	1946	<i>6,577,127</i>	41	38	10	11
	1968	<i>9,229,592</i>	28	43	17	12
	1990	<i>10,660,075</i>	20	38	22	20
	2006	<i>11,528,869</i>	19	37	22	22
Employment	1968	<i>4,209,536</i>	46	35	10	9
	1990	<i>5,062,338</i>	36	35	15	14
	2006	<i>5,497,598</i>	32	35	17	16

Sources: Population Census 1968-2006.

Table 2.2: Commuting pattern across Paris region (Île-de-France) in 1968 and 2006 (in percent)

Place of residence		Place of work				<i>Total</i>
		Paris	Suburbs			
			<15km	15-25km	>25km	
1968	Paris	87	11	1	1	<i>100</i>
	<15 km	31	65	2	1	<i>100</i>
	15-25 km	27	19	51	3	<i>100</i>
	>25 km	11	5	4	80	<i>100</i>
2006	Paris	68	24	5	3	<i>100</i>
	<15 km	29	60	8	3	<i>100</i>
	15-25 km	19	28	43	9	<i>100</i>
	>25 km	12	14	16	58	<i>100</i>

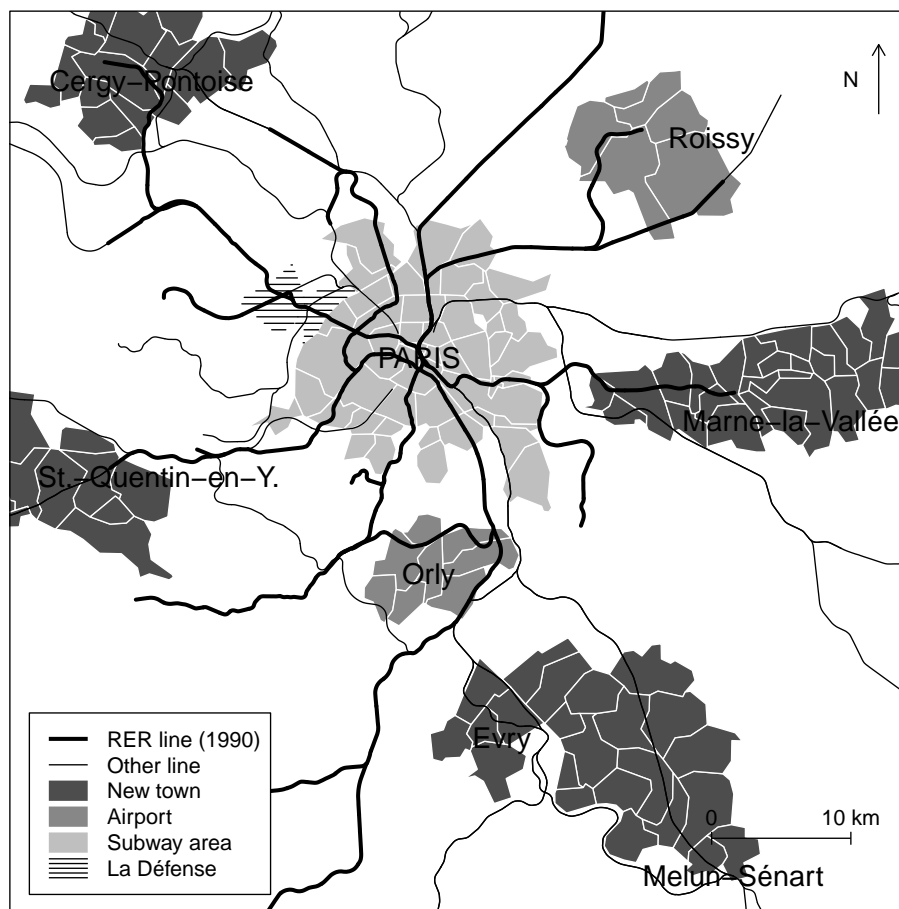
Reading note: in 2006, 29% of the workforce living in the suburbs, within 15km from Paris, commutes to Paris.

Sources: Population Census 1968-2006.

living in the City of Paris and people living on the outskirts of the metropolitan areas (except for those living more than 25 km away from Paris, see Table 2.2). Interestingly, the diagonal terms of the table, describing people working and living in the same part of the Paris region, are always lower in 2006, compared to 1968. This indicates that movements across the Paris region are more frequent than before.

While the development and organization of the suburbs was uncontrolled until the 1960s, the coming to power of President De Gaulle can be seen as a turning point (see section 2.A in the appendix for more details on the previous period). Indeed, his government decided to implement a new planning policy to organize the scat-

Figure 2.1: RER and new subcenters



Source: IAU – Île-de-France.

tered and under-equipped suburbs⁸ and to support the economic and demographic development of the Paris region. The SDAURP⁹ urban plan, presented in 1965 embodied this change in policy. This plan included the redistribution of administrative boundaries, the construction of new infrastructures, the decentralization of job and population in “new towns”. This SDAURP plan especially envisioned an ambitious commuter rail system, the so-called Regional Express Rail. The RER was supposed to upgrade the suburban train network by the construction of hundreds of kilometers of new lines crossing the historical core of Paris towards the new subcenters of the Paris metropolitan area, namely the five new towns¹⁰ (*Marne-la-Vallée*, *Cergy-Pontoise*, *Saint-Quentin-en-Yvelines*, *Melun-Sénart* and *Évry*), the two airports (*Orly* and *Roissy*) and *La Défense* business district.

⁸During a helicopter tour over the metropolitan area, President De Gaulle would even have ordered Paul Delouvrier, General delegate for the Paris region, to “Put this mess in order!”

⁹*Schéma directeur d'aménagement et d'urbanisme de la Région Parisienne.*

¹⁰New towns designate planned sub-cities located between 15 and 35 km away from the center of Paris in relatively underdeveloped areas. They were supposed to receive between 500,000 and 1,000,000 inhabitants and thus disperse the population over the Paris region, in order to reduce urban congestion.

This ambitious project was implemented in the two subsequent decades in a more modest way than initially planned. Eventually, the RER project mainly consisted of upgrading existing lines by connecting them together with tunnels under the historical city core of Paris. It also included the construction of new branch lines in the outskirts, the commissioning of new trains and high-frequency services. In fact, only 71 of the 433 RER stations are fully new; 98 km of railways were built and 22 km were reopened out of a 600-km network.

However, the five-line network, progressively opened between 1969 and 2004, reaches the goals assigned by the 1965 plan to connect the new subcenters to the historical center of Paris (see Figure 2.1). Despite only few new track segments, the RER led to a significant improvement of the commuter rail network and made commuting much easier (see Figure 2.4 in the appendix for an example). According to our simulations, the mean travel time to Paris¹¹ was 49.9 minutes in 1969. Between 1969 and 2009 it decreased by 5.8 minutes for the municipalities connected to the RER while it decreased only by 1.3 minutes in the municipalities outside of the new network. Thus, the RER offered about a 10 percent drop in commuting time for connected municipalities. This improvement may seem limited but the average effect hides a wide variety of situations. First, the journey time may only change for some neighborhoods. In addition, some lines did not improve much the travel time because they almost exclusively used existing rail tracks.

2.3 Identification strategy

One of the core challenges raised by this study was the need for an identification strategy addressing the endogeneity issue. A significant problem of transportation system evaluation is that new infrastructures are obviously not randomly located. As new lines and new stations were actually rare, our empirical strategy focuses on existing stations. We build a control group containing some suburban train stations that were not connected to the RER in 1990. We compare it to some stations that already existed in 1960 and that have been upgraded to RER stations before 1990. This section describes precisely which stations we select to obtain an exogenous treatment. We first present our main identification strategy, based on intermediate stations that are located between Paris city center and new economic subcenters and explain why such stations were upgraded “by accident”. Then, we introduce an alternative identification strategy, as a robustness test, which relies on the comparison between the initial plan and the actual RER network.

2.3.1 Comparison of intermediate cities

Our main identification strategy focuses on intermediate cities. As stated above, the RER network was developed with the aim of connecting the historical center of Paris to new subcenters. Consequently, RER lines happen to cross municipalities located in-between. We argue these municipalities have been connected to the RER network

¹¹Travel time to Paris is the mean of the minimum travel times to the 20 boroughs of Paris. In addition, we only consider the municipalities between 5 and 35 km from Paris which are the most likely to benefit from the RER, excluding the underground catchment area.

without intention to treat, in a similar approach than the one developed by Banerjee et al. (2012). In addition, the RER project mainly consisted of the enhancement of the existing commuter train network. This means that most of the intermediate stations were built in the 19th century, thus leaving no possibility of RER route manipulation. Besides, urban planning policies were highly centralized until the 1982 decentralization law, which invalidate the argument that local authorities may have influenced the RER project definition.

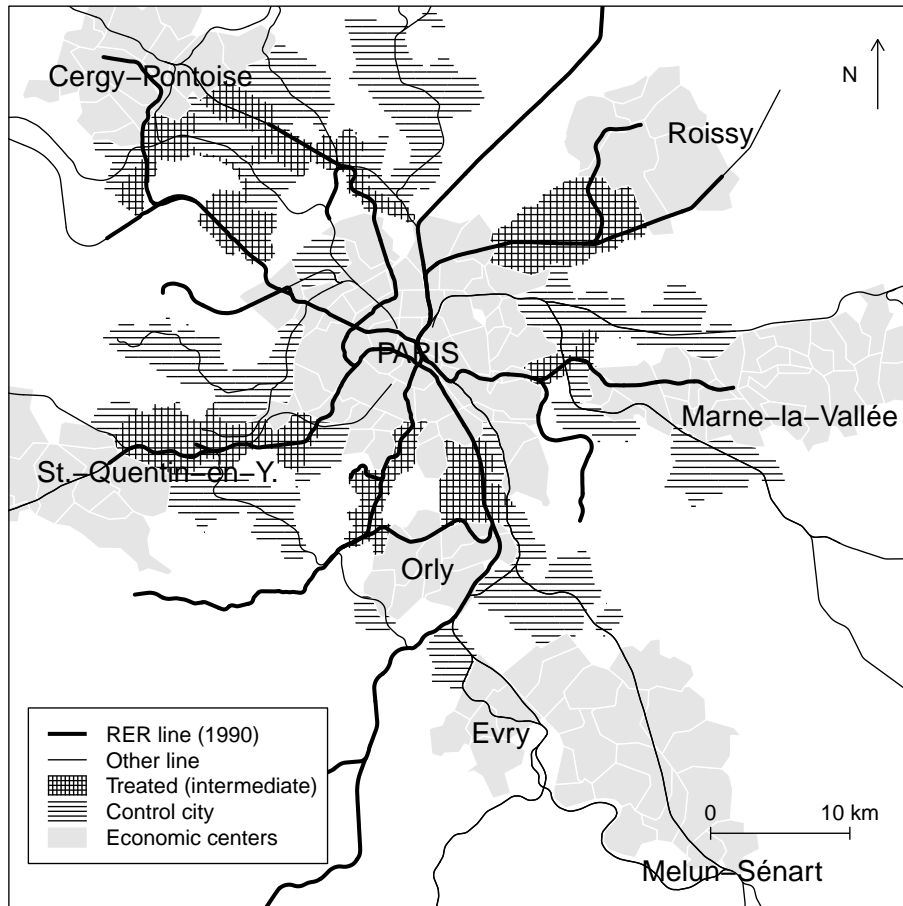
In our regressions, we only consider municipalities with at least one commuter rail station in 1975. We first exclude termini areas from both control and treatment groups as they might have been explicitly targeted by the RER policy. By termini areas, we mean the historic city of Paris and municipalities which are part of a new town, host an airport or the business district of *La Défense* (see Figure 2.1). Treatment is clearly not exogenous in these cases. In addition, as all new towns, airports, historic city center or business district were connected to the RER network, it is impossible to find a proper counterfactual for these municipalities. We also exclude municipalities connected to the underground network because it is not possible to use our identification strategy for them, that is to say a strategy based on intermediate municipalities.

Among the municipalities selected in our subsample, some were connected to the RER network because they happened to be located on the itinerary between the historical city center and new economic centers. We logically use them as a treatment group, excluding municipalities which were treated for other reasons.¹² Other municipalities, which are still served by commuter train but not by the RER, are used as a control group. Finally, we only include municipalities within 25km from Paris in our sample because outer ring municipalities, located further, were often rural in the 1960s and makes up only 11% of the Paris region population and 9% of jobs in 1968 (see Table 2.1). In addition, these municipalities are too far from Paris to be located on the itinerary to new economic subcenters (see Figure 2.2). Finally, Table 2.10 (in the appendix) shows that there is no significant difference in the employment growth of control and treatment groups before the RER implementation, providing support for the hypothesis of treatment exogeneity in this subsample.

We argue that the location of the new economic centers in the Paris metropolitan region is exogenous. Indeed, the initial 1965 project mentioned the construction of eight new towns while only five have been actually built. Moreover, experts of that

¹²Collardey (1999) details many technical reasons which explain the selection of RER lines amongst suburban rail lines. First, some lines were totally out of date and needed a major improvement. For example, the Vincennes line which serves the South East part of the region, was still served by steam-engine trains in 1969. Second, three stations in Paris required an underground extension in order to relieve traffic congestion, and were logically connected to the RER. In Austerlitz station, suburban train interfered with main-line traffic when crossing the station; St-Lazare station was the first station in terms of suburban traffic; free surface railway tracks were needed in Lyon station for high speed rail. Third, Gerondeau (2003) interestingly describes the difficult relationships between the two public companies in charge of Paris suburban train network, RATP (the subway company) and SNCF (the national railway company). RATP envisioned the RER project as a regional subway which would be independent of SNCF suburban network. The first two lines and the SDAURP plan of 1965 have been planned according to this idea and consequently required SNCF to sell local lines, without any main-line traffic, or to build brand new lines. This would not be the case anymore from the 1980s.

Figure 2.2: Control and treatment groups



Source: IAU – Île-de-France.

process (Alduy, 1983) insist on the fact that they were located in mostly rural areas and not in already developed places. Besides, the Orly airport was established on a WWI military base. The Roissy airport was located in a large agricultural land plot. Consequently, it is very unlikely that the location of these subcenters (airports and new towns) was determined to facilitate the connection of intermediate cities to the RER.

2.3.2 Differences between plans and realizations

Our alternative identification strategy, used as a robustness test, relies on the difference between the initial RER project and the actual network. Even if tunneling works for the East-West line started in 1961 (see section 2.A in the appendix for more details), the RER project was actually launched in 1965 with the SDAURP plan. As stated before, this very ambitious plan has been deeply modified during the development of the actual RER network. When possible, existing lines were improved instead of building sections of new rail tracks. We use this substantial difference between the 1965 SDAURP plan and the eventual network to build an alternative identification strategy. The treatment group contains municipalities which

were not supposed to be treated according to the initial 1965 plan, but were eventually connected to the RER network (see Figure 2.3 in the appendix). The control group remains unchanged compared to the main identification strategy.

In addition, this turnout in the RER project is largely due to non-economic factors, independent from growth perspectives of treated municipalities. Zembri (2006) shows that the election of President Pompidou, following the resignation of President de Gaulle in 1969, played a major role in the way the RER project was developed. The cost of the projected new lines was his main reason to deviate from the initial project, the new administration considered it was possible to achieve similar goals, largely relying on the existing network. As stated before, this turned out to be true. Such sudden changes in the RER program also suggest that long-term economic anticipations were not pivotal in the choice of RER routes except for new subcenters. Moreover, there is no chance that development perspectives do change that quickly when considering the construction of a transport infrastructure designed to last more than a century. Finally, we find similar results with both identification strategies for employment (see Table 2.13 in the appendix).

2.4 Econometric method

This section presents how we measure the effect of the RER on firm location, employment and population, applying the two identification strategies presented in the previous section. As stated before, we work at the municipality level and our main treatment variable is the variation in travel time by public transportation from our stations of interest to the city center (defined as the City of Paris). This accounts more precisely for the heterogeneity in the RER treatment. We also run robustness checks using a dummy variable taking value one if a RER station is located in the municipality.

The period covered by our estimation stretches from 1975 to 1990. Considering the fact that the RER network progressively spread over the Paris metropolitan region, the treatment group enlarges over time while the control group becomes smaller (see Figure 2.2). Consequently, there are too few untreated municipalities in the inner ring after 1990, it is thus difficult to use our identification strategy after that date. In addition, major network improvements were put into service in the 1970s and the 1980s; RER impact consequently fades out in the subsequent period, even if we find significant but smaller RER effect between 1990 and 2006 (see Table 2.12 in the appendix).

$$\Delta \ln Y_{i,75 \rightarrow 90} = \Delta \text{time}_{i,75 \rightarrow 90} + \beta X_{i,1975} + \epsilon_i \quad (2.1)$$

The dependent variable is the growth rate in population, employment or count of firms (Y) in municipality i between 1975 and 1990, $\Delta \ln Y_{i,75 \rightarrow 90} = \ln Y_{i,1990} - \ln Y_{i,1975}$. We regress this variable on the treatment which is the variation in the travel time between 1975 and 1990¹³. We also add initial socio-demographic and geographic controls X_i : initial density of the considered variable, land availability (i.e. the share of farm land in 1960), distance to Paris, surface, geographic dummies

¹³Alternative time spells and variables are tested as robustness tests, see part 2.6.4.

Table 2.3: Comparison of control and treatment groups – Mean values and standard deviations

	Untreated	Treated
Pop. density in 1975 (people per sq. km)	3420 (3266)	5800 (2909)
Empl. density in 1975 (worker per sq. km)	989 (1317)	1759 (1510)
Firm density in 1975 (firm per sq. km)	109 (140)	167 (105)
Travel time to Paris in 1975 (minutes)	47 (8)	41 (7)
Δ travel time to Paris 1975-1990 (minutes)	1.4 (1)	2.8 (1.5)
Distance to Paris (km)	16.5 (4.6)	13.6 (4.4)
Surface (sq. km)	6 (3.3)	6.6 (4.9)
Job growth rate 1968-75 (in pct)	24 (48)	12 (29)
Job growth rate 1975-90 (in pct)	32 (62)	22 (38)
Number of cities	64	32

Note: treatment status in 1990, a treated city includes a RER station. Standard deviations in parenthesis.

Sources: Population Census, SIRENE.

(North, South, East or West of Paris), initial travel time by mass transit and measures of alternative transportation infrastructure (highways, and commuter train for some specifications).

Table 2.3 shows initial discrepancies between control and treatment groups in 1975. Treated municipalities, i.e. municipalities with at least one RER station, are denser and closer to Paris. We also noticed that the growth rate of employment is higher for untreated municipalities compared to treated ones. It is due to the fact that control group municipalities are smaller and grew more rapidly because of a catch-up phenomenon.

We investigate further on those differences in Table 2.4, where we regress travel time reduction and a dummy variable indicating the presence of a RER station on the characteristics of the city. Column (1) shows a clear link between RER and travel time reduction. Travel time decreased by 2.8 minutes between 1975 and 1990 in treated municipalities while it only dropped by 1.4 minutes in municipalities outside of the RER network. Column (2) to (3) shows that very few variables are significantly correlated with travel time variation, which validates the choice of the treatment variable. On the opposite, as stated in column (4) and (5), there is a clearer link between RER stations location and demographic characteristics of cities, confirming descriptive statistics presented in Table 2.3. It is a second important argument in favor of using time variation as treatment variable, it does not only provide a more accurate quantification of the treatment, it is also more exogenous. Finally, we provide an additional robustness test based on the weighted propensity score method (Imbens and Wooldridge, 2008), to ensure that our estimation of the

treatment effect is not biased by non-linearities.¹⁴ The results presented in Table 2.11 (in the appendix) are very similar to baseline results. This confirms our model controls for the preexisting differences between control and treatment groups.

Finally, an important caveat should be added regarding interpretation of the results. There may be two effects of RER: attractiveness and displacement. If employment rises due to increased attractiveness, untreated municipalities should not be affected. In that case, the RER network increases employment or the total number of firms in the whole region. On the contrary, if employment increases in a treated municipality because of a displacement effect, it necessarily implies a decrease for other municipalities, and even potentially municipalities belonging to the control group. Obviously, the reality is most likely in between. To discriminate between the two effects in the case of firm location, Schmidheiny and Brühlhart (2011) suggest the use of a nested logit model in order to separate the respective share of relocation and attractiveness. The estimation of such a model requires to consider outside options for individual firms (for example, the rest of France and other European countries). We leave this more ambitious exercise to future research, as focusing on effects inside the Paris area will not allow us to isolate the two effects rigorously.

2.5 Data

We use data at the municipality level from different sources to provide information on firm counts, employment and population. We also build a new dataset to precisely describe the evolution of the urban transportation system between the 1970s and the 2000s. Municipality is the most accurate geographical scale available since more desegregated data are computed only from the 1990s. Although, French municipalities are particularly small in comparison with other European countries (1300 in the Paris region) which is an adequate geographical scale for this type of estimation. Finally, there are 96 municipalities in the regression sample (see Figure 2.3).

Most of the data used in our analysis are Census data. Census provides a large set of variables at the municipality level, directly comparable over time. This dataset includes population level, employment broken down in four industries (agriculture, manufacturing, construction and services), commuting patterns and the social composition in terms of diploma at each census year (e.g. 1968, 1975, 1982, 1990, 1999 and 2006). Data about firms come from the French administrative business register “SIRENE” between 1974 and 2004, which is less reliable than the Census for use over a long period of time (changing structure, new nomenclatures). SIRENE provides information on the industry and location of each firm, along with its opening and closing years. We calculate the firm stock at the municipality level, at each census year. We also use information on foreign investments over the period under study. Until the 1990s, foreigner investors had to register every investment in France at the French Treasury. We computed a dataset of foreign investment direct investment

¹⁴We weight treated municipalities, i.e. municipalities with a RER station in 1990, by $1/e$ and control municipalities by $1/(1 - e)$ in our baseline regression, where e is the propensity score estimated with the regression presented in the column (5) of Table 2.4.

(FDI), using this administrative requirement. As this compulsory registration was phased out in the 1990s, reliable data on FDI are available only until 1994.

Our first treatment variable is the variation in travel time by public transportation. It partly accounts for the RER effect on train service quality. To build this variable, we assume that RER did not increase the speed of trains, but improved travel time by public transportation thanks to fewer train changes (see Figure 2.4 for an example). This assumption is likely to hold as a large majority of the electrification of the commuter train network, which increased the train speed through the phasing out of steam traction, was completed several years before. The observed variation of travel time is therefore due to the connection of isolated lines. In fact, we consider the commuter train and metro networks as one graph. The train and metro stations are the nodes of this graph while lines which link the stations are the edges of this graph. We apply a simple shortest path algorithm to calculate the journey time between two stations of the transportation system. To compute this journey duration for a given year, we remove edges corresponding to lines opened after this date. We thus obtain a matrix of travel duration between every station of the transportation system, year by year, from 1969 to 2009.

We aggregate the matrix in a single variable which is the mean of the travel time to the 20 districts of the City of Paris. This variable, hereafter the mean travel time to Paris, is a good summary of the transportation system as the network is highly centralized towards Paris. We consider the travel time to all districts of Paris because the RER development mainly consisted of the digging of four tunnel inside Paris to connect isolated lines. The travel time to the closest districts possibly did not decrease while a new tunnel may allow a better connection, and thus a lower travel time, to more distant districts.

However, this treatment variable may not fully account for the improved train service due to the RER, such as higher frequency, new trains, better reliability. To test the robustness of our approach, we use alternative variables, namely the presence of a RER station in the municipality and the number of RER stations in the municipality. A natural measure of the improved quality of the network would be the increased frequency of trains, which played a big role in the improvement of the Paris commuter train system. Unfortunately, data are not available over our period of reference.

Note also that all treatment variables are aggregated at the municipality level, while they only affect economic agents located in the vicinity of a station. Consequently, we do not know exactly who benefit from such improvements. Indeed, in a given municipality, there are both treated (in the catchment area of a RER station) and untreated (in other parts of the municipality) economic agents. In other words, a higher number of RER stations or a greater reduction in travel time do not necessarily imply a transport service improvement of same magnitude, at the municipality level.

We do not want highway accessibility to bias RER effect estimation, that is why we also control for proximity to the highway network. Indeed, highways are not uniformly distributed among Paris metropolitan region, car-based accessibility was improved in some municipalities while it is stable in other. For a given municipality, we create a dummy taking value one if the nearest highway is less than 1km away.

Table 2.4: Determinants of municipality selection for RER treatment

<i>Dependant variable:</i>	$\Delta time_{Paris} 1975-90$			RER station	
<i>Model:</i>	OLS			Logistic	
	(1)	(2)	(3)	(4)	(5)
Intercept	1.351*** (0.128)	2.381** (0.97)	0.501 (1.758)	-0.439 (0.924)	5.65* (2.902)
RER ₁₉₉₀	1.445*** (0.295)				
Pop density ₁₉₇₅ < 1000		-1.165 (1.012)	-2.058 (1.349)	-1.817 (1.442)	-6.026** (2.468)
Pop density ₁₉₇₅ [1000, 2500]		-1.294 (1.023)	-2.196* (1.308)	-1.21 (1.208)	-5.527** (2.279)
Pop density ₁₉₇₅ [2500, 5000]		-0.751 (1.022)	-1.611 (1.27)	0.796 (1.073)	-1.791 (1.879)
Pop density ₁₉₇₅ [5000, 10000]		-0.079 (1.036)	-0.648 (1.132)	1.073 (1.015)	0.463 (1.58)
Pop density ₁₉₇₅ > 10000		ref.	ref.	ref.	ref.
Pop growth ₁₉₄₆₋₇₅		0.049 (0.043)	0.007 (0.041)	-0.177 (0.128)	-0.362* (0.214)
$time_{Paris} 1975$			0.052 (0.034)		-0.113** (0.052)
< 1km highway			0.427 (0.422)		2.708** (1.173)
$5km \leq d_{Paris} < 10km$			-0.399 (0.764)		-3.456* (2.1)
$10km \leq d_{Paris} < 15km$			0.409 (0.497)		-1.389 (1.623)
$15km \leq d_{Paris} < 20km$			0.508* (0.275)		0.601 (1.419)
$20km \leq d_{Paris} < 25km$			ref.		ref.
Share of farmland ₁₉₆₀			-0.425 (0.543)		4.317** (2.14)
West			0.031 (0.375)		0.289 (1.017)
North			ref.		ref.
East			-0.019 (0.628)		-1.005 (1.316)
South			0.272 (0.416)		-0.206 (1.007)
Surface			0.017 (0.037)		0.064 (0.096)
Number of observations	96	96	96	96	96
R ²	0.246	0.125	0.258		
AIC				111	109

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities.

2.6 Results

2.6.1 The RER effect on employment

Table 2.5 provides our benchmark results on employment growth. The first column presents the simplest way of estimating the parameter of interest. All municipalities connected to the suburban rail network are included except for the most central part of the Paris region, very urbanized and equipped with a dense subway system. We also control for some basic characteristics of the municipalities. A dominant feature of the city-level growth in Paris region seems to be the catching up, since the effect of initial job density declines steadily with the level of density. We obtain a positive and significant impact of 5.7%. Besides, this column shows that the economic subcenters of Paris region grew extremely quickly between 1975 and 1990. Excluding these subcenters in column (2) hardly decreases the coefficient associated to the treatment to around 5.6%.

The results of our preferred identification strategy are reported in column (3) and (4). The treatment group includes only intermediate cities, located between Paris and economics subcenters. In the regression whose results are reported in column (3), we include a large set of controls that are likely to drive job location whereas we only keep influential variables in column (4). Finally, our preferred regression is reported in column (4). We find that employment grows by 5.9% when reducing travel time to Paris by one minute, between 1975 and 1990. Note that distances to the closest airport and the closest new town are not significant in column (3) and do not change much the magnitude of treatment effect when removed in column (4). This means that the larger employment growth in RER intermediate municipalities is not due to a potential spillover effect deriving from their proximity to economic subcenters. We argue that the effect we find is due to improved transportation.

This RER effect appears to be important, our explanation is that treatment measurement does not only capture the fact that the RER system shortens travels by public transportation, thanks to more direct routes. It probably also accounts for all other RER improvements: new trains, more frequent services or renovated stations. As a robustness test, we interact the RER dummy variable with our baseline treatment variable (see Table 2.9 in the appendix). We interestingly find that employment increases only if the travel time reduction is associated with the the opening of a RER station. This confirms that our treatment variable captures multiple dimensions of RER improvement.

2.6.2 Different tastes for accessibility

We now turn to the effect of RER on the number of firms and across sectors for employment. Table 2.6 indicates that the positive effect of the Regional Express Rail is also valid for firm location choices. However, we note that the estimated impact on the number of firms is lower than the one obtained for employment. Striking differences arise when we look at the effects of the RER system on foreign-owned firms. First, initial density does not seem to exhibit the same catch up effect as in column (1). Second, the RER treatment effect is much larger in magnitude for this set of firms. The number of foreign-owned firm grows by 12.5% when travel time

Table 2.5: Effect of RER on employment at the municipality level

	(1)	(2)	(3)	(4)
<i>Dependant variable:</i>	$\Delta \ln \text{employment}_{1975-90}$			
<i>Sample restriction:</i>				
- No economic subcenters		✓	✓	✓
- Only intermediate municipalities in treatment group			✓	✓
Intercept	-0.194 (0.381)	-0.155 (0.384)	0.031 (0.59)	-0.252 (0.269)
$\Delta \text{time}_{Paris}$ 1975–90	0.057*** (0.017)	0.056*** (0.017)	0.066*** (0.02)	0.059*** (0.019)
time_{Paris} 1975	-0.005 (0.005)	-0.003 (0.005)	-0.006 (0.006)	-0.006 (0.005)
Job density ₁₉₇₅ < 200	0.819*** (0.133)	0.72*** (0.135)	0.657*** (0.181)	0.652*** (0.157)
Job density ₁₉₇₅ [200, 500]	0.643*** (0.118)	0.588*** (0.115)	0.488*** (0.173)	0.519*** (0.137)
Job density ₁₉₇₅ [500, 1000]	0.318*** (0.094)	0.339*** (0.092)	0.308* (0.163)	0.317*** (0.116)
Job density ₁₉₇₅ [1000, 2500]	0.217*** (0.081)	0.206*** (0.077)	0.172 (0.143)	0.199** (0.1)
Job density ₁₉₇₅ > 2500	ref.	ref.	ref.	ref.
< 1km highway	0.082 (0.081)	0.076 (0.079)	0.082 (0.1)	0.086 (0.097)
$5\text{km} \leq d_{Paris} < 10\text{km}$	0.355*** (0.135)	0.353*** (0.128)	0.09 (0.175)	0.157 (0.133)
$10\text{km} \leq d_{Paris} < 15\text{km}$	0.172* (0.094)	0.167* (0.095)	0.008 (0.128)	0.088 (0.102)
$15\text{km} \leq d_{Paris} < 20\text{km}$	0.114 (0.083)	0.135 (0.082)	0.025 (0.107)	0.05 (0.106)
$20\text{km} \leq d_{Paris} < 25\text{km}$	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.245** (0.123)	0.231* (0.121)	0.207 (0.145)	0.245 (0.157)
Surface	-0.002 (0.005)	0.002 (0.005)	0.008 (0.007)	0.011 (0.008)
West	0.055 (0.083)	-0.014 (0.071)	0.036 (0.084)	
North	ref.	ref.	ref.	
East	0.062 (0.124)	-0.023 (0.109)	0.022 (0.132)	
South	-0.021 (0.131)	-0.093 (0.125)	-0.111 (0.166)	
Dist. to new town	-0.007 (0.01)	-0.008 (0.01)	-0.0005 (0.0156)	
Dist. to airport	-0.004 (0.007)	-0.007 (0.007)	-0.009 (0.011)	
New town	0.707** (0.302)			
Airport	0.04 (0.112)			
La Defense	0.491*** (0.13)			
Number of observations	143	128	96	96
R ²	0.508	0.38	0.398	0.379

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975 and located between 5 and 25 km away from Paris. Sources: Population Census.

decreases by one minute. This suggests that mass transit affects foreign firms more intensely than local ones. It is also to be noted that highways have a much stronger impact. Therefore, transport infrastructure, and access to the city center appears to play a major role in decision to invest for foreign investors. The last four columns show the effect of the RER on the industry specialization of the municipality, broken down into agriculture, construction, manufacturing and services. The point estimate is similar across industries, except for agriculture. Based on these results, we can not conclude that RER caused a shift in industry composition in treated municipalities, except obviously for agriculture.

Table 2.7 reports results for the overall population growth. We find a weak and significant effect of travel time decrease on the population. However, this effect is not robust to the use of alternative treatment variables (see Tables 2.9 and 2.14) or of the second identification strategy (see Table 2.15 in the appendix).

We find suggestive evidence of a gentrification effect. We do not observe either income or housing prices at the city level in the 1970s and the 1980s. Given this data limitation, the skill level of the population can be considered as an acceptable first approximation. We break down the population into three categories: low-skilled (primary- or middle-school), middle-skilled (vocational- or high-school) and highly-skilled (higher education). We find a significant impact of the RER on the highly-skilled population and this effect is robust across specifications (see Tables 2.14 and 2.15 in the appendix). This suggests a greater attractiveness of land nearby RER stations, meaning that more accessible areas end up being inhabited by households with a higher willingness to pay for housing. Given that the inner ring was already widely urbanized in the 1960s, especially in the vicinity of suburban train stations, a global population growth would have required to densify treated municipalities. However, chapter 5 highlight a low supply elasticity on the French housing market in the long-term, suggesting that the RER is unlikely to increase the housing stock in previously developed areas.¹⁵ In a word, our results suggest that the increased accessibility resulted in a population displacement effect.

2.6.3 A better access to metropolitan job market

We test our model on the length of commuting trips to better understand the relation between firm location choice and transportation. For a given municipality, we calculate the mean distances traveled by residents to go to work and the mean distance traveled by workers to commute from home. Then we regress the change in the distance traveled between 1975 and 1990 on the variables of the model. Table 2.8 shows that workers commute significantly further from home in treated municipalities. When travel time to Paris decreases by one minute, the mean commuting distance of workers increases by 4.1%. On the contrary, we find no impact of RER on the commuting distance of residents. Such results are in line with the fact that RER affects more clearly firm location than resident location. It also means that firms choose to locate in treated municipalities because they can reach a broader labor market. RER is probably a driver of job decentralization within the Paris region be-

¹⁵This question would need to be linked with regulation of land use and building height, but here again lack of data limits the possibilities for further investigations.

Table 2.6: Effect of RER on firms and employment by industry

<i>Dependant variable:</i>	$\Delta \ln \text{firm}_{75-90}$		$\Delta \ln \text{employment}_{75-90}$			
<i>Sample:</i>	All firms	Foreign firms	Agriculture	Manufacturing	Construction	Services
Intercept	-0.446* (0.229)	0.754 (0.633)	-0.946 (0.867)	1.434 (0.972)	3.139*** (0.986)	2.256 (1.522)
$\Delta \text{time}_{Paris} 1975-90$	0.031** (0.014)	0.125** (0.048)	0.022 (0.068)	0.08** (0.037)	0.094*** (0.03)	0.056** (0.022)
$\text{time}_{Paris} 1975$	0.009** (0.004)	-0.013 (0.014)	0.003 (0.015)	-0.018 (0.012)	-0.01 (0.008)	-0.009 (0.006)
$5km \leq d_{Paris} < 10km$	0.161 (0.108)	0.945*** (0.329)	0.811* (0.439)	0.524** (0.216)	0.086 (0.225)	0.049 (0.136)
$10km \leq d_{Paris} < 15km$	0.102 (0.094)	0.821*** (0.262)	-0.153 (0.318)	0.348* (0.195)	0.103 (0.214)	0.012 (0.103)
$15km \leq d_{Paris} < 20km$	0.051 (0.094)	0.537* (0.308)	-0.396 (0.253)	0.351* (0.195)	0.022 (0.177)	0.021 (0.122)
$20km \leq d_{Paris} < 25km$	ref.	ref.	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.278* (0.156)	0.032 (0.624)	0.896* (0.501)	0.22 (0.377)	0.06 (0.248)	0.161 (0.17)
Surface	0.002 (0.007)	0.019 (0.018)	-0.009 (0.023)	0.004 (0.012)	0.014 (0.013)	0.007 (0.009)
< 1km highway	0.022 (0.076)	0.526** (0.21)	-0.328 (0.242)	0.4** (0.162)	-0.19 (0.161)	0.098 (0.128)
Firm density ₁₉₇₅ < 50	0.327** (0.133)	-0.342 (0.55)				
Firm density ₁₉₇₅ [50, 100]	0.186* (0.111)	-0.653 (0.506)				
Firm density ₁₉₇₅ [100, 200]	0.115 (0.082)	-0.395 (0.422)				
Firm density ₁₉₇₅ [200, 500]	0.017 (0.066)	-0.523* (0.275)				
Firm density ₁₉₇₅ >500	ref.	ref.				
$\ln \text{dens. foreign firms}_{1975}$		-0.51*** (0.097)				
Job density ₁₉₇₅ < 200			0.995* (0.533)	0.316 (0.509)	-1.233*** (0.406)	-0.412 (0.597)
Job density ₁₉₇₅ [200, 500]			0.554 (0.458)	0.268 (0.358)	-0.661* (0.369)	-0.222 (0.431)
Job density ₁₉₇₅ [500, 1000]			0.964** (0.399)	-0.002 (0.274)	-0.703** (0.299)	-0.176 (0.317)
Job density ₁₉₇₅ [1000, 2500]			0.524 (0.331)	0.085 (0.163)	-0.375* (0.2)	-0.103 (0.179)
Job density ₁₉₇₅ > 2500			ref.	ref.	ref.	ref.
$\ln \text{dens. agricult.}_{1975}$			-0.504*** (0.098)			
$\ln \text{dens. manufa.}_{1975}$				-0.303*** (0.095)		
$\ln \text{dens. building}_{1975}$					-0.548*** (0.158)	
$\ln \text{dens. services}_{1975}$						-0.263 (0.192)
Number of observations	96	74	69	95	96	96
R ²	0.485	0.506	0.53	0.484	0.398	0.319

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. The regressions on skill level are run on the labor force.

Sources: Population Census, SIRENE.

Table 2.7: Effect of RER on population by level of education

<i>Dependant variable:</i>	$\Delta \ln \text{population}_{75-90}$			
<i>Sample:</i>	All	Primary or middle school	Vocational or high school	Higher education
Intercept	0.23 (0.172)	-0.275 (0.818)	1.537** (0.755)	2.182*** (0.382)
$\Delta \text{time}_{Paris} 1975-90$	0.022** (0.009)	0.026** (0.012)	0.03** (0.013)	0.046*** (0.016)
$\text{time}_{Paris} 1975$	-0.005 (0.003)	-0.003 (0.004)	-0.01** (0.004)	-0.0001 (0.0036)
$5km \leq d_{Paris} < 10km$	-0.167** (0.08)	-0.247** (0.099)	-0.369*** (0.104)	0.158 (0.13)
$10km \leq d_{Paris} < 15km$	-0.113 (0.072)	-0.186** (0.086)	-0.283*** (0.088)	0.025 (0.095)
$15km \leq d_{Paris} < 20km$	-0.053 (0.065)	-0.072 (0.075)	-0.177** (0.078)	-0.025 (0.089)
Share of farmland ₁₉₆₀	0.074 (0.138)	0.18 (0.152)	0.055 (0.149)	-0.187 (0.144)
Surface	-0.003 (0.005)	0.003 (0.007)	-0.001 (0.006)	-0.007 (0.007)
< 1km highway	0.047 (0.065)	0.028 (0.073)	0.069 (0.066)	0.016 (0.075)
Pop density ₁₉₇₅ < 1000	0.185* (0.106)	-0.024 (0.286)	-0.103 (0.315)	-0.223 (0.172)
Pop density ₁₉₇₅ [1000, 2500]	0.236*** (0.088)	0.107 (0.183)	0.09 (0.196)	0.093 (0.154)
Pop density ₁₉₇₅ [2500, 5000]	0.049 (0.059)	-0.039 (0.124)	-0.08 (0.13)	0.039 (0.105)
Pop density ₁₉₇₅ [5000, 10000]	-0.0007 (0.0217)	-0.026 (0.063)	-0.036 (0.066)	0.012 (0.087)
Pop density ₁₉₇₅ > 10000	ref.	ref.	ref.	ref.
$\ln \text{dens. prim. or middle school}_{1975}$		0.027 (0.09)		
$\ln \text{dens. voc. or high school}_{1975}$			-0.093 (0.103)	
$\ln \text{dens. higher education}_{1975}$				-0.284*** (0.059)
Number of observations	96	96	96	96
R ²	0.359	0.3	0.43	0.567

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities.

Sources: Population Census.

cause firms can hire workers that used to be reachable only from the central part of the metropolitan region before RER implementation. Better public transportation allows to hire them in a peripheral location.

2.6.4 Robustness checks

The central assumption of difference-in-differences models is that the control group and the treatment group would have grown by the same amount in absence of the treatment. To test for this common trend assumption, we provide a placebo test. We run our model on the 1968-1975 period to be sure there is no ex-ante trend gap between groups. The placebo test gives support to our identification strategy as we do not find any significant impact of RER before 1975 using two different treatment variables for both population and employment (see Table 2.10 in the appendix). It also shows that the RER did not induce significant anticipation effects on firms. We generalize these placebo tests, in Table 2.12 (in the appendix). We estimate the effect of travel time variation on population and employment growth across different periods (1968-1975, 1975-1990 and 1990-2006). We find no significant impact of RER before 1975 and a smaller impact after 1990.

Table 2.9 (in appendix) presents the estimation of the treatment effect using alternative treatment variables: a dummy variable indicating whether a municipality is connected to the RER network or not in 1990, the interaction terms of the previous dummy variable with the travel time decrease, and the number of stations in a given municipality in 1990. All variables yield significant results for job location confirming the robustness of mass transit impact on employment. Indeed, employment increases by 12.3% in municipalities connected to the RER network compared to municipalities which are only served by suburban train. Besides, employment increases by 12.7% with an additional station. As stated above, the reduction in journey duration causes an increase in employment only in municipalities connected to the RER network. Finally, this robustness check is not conclusive for population, treatment effect is weakly significant in only one out of three specifications and no significant at all in the two other cases.

Finally, we run our model using the second identification strategy presented in section 2.3.2 using the differences between the actual RER network and the initial 1965 project. As explain before, we do not use intermediate stations, located on RER lines linking Paris and economic subcenters, as a treatment group. We select instead municipalities that should not have been connected to the RER network according to the 1965 SDAURP plan but that happened to be actually treated. We obtain a very similar RER effect for employment and placebo tests (see Table 2.13 in the appendix). However, as mentioned before, we do not find any significant impact of a reduction in journey duration on the total population growth.

2.7 Conclusion

The Regional Express Rail was an important enhancement of the Paris suburban train service. From 1969 to 2003, it progressively improved the mass transit system by connecting isolated lines, serving new economic sub-centers and introducing more

Table 2.8: Effect of RER on commuting distance

<i>Sample:</i>	Workers (dist. to home)		Residents (dist. to the workplace)	
<i>Dependent variable:</i>	$\Delta \ln$ mean commuting distance			
	1975-1990	1968-1975	1975-1990	1968-1975
Intercept	0.931*** (0.188)	0.256 (0.297)	0.953*** (0.098)	0.893*** (0.208)
$\Delta time_{Paris}$ 1975–90	0.041*** (0.014)	−0.015 (0.015)	0.0007 (0.0029)	−0.003 (0.006)
$time_{Paris}$ 1975	−0.004 (0.003)	0.013** (0.006)	−0.002* (0.001)	0.0006 (0.0018)
$5km \leq d_{Paris} < 10km$	0.066 (0.076)	0.183 (0.131)	−0.155*** (0.037)	−0.262*** (0.085)
$10km \leq d_{Paris} < 15km$	0.082 (0.073)	0.103 (0.102)	−0.095*** (0.025)	−0.177*** (0.06)
$15km \leq d_{Paris} < 20km$	0.082 (0.065)	−0.06 (0.121)	−0.037* (0.02)	−0.123** (0.052)
$20km \leq d_{Paris} < 25km$	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	−0.035 (0.108)	0.463** (0.191)	−0.075** (0.032)	−0.11 (0.087)
Surface	0.014** (0.006)	0.00007 (0.00845)	−0.002 (0.001)	0.002 (0.003)
< 1km highway	0.162** (0.078)	0.019 (0.073)	−0.005 (0.016)	−0.059 (0.04)
Job density ₁₉₇₅	✓			
Job density ₁₉₆₈		✓		
Population density ₁₉₇₅			✓	
Population density ₁₉₆₈				✓
\ln dist. to home ₁₉₇₅	−0.424*** (0.102)			
\ln dist. to home ₁₉₆₈		−0.446*** (0.088)		
\ln dist. to the workplace ₁₉₇₅			−0.344*** (0.037)	
\ln dist. to the workplace ₁₉₆₈				−0.334*** (0.078)
Number of observations	96	96	96	96
R ²	0.493	0.454	0.766	0.401

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. Sources: Population Census.

frequent trains. This natural experiment allows us to estimate the impact of urban transit on population, firm and employment growth.

A classic endogeneity issue arises from the fact that transport infrastructures are not randomly located. We address this problem by comparing suburban train stations, which existed before RER introduction. Among them, some were upgraded into RER stations and other were not, for reasons we document to be, to a large extent, exogenous to their future growth. First, one of the main goals of the RER program was to connect the city center to the new economic subcenters (airports, the business district of *La Défense* and new towns). We restrict our analysis to municipalities located between the two latter types of places, arguing there is no intention to treat such municipalities crossed by the RER. Secondly, the discrepancies between the 1960s' projects and the 1990s' network confirms there were no clear intention to connect some areas rather than others except for economic subcenters.

We find that a one-minute drop in travel time by public transportation increases employment by 5.9% in the 1975-1990 period at the municipality level. We obtain similar results for firm location. We find no effect on the population as a whole, but there is a significant link between RER and location choice of highly-skill households, suggesting a different willingness to pay for accessibility depending on the type of economic agent. We run a placebo test for both strategies, showing there were no significant differences between control and treatment groups before the RER introduction.

Finally, our results suggest that the RER network caused the location of 800 additional jobs in each municipality between 1975 and 1990.¹⁶ This corresponds to a total increase of 26,000 jobs in the whole treatment group, and even 66,200 jobs if we consider that our treatment estimation is valid for all peripheral municipalities connected to the RER.¹⁷

¹⁶Travel time decreased by 2.8 minutes in the treatment group to 1.4 minutes in the control group. Using the point estimate of our baseline regression (Table 2.5), RER thus caused a 8.5 percent increase of employment in connected municipalities. Besides, considering that 9,500 employees worked in each treated municipality in 1975, we obtain that the total employment stock grew by 800 between 1975 and 1990 because of the RER.

¹⁷The treatment group includes 32 municipalities. Besides, the average employment stock amounts to 900 in 1975, in the 73 municipalities that are connected to the RER network within 25km from Paris. We exclude the most central part of the Paris region connected to the subway network in this quantification work.

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2.A Paris region before the 1960s

2.A.1 Urban policy before the 1960s

The history of the suburban rail is intricate. It reflects the changing place of Paris in the national planning policies and the complicated relation between the city of Paris and its suburbs. First, there is a strong and long-lasting opposition between the city of Paris and the outskirts of the city. The border, marked by a protection wall in the 19th-century, replaced by a urban highway in the 1960s and the 1970s (*Boulevard Périphérique*), is still present in the minds.¹⁸ The city of Paris has been strongly renovated by Baron Haussmann¹⁹ in the 19th century and is still very much organized according to the overall scheme defined and followed then. Conversely, the development and organization of the suburbs have been uncontrolled, which did not prevent the population from growing rapidly while it stabilized and eventually declined in the city center.

The first proposals to guide and organize urban growth were presented in the 1930s²⁰ but were only partially put in place. The post-WWII decade is marked by the attempt to “contain” the Paris region growth (Cottour, 2008), especially strong in an urban plan issued in 1960, called the PADOG²¹. This plan intended to limit urban development to the already built-up areas of the region. This central part should be reorganized and equipped, partly thanks to transportation infrastructures, while the rest of the region should remain unbuilt. At that time, the capital city was regarded as crowded, overdeveloped and its size and growth were seen as detrimental to the balanced development of the country. This sentiment was best summarized by the expression “*Paris and the French desert*” which refers to a French book by the geographer Jean-François Gravier, that was very influential in the (central) authorities in charge of regional development in France. As stated before, the coming to power of President De Gaulle marks the end of this malthusian urban policy.

2.A.2 Underinvestment in suburban rail until the 1960s

After a long period of underinvestment in suburban rail, the introduction of the RER offers a rapid and unprecedented improvement of the Paris mass transit system. The French railways have been mostly built during the 19th century by private companies. Each company was in charge of connecting a specific part of France to Paris. This institutional context results in a highly centralized network, the majority of lines are directed towards Paris and circular tracks are scarcer than radial ones, especially in the Paris region. In addition, networks of different companies were hardly connected and each of them ended in a different terminal station in Paris, even after merging the private companies in a unique public company in 1938. Consequently, it was not possible to go across Paris by train. A very dense and efficient subway system was built between 1900 and WWII, but it only served the city center.

¹⁸Even today, the city of Paris is significantly denoted by the term *intra muros*.

¹⁹Prefect of the Seine Department between 1853 and 1870, which included Paris in until 1967.

²⁰The *Plan Prost* in 1932 for example.

²¹*Plan d'Aménagement et D'Organisation Générale de la Région Parisienne*.

Contrary to certain initial projects²², the subway was not connected to the existing railway lines serving the suburbs. Because the Paris city council was against these connections in order to limit urban sprawl (Gerondeau, 2003). Consequently, commuting from one suburb to another required a train change and a metro connection (see Figure 2.4). These initial decisions deprived the Paris region from an efficient suburban train system until the 1970s. The deficient mass transit system raised many proposals in the 1920s and the 1930s aiming at connecting isolated lines by building railway tunnels through the city of Paris and the suburbs (Larroque et al., 2002). If the first extensions of the subway to the suburbs were actually built in the 1930s, almost nothing was done at this period for the suburban rail system.²³ After WWII, which logically stopped rail projects, a new suburban train system for Paris was regularly mentioned, without being actually started.

The first substantial plan for suburban mass transit is included in the PADOG plan issued in 1960. It summarizes the previous projects and proposes the digging of several tunnels through Paris in order to connect isolated suburban railway lines. The plan is rapidly followed by the start of civil works for the East-West line of the RER network in 1961. As explained before, the RER network project was actually launched by the SDAURP plan in 1965.

²²For example, the Haag project in 1887.

²³Except for the electrification of the Sceaux line in the south part of the Paris region.

2.B Supplementary estimates

Table 2.9: Effect of RER on employment - other treatment variables

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependant variable:</i>	$\Delta \log \text{employment}_{1975-90}$			$\Delta \log \text{population}_{1975-90}$		
Intercept	-0.402*** (0.149)	-0.233 (0.323)	-0.37*** (0.118)	0.024 (0.071)	0.199 (0.193)	0.063 (0.076)
RER ₁₉₉₀	0.123** (0.053)	0.137 (0.115)		0.067* (0.035)	0.035 (0.077)	
$\Delta \text{time}_{Paris} 1975-90$ (RER ₁₉₉₀ = 0) ×		0.032 (0.026)			0.016 (0.016)	
$\Delta \text{time}_{Paris} 1975-90$ (RER ₁₉₉₀ = 1) ×		0.082** (0.041)			0.019 (0.021)	
$\text{time}_{Paris} 1975$		-0.006 (0.007)			-0.004 (0.004)	
# RER stations ₁₉₉₀			0.127*** (0.044)			0.038 (0.023)
# train stations ₁₉₉₀			-0.071 (0.049)			-0.017 (0.024)
$5km \leq d_{Paris} < 10km$	0.195 (0.144)	0.153 (0.15)	0.234* (0.132)	-0.097 (0.066)	-0.155* (0.083)	-0.108 (0.068)
$10km \leq d_{Paris} < 15km$	0.148 (0.101)	0.088 (0.112)	0.113 (0.094)	-0.064 (0.069)	-0.104 (0.072)	-0.075 (0.071)
$15km \leq d_{Paris} < 20km$	0.08 (0.112)	0.052 (0.109)	0.068 (0.109)	-0.032 (0.064)	-0.051 (0.067)	-0.032 (0.065)
$20km \leq d_{Paris} < 25km$	ref.	ref.	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.172 (0.167)	0.221 (0.165)	0.104 (0.167)	0.061 (0.14)	0.065 (0.141)	0.056 (0.143)
Surface	0.01 (0.008)	0.009 (0.007)	0.011 (0.01)	-0.003 (0.005)	-0.003 (0.005)	-0.003 (0.006)
< 1km highway	0.067 (0.099)	0.048 (0.111)	0.083 (0.095)	0.022 (0.059)	0.036 (0.059)	0.034 (0.064)
Job density ₁₉₇₅	✓	✓	✓			
Population density ₁₉₇₅				✓	✓	✓
Number of observations	96	96	96	96	96	96
R ²	0.354	0.386	0.377	0.349	0.361	0.344

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. Sources: Population Census.

Table 2.10: Effect of RER on employment - placebo tests

	(1)	(2)	(3)	(4)
<i>Dependant variable:</i>	$\Delta \log \text{employment}_{1968-75}$		$\Delta \log \text{population}_{1968-75}$	
Intercept	-0.618** (0.269)	-0.069 (0.125)	-0.332 (0.254)	0.135 (0.1)
$\Delta \text{time}_{Paris} 1975-90$	0.003 (0.014)		0.021 (0.014)	
$\text{time}_{Paris} 1975$	0.012** (0.005)		0.008* (0.004)	
RER ₁₉₉₀		-0.007 (0.047)		0.019 (0.044)
$5km \leq d_{Paris} < 10km$	0.173 (0.128)	0.009 (0.114)	-0.061 (0.118)	-0.219** (0.092)
$10km \leq d_{Paris} < 15km$	-0.011 (0.105)	-0.116 (0.1)	-0.131 (0.101)	-0.208** (0.092)
$15km \leq d_{Paris} < 20km$	-0.013 (0.1)	-0.065 (0.104)	-0.106 (0.101)	-0.139 (0.103)
$20km \leq d_{Paris} < 25km$	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.39** (0.191)	0.306 (0.186)	-0.041 (0.194)	-0.113 (0.2)
Surface	0.008 (0.01)	0.007 (0.01)	0.008 (0.008)	0.008 (0.009)
< 1km highway	-0.014 (0.084)	0.037 (0.084)	-0.077 (0.071)	-0.036 (0.07)
Job density ₁₉₆₈	✓	✓		
Population density ₁₉₆₈			✓	✓
Number of observations	96	96	96	96
R ²	0.322	0.271	0.468	0.428

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. Sources: Population Census.

Table 2.11: Effect of RER on employment - weighted propensity score matching

	(1)	(2)	(3)	(4)
<i>Dependant variable:</i>	$\Delta \log \text{employment}_{1968-75}$	$\Delta \log \text{employment}_{1968-75}$	$\Delta \log \text{population}_{1968-75}$	$\Delta \log \text{population}_{1968-75}$
Intercept	-0.161 (0.285)	-0.532** (0.253)	0.233 (0.179)	-0.331 (0.235)
$\Delta \text{time}_{Paris\ 1975-90}$	0.053*** (0.017)	-0.0006 (0.0087)	0.027*** (0.008)	0.014 (0.012)
$\text{time}_{Paris\ 1975}$	-0.007 (0.005)	0.011** (0.005)	-0.007** (0.003)	0.008* (0.004)
$5\text{km} \leq d_{Paris} < 10\text{km}$	0.145 (0.161)	0.081 (0.127)	-0.129* (0.071)	-0.022 (0.11)
$10\text{km} \leq d_{Paris} < 15\text{km}$	0.098 (0.123)	-0.09 (0.106)	-0.077 (0.059)	-0.06 (0.085)
$15\text{km} \leq d_{Paris} < 20\text{km}$	0.093 (0.102)	-0.023 (0.079)	0.004 (0.053)	0.048 (0.099)
$20\text{km} \leq d_{Paris} < 25\text{km}$	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.211 (0.166)	0.405** (0.16)	0.171 (0.14)	0.025 (0.178)
Surface	0.009 (0.007)	0.003 (0.009)	-0.007 (0.006)	0.002 (0.008)
< 1km highway	0.174* (0.1)	0.073 (0.069)	0.099 (0.06)	-0.023 (0.073)
Job density ₁₉₇₅	✓			
Job density ₁₉₆₈		✓		
Population density ₁₉₇₅			✓	
Population density ₁₉₆₈				✓
Number of observations	96	96	96	96
R ²	0.468	0.412	0.512	0.361

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities.

Sources: Population Census.

Table 2.12: Effect of RER on employment and population across different time periods

	$\Delta \log$ employment			$\Delta \log$ population		
	1968-75	1975-90	1990-99	1968-75	1975-90	1990-99
$\Delta time_{1968-1975}$	-0.152 (0.23)	-0.209 (0.191)	-0.236 (0.159)	-0.148 (0.138)	-0.185 (0.117)	-0.06 (0.072)
$\Delta time_{1975-1990}$	0.003 (0.014)	0.059*** (0.019)	0.007 (0.012)	0.021 (0.014)	0.022** (0.009)	-0.001 (0.005)
$\Delta time_{1990-2006}$	0.016 (0.012)	0.018 (0.015)	0.016** (0.008)	0.009 (0.015)	0.013 (0.009)	0.012*** (0.004)

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. Control variables are the same as in the baseline regression presented in Table 2.5. Each cell corresponds to a different regression.

Sources: Population Census.

Table 2.13: Effect of RER on employment - identification strategy based on the comparison with the 1965 RER project

	(1)	(2)	(3)	(4)
<i>Dependant variable:</i>	$\Delta \log$ employment	$\Delta \log$ population	$\Delta \log$ employment	$\Delta \log$ population
	1975-90	1968-75	1975-90	1968-75
Intercept	-0.545** (0.256)	-0.518** (0.235)	0.12 (0.179)	-0.471* (0.258)
$\Delta time_{Paris}$ 1975-90	0.058** (0.028)	0.03 (0.02)	0.007 (0.022)	0.029 (0.019)
$time_{Paris}$ 1975	0.002 (0.005)	0.01** (0.005)	-0.002 (0.004)	0.011** (0.004)
$5km \leq d_{Paris} < 10km$	0.259* (0.132)	0.13 (0.107)	-0.139 (0.106)	0.003 (0.113)
$10km \leq d_{Paris} < 15km$	0.131 (0.111)	-0.136 (0.085)	-0.077 (0.093)	-0.113 (0.086)
$15km \leq d_{Paris} < 20km$	0.029 (0.104)	-0.045 (0.076)	-0.106 (0.078)	-0.093 (0.078)
$20km \leq d_{Paris} < 25km$	ref.	ref.	ref.	ref.
Share of farmland ₁₉₆₀	0.206 (0.171)	0.379** (0.16)	-0.084 (0.154)	-0.038 (0.205)
Surface	0.003 (0.007)	-0.002 (0.006)	-0.007 (0.006)	0.00006 (0.00653)
< 1km highway	0.104 (0.133)	-0.071 (0.08)	0.102 (0.126)	-0.145* (0.078)
Job density ₁₉₇₅	✓			
Job density ₁₉₆₈		✓		
Population density ₁₉₇₅			✓	
Population density ₁₉₆₈				✓
Number of observations	110	110	110	110
R ²	0.368	0.339	0.268	0.455

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes cities that should have not been connected to the RER network according to the 1965 plan.

Sources: Population Census.

Table 2.14: Effect of RER on population by level of education – alternative treatment variable

<i>Dependant variable:</i>	$\Delta \ln \text{population}_{75-90}$			
<i>Sample:</i>	All	Primary or middle school	Vocational or high school	Higher education
Intercept	0.024 (0.071)	-0.351 (0.764)	1.296* (0.731)	2.241*** (0.35)
RER ₁₉₉₀	0.067* (0.035)	0.062 (0.043)	0.076 (0.046)	0.133*** (0.049)
$5km \leq d_{Paris} < 10km$	-0.097 (0.066)	-0.223*** (0.075)	-0.223*** (0.084)	0.124 (0.116)
$10km \leq d_{Paris} < 15km$	-0.064 (0.069)	-0.161** (0.076)	-0.189** (0.081)	0.025 (0.092)
$15km \leq d_{Paris} < 20km$	-0.032 (0.064)	-0.059 (0.071)	-0.133* (0.074)	-0.027 (0.093)
Share of farmland ₁₉₆₀	0.061 (0.14)	0.154 (0.154)	0.042 (0.154)	-0.258* (0.139)
Surface	-0.003 (0.005)	0.003 (0.008)	-0.002 (0.006)	-0.008 (0.006)
< 1km highway	0.022 (0.059)	0.017 (0.067)	0.03 (0.066)	0.005 (0.073)
Pop density ₁₉₇₅ < 1000	0.162 (0.101)	-0.041 (0.286)	-0.23 (0.294)	-0.203 (0.154)
Pop density ₁₉₇₅ [1000, 2500]	0.222** (0.089)	0.088 (0.182)	0.016 (0.183)	0.087 (0.146)
Pop density ₁₉₇₅ [2500, 5000]	0.018 (0.057)	-0.07 (0.129)	-0.154 (0.123)	0.0005 (0.0888)
Pop density ₁₉₇₅ [5000, 10000]	-0.022 (0.023)	-0.048 (0.073)	-0.076 (0.065)	-0.029 (0.075)
Pop density ₁₉₇₅ > 10000	ref.	ref.	ref.	ref.
$\ln \text{dens. prim. or middle school}_{1975}$		0.028 (0.09)		
$\ln \text{dens. voc. or high school}_{1975}$			-0.117 (0.097)	
$\ln \text{dens. higher education}_{1975}$				-0.277*** (0.059)
Number of observations	96	96	96	96
R ²	0.349	0.291	0.39	0.562

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities. Sources: Population Census.

Table 2.15: Effect of RER on population by level of education – alternative identification strategy

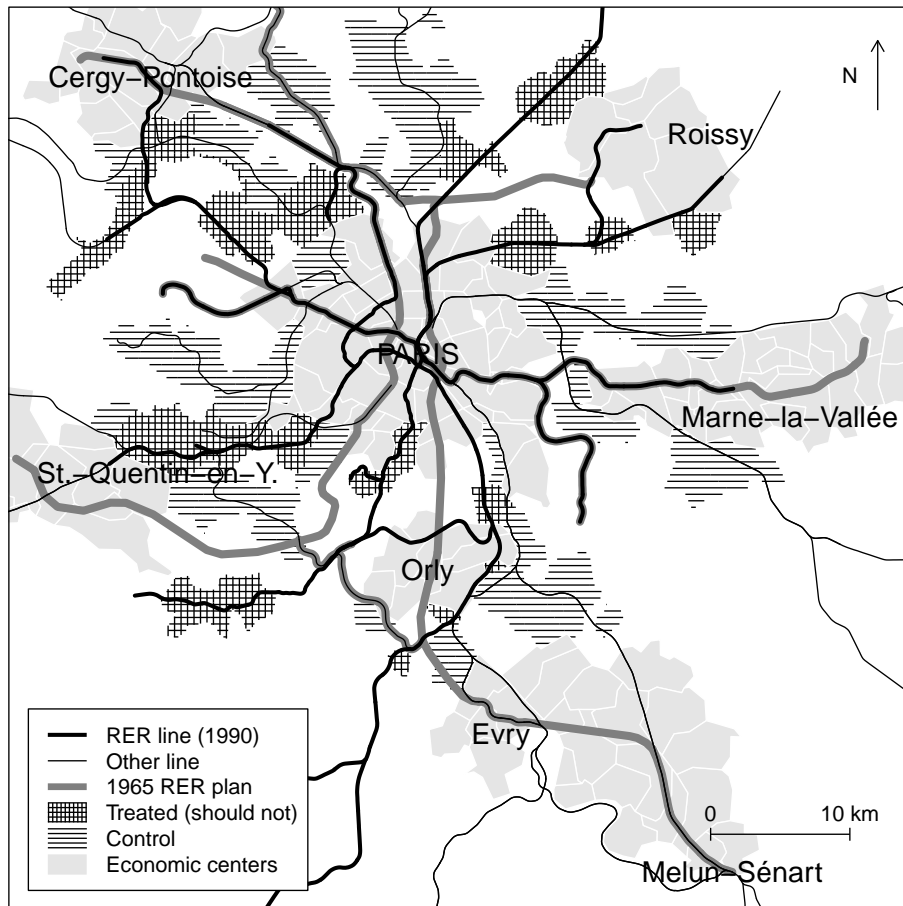
<i>Dependant variable:</i>	$\Delta \ln \text{population}_{75-90}$			
<i>Sample:</i>	All	Primary or middle school	Vocational or high school	Higher education
Intercept	0.12 (0.179)	-0.048 (1.21)	3.688** (1.842)	2.393*** (0.441)
$\Delta \text{time}_{Paris} 1975-90$	0.007 (0.022)	0.001 (0.024)	0.014 (0.028)	0.046* (0.025)
$\text{time}_{Paris} 1975$	-0.002 (0.004)	-0.0005 (0.0041)	-0.005 (0.005)	0.004 (0.004)
$5km \leq d_{Paris} < 10km$	-0.139 (0.106)	-0.171 (0.107)	-0.236* (0.136)	0.215* (0.127)
$10km \leq d_{Paris} < 15km$	-0.077 (0.093)	-0.128 (0.1)	-0.17 (0.117)	0.1 (0.091)
$15km \leq d_{Paris} < 20km$	-0.106 (0.078)	-0.076 (0.08)	-0.188* (0.095)	-0.086 (0.085)
Share of farmland ₁₉₆₀	-0.084 (0.154)	0.022 (0.187)	-0.337 (0.248)	-0.401** (0.177)
Surface	-0.007 (0.006)	0.0002 (0.0054)	-0.004 (0.006)	-0.01 (0.008)
< 1km highway	0.102 (0.126)	0.051 (0.104)	0.104 (0.113)	0.056 (0.108)
Pop density ₁₉₇₅ < 1000	0.447*** (0.152)	0.104 (0.372)	-0.649 (0.64)	-0.119 (0.187)
Pop density ₁₉₇₅ [1000, 2500]	0.301*** (0.083)	0.107 (0.216)	-0.313 (0.378)	0.023 (0.143)
Pop density ₁₉₇₅ [2500, 5000]	0.064 (0.055)	-0.048 (0.155)	-0.353 (0.238)	-0.034 (0.1)
Pop density ₁₉₇₅ [5000, 10000]	0.022 (0.025)	0.002 (0.083)	-0.178 (0.131)	-0.057 (0.088)
Pop density ₁₉₇₅ > 10000	ref.	ref.	ref.	ref.
$\ln \text{dens. prim. or middle school}_{1975}$		-0.012 (0.134)		
$\ln \text{dens. voc. or high school}_{1975}$			-0.406 (0.246)	
$\ln \text{dens. higher education}_{1975}$				-0.341*** (0.071)
Number of observations	110	110	110	110
R ²	0.268	0.18	0.416	0.55

Note: Standard errors in parenthesis. Significance levels: *** 1%, ** 5%, * 10%. All regressions are run on cities that had a train station in 1975, excluding economic subcenters and located between 5 and 25 km away from Paris. Treatment group includes only intermediate cities.

Sources: Population Census.

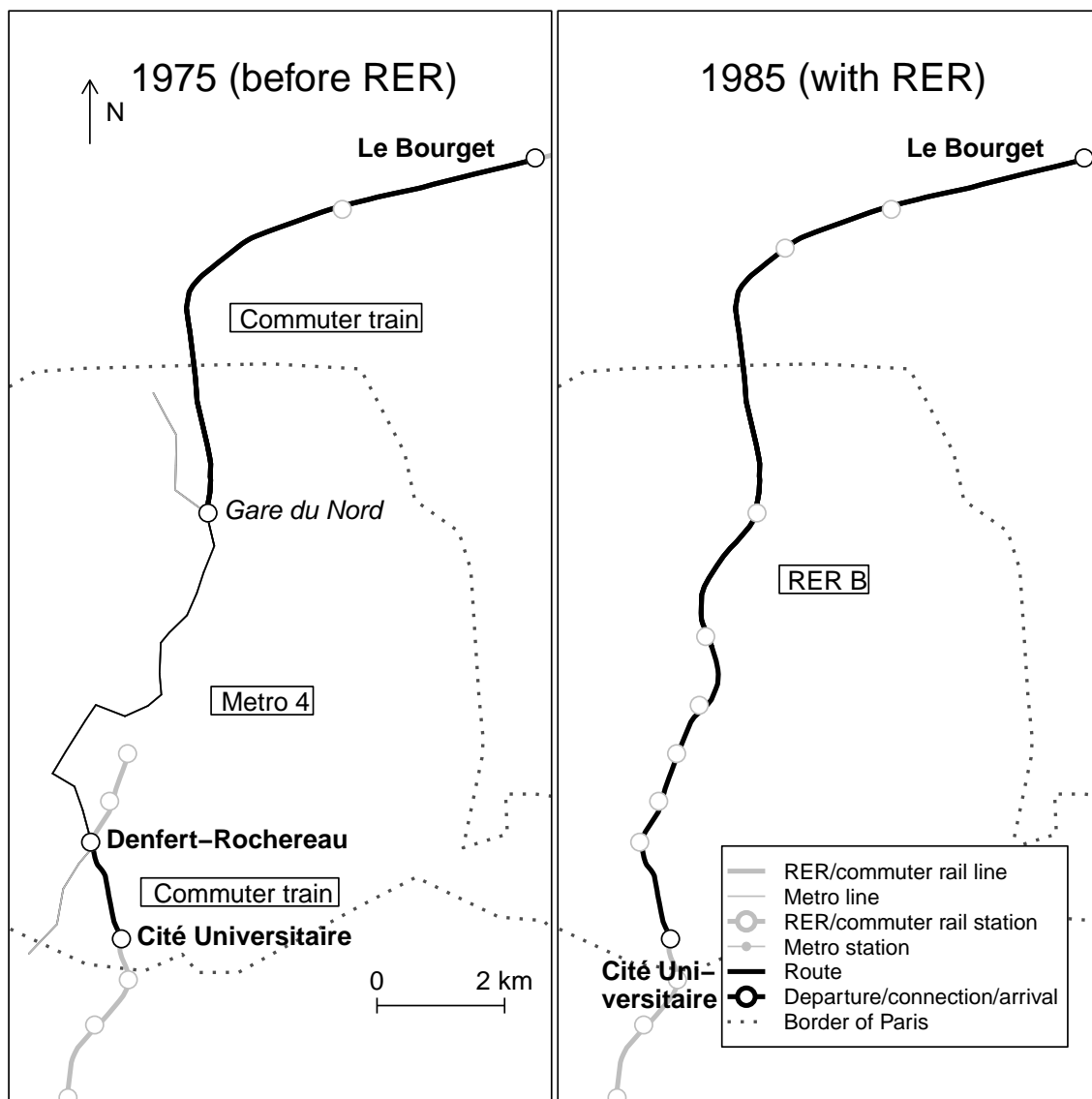
2.C Maps

Figure 2.3: Control and treatment groups – alternative identification strategy



Sources: IAU – Île-de-France.

Figure 2.4: Example of the route between *Le Bourget* and *Cité Universitaire* with the RER



Reading note: before the commissioning of the RER, the journey between *Cité Universitaire* and *Le Bourget* required to change train twice. First, one needed to take a commuter train station to the connection station *Denfert-Rochereau*. Then, one had to ride the metro line 4 to *Gare du Nord*. And finally, another commuter rail line final destination, *Le Bourget*. Thanks to the RER, it is possible to cross Paris from *Cité Universitaire* to *Le Bourget* without any connection, instead of two connections before, which lowers the journey time from 45 to 26 minutes.

Sources: IAU – Île-de-France.

Chapter 3

Is High-Speed Rail a Business Communication Technology? Evidence from French Multi-Plant Businesses

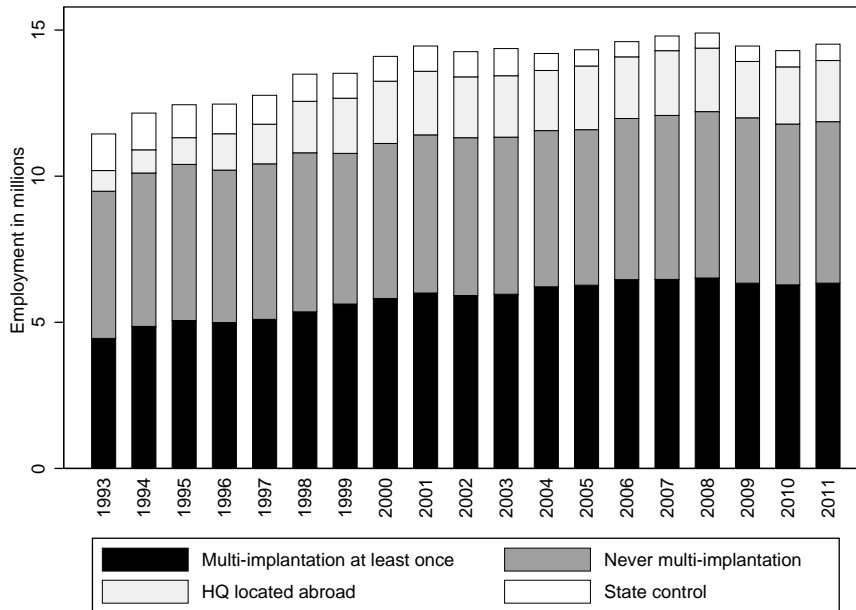
This chapter is cowritten with Pauline Charnoz (RITM-Université Paris Sud and Crest) and Claire Lelarge (Insee and Crest).

3.1 Introduction

Large corporations operating multiple affiliates located in different locations are prevalent and account for a disproportionate fraction of output and employment. Figure 3.1 shows that in France, geographically dispersed corporate groups account for around 40% (6 million workers in 2011) of total employment in the for-profit sector,¹ and even more when taking account of groups headquartered from abroad. The splitting and implantation decisions of such corporations involves a trade-off between the gains to access remote markets and the managerial costs to operate distant affiliates. That has been the object of study of a large literature in economic geography (e.g. Aarland et al., 2007, Davis and Henderson, 2008, Henderson and Ono, 2008, Strauss-Kahn and Vives, 2009) and in trade (see the recent survey about the prominent role of multinationals in Antràs and Yeaple, 2014). Yet, empirical contributions often propose analyzes that are reduced form in nature, such that in practice, very little is known about the way these business organizations are managed. In this paper, we contribute to fill this gap and provide detailed evidence about the nature of the higher managerial costs implied by geographic dispersion.

¹Excluding workers directly hired by households.

Figure 3.1: The prevalence of geographically dispersed corporate groups – Employment by type of business organization



Note: Employment is measured in terms of days of work, normalized by 360 (to be comparable with headcounts).

Sources: DADS and LIFI survey, for-profit sector (except agricultural activities and workers of the personnel service industries directly employed by households).

To that end, we take advantage of large scale administrative and survey data allowing us to describe the structure of the workforce of all French corporate groups, over almost 20 years (from 1993 to 2011). We furthermore use the expansion of the French high-speed rail (HSR) network over that period as a shock on communication costs between the headquarters and affiliates of groups which benefited from this new infrastructure.

Our empirical investigation is guided by theoretical predictions. The literature in corporate finance (Giroud, 2013, Giroud and Mueller, 2015) suggests that the geographical dispersion of affiliates might hamper information gathering and monitoring by the managers of the group’s headquarters, thus amplifying the moral hazard problems that characterize the management of remote affiliates. This literature predicts in particular that lower geographical dispersion, associated with lower travel times, could affect affiliate size positively. The literature in economic geography (Duranton and Puga, 2005) further suggests that the geographical dispersion of affiliates in a corporate group is related to the optimal mix of “functions” present in each implantation. A prediction of this strand of literature is that a reduction in communication costs between headquarters and affiliates lowers the cost of transferring headquarters services to remote affiliates and therefore decreases the need to locate the corresponding support activities at affiliates. This in turn increases the incentives to rationalize the mix of functions at affiliates and make them more focused on their production activities. Last, the literature in organizational eco-

nomics (Garicano, 2000, Garicano and Rossi-Hansberg, 2006, 2012, Antràs et al., 2006, 2008) predicts that workers within the corporate groups are differentially impacted by decreases in internal communication costs, depending on their skills. A robust prediction of this set of papers is that the need for skills decreases at affiliates, such that wages of the less skilled workers (and the associated labor costs) decrease.²

We test these predictions in the French data and implement different identification strategies that have been proposed in the literature to address the problem of the endogenous placement of the HSR infrastructure (use of high-dimensional fixed effects controlling for local and affiliate-level shocks as in Giroud, 2013 and evidence from un-realized lines as in Donaldson, 2014). Our regressions show that the impact of reduced travel times on affiliate size and functional specialization is higher in the service industries, where information to be transmitted is arguably softer (Petersen and Rajan, 2002). Results are also significant in the trade and manufacturing industries, but point estimates are lower. Our estimates allow to quantify the impact of the expansion of the HSR network on the management of remote affiliates. We obtain that for the average affiliate benefiting from the infrastructure, moving back to the 1980 rail network without HSR would induce a shift of roughly one job from administrative to operational activities in service industries (with associated increase in the productive capacity of the considered affiliate), against 20% of a job in other industries (retail, trade or manufacturing). We also obtain that affiliates in the manufacturing and business services industries experienced decreases in production labor costs, of around half the cost of a production job for the average affiliate. At the group level, our regressions suggest that the impact on the operational profit margin ranges from 0.5 to 1.5 percentage points depending on the industry.

Our results can also be used to provide estimates of the overall cost of geographical dispersion: for example, our estimates imply that remote affiliates in the personnel service industries would operate with roughly 1.5 additional production jobs if geographical distance could be fully abolished by a perfect communication technology. The figure would raise to 4 production jobs on average in business services industries, and to 2 production jobs in manufacturing industries. These quantifications can be interpreted as an upper bound for the “productivity” effect that can be expected from internal communication technologies. In this respect, our results also show (by revealed preferences) that face-to-face interactions made easier by the HSR technology remain crucial, in spite of the development of other means of communication (phone, e-mail, visio-conference) over the same period (Storper and Venables, 2004). Our results also show that business travellers (or their employers) are willing to pay a significant premium for reduced travel time, in spite of the arguably high comfort and “workability” of HSR coaches.

Last, our paper also contributes to the large literature investigating the economic impact of transport infrastructures. However, most papers in this field focus on standard rail or road infrastructure, which essentially generate a reduction in trade barriers which pertains mainly to the circulation of goods (Donaldson and Hornbeck, 2015). The impact of such shocks are now well understood (Melitz, 2003). They induce increases in the global volume of trade activities as well as in firm selection,

²Refer to Bassanini et al. (2015) and Landier et al. (2007) for an in-depth analysis of labor adjustments on the extensive margin (dismissals) depending on the distance to headquarters.

and ultimately aggregate productivity (Michaels, 2008, Datta, 2012, Banerjee et al., 2012, Donaldson, 2014, Faber, 2014, Ghani et al., 2015). In contrast, high-speed rail is a transportation technology that is almost only accessible to people.³ As previously explained, the mechanisms involved are therefore very different (Bernard et al., 2015, Nunn, 2007, Cristea, 2011).

The remainder of the chapter is organized as follows. Section 3.2 proposes a synthesis of theoretical predictions relating communication costs to the managerial organization of corporate groups and their overall performance. In section 3.3, we describe our data as well as the French HSR network. We also provide a comprehensive picture of the geographical dispersion of French corporate groups. Our empirical strategy is explained in section 3.4 and the main regressions results at the affiliate level are discussed in section 3.5. Section 3.6 provides additional descriptive results at the (entire) group level and section 3.7 concludes.

3.2 The management of multi-plant businesses: a review of theoretical predictions

In this section, we review three different (but non mutually exclusive) strands of the literature analyzing how travel times between headquarters and affiliates of corporate groups are likely to affect their managerial organization and performance. In each case, the testable empirical predictions are outlined.

3.2.1 Geographical dispersion and affiliate size

A recent literature in corporate finance (Giroud, 2013, Giroud and Mueller, 2015) relates travel times to information transmission between headquarters and affiliates in settings where there are information asymmetries and moral hazard problems. It delivers predictions linking travel times and affiliate size, as measured by e.g. employment.

These contributions specifically focus on the dual managerial structure of corporate groups, with both managers at headquarters (“principals”), who are endowed with the ultimate decision rights, and managers at remote affiliates (“agents”) who are not, but who have an informational advantage over managers at headquarters about the profitability of local investment projects. The management of such business organizations features a moral hazard problem if the interests of managers at affiliates are not fully aligned with the interests of managers at headquarters. Whether local managers over-invest (over-hire) or under-invest (under-hire) when decision rights are delegated to them depends on whether managers at affiliates have preferences for local “empire building” strategies, or conversely if they prefer an excessively “quiet life”. Bertrand and Mullainathan (2003) actually show that

³In the case of the French High Speed Rail program, most of the infrastructure is not even accessible to freight (at the notable exception of mail) because HSR tracks are too steep for the weight of freight trains. Notice that in this paper, we only focus on the “productivity” effect of HSR and do not consider the potential market creation impact of HSR, which could be particularly relevant in industries related to tourism. The analysis of this dimension would require an entirely different identification strategy.

the second case is more frequent among US managers, which implies that managers at affiliates are likely to under-invest when investment decisions are delegated to them. In such a setting, lower travel times between headquarters and remote affiliates increase monitoring and are therefore associated with higher investment (Giroud, 2013) and higher complementary employment (Giroud and Mueller, 2015) at affiliates.⁴

To test the relevance of such mechanisms in the French data, we replicate the analyzes in Giroud (2013) and Giroud and Mueller (2015) and investigate the correlations between employment at affiliates and travel time to their headquarters.⁵

3.2.2 Geographical dispersion and the functional specialization of affiliates

The literature in economic geography suggests that the geographical dispersion of affiliates in a corporate group is related to the optimal mix of “functions” present in each implantation. In Duranton and Puga (2005), firms are considered as bundles of two broad types of functions: “headquarter services” on one hand, and production activities on the other hand. These two functions can be either pooled in the same location or split into different plants. Splitting is costly, for example because of the agency problems outlined in section 3.2.1.⁶ However, there are gains to split firms when there exist “function specific” agglomeration economies, such as the possibility to outsource certain activities to local suppliers that might be specifically appropriate, the optimization of labor costs across local labor markets (depending on local labor supply), or simply market access for final products.

The distinctive prediction of Duranton and Puga (2005) is that a reduction in travel time lowers the cost of transferring headquarter services to remote affiliates, thus increasing the incentives to specialize affiliates by function.⁷ We therefore

⁴This reasoning assumes that investment decisions for affiliates are delegated to local managers. This needs not be the case (see section 3.2.2) but the same prediction holds (in expectation) under centralized control at headquarters when HQ managers are risk averse: easier information acquisition about the profitability of investment projects at remote affiliates decreases the “uncertainty premium” required by them and increases average investment.

⁵Unfortunately, our data do not enable us to observe investment at the affiliate level, such that for this variable, we will only be able to estimate regressions aggregated across all affiliates, at the group level (see section 3.6).

⁶Duranton and Puga (2005) model such mechanisms in a reduced form, as a fraction of managers’ time that is lost in travels to visit the remote affiliate. Refer to Acemoglu et al. (2007) for a more detailed description of the trade-offs involved: the optimal organizational choice between delegation of authority to a local manager or centralized decision taking at headquarters trades off the gain to rely the local manager’s superior information against the risk that he could use his informational advantage to make choices that are not in the best interest of the group as a whole. Shorter travel times ease information acquisition by principals, and shift the trade-off in favor of centralized control at headquarters.

⁷Notice that in Duranton and Puga (2005), this result is an equilibrium outcome: a decrease in the cost of remote management (if sufficiently large) shifts the entire economy from an equilibrium where no firm is geographically dispersed and cities specialize by sector, to an equilibrium where all firms adopt a multi-location organizational form and cities specialize by function. The authors suggest that a “smoother” result would hold in an augmented version of the model incorporating some firm level heterogeneity, together with (sufficiently large) sunk costs of reorganization. Such

expect them to be relatively more focused on their production activities and to discard the administrative tasks which are cheaper to complete at headquarters. This prediction can be tested by regressing the share of employment at affiliates that is devoted to production activities (as opposed to managerial activities) against travel time between affiliates and headquarters: we expect the sign of the corresponding coefficient to be negative.

3.2.3 Geographical dispersion and wages at affiliates

The literature in organizational economics (Garicano, 2000, Garicano and Rossi-Hansberg, 2006, 2012, Antràs et al., 2006, 2008) enables to make predictions about the way heterogeneous workers (in terms of skills) might be differentially impacted by decreases in internal communication costs within a same group. These papers show that business organizations (groups, or sub-units of groups such as affiliates) tend to organize as hierarchies because these types of organizations allow for the most parsimonious usage of two costly inputs of production: workers' time and workers' knowledge. In such structures, the bottom layer is specialized in the most common problems and concentrates the less skilled workers, while the upper layers only deal with exceptions (Garicano, 2000). Managers in one layer spend their time communicating with less skilled agents in the layer just below and solving some of the problems that are transmitted to them. They pass the rest to the layer with more skilled agents just above them.

This baseline representation of how productive activities organize is plugged into a two - region model in Antràs et al. (2006) and Antràs et al. (2008), which can easily be transposed to our setting. As in section 3.2.2, lower travel times, i.e. decreases in communication costs between units located in different regions, always increase the relative benefit to split businesses in order to take advantage of lower relative wages in the less dense areas; it also has a positive impact on overall group size. A more distinctive prediction of these papers is that lower communication costs increase the incentives to organize in more complex hierarchical organizations (having more layers) by making them a better “technology” to economize on knowledge. As a result, the relative role of managers at headquarters increases, while it is profitable to decrease the knowledge content of bottom production workers at affiliates (and therefore, their skills), in order to save on their wage. We test this prediction by regressing the wage of low-skill production workers on travel time, and expect a positive sign for this coefficient.⁸

additional dimensions of firm heterogeneity would explain why all firms would not split instantaneously and relocate all of their units along the new HSR lines as they open.

⁸Section 3.2.1 also generates predictions related to wages. In this set-up, a decrease in communication costs would alter the wage contracts of managers at affiliates from rather high- to rather low- powered incentive contracts. Unfortunately, we are not able to test this prediction with our data.

3.2.4 Geographical dispersion and operational profit at the group level

In all models of sections 3.2.2 to 3.2.3, decreases in travel time between headquarters and affiliates always decrease the overall costs of operating large, multi-plant businesses. At the group level, we therefore expect a negative correlation (other things equal) between the operating profit margin and average travel time to affiliates. Furthermore, corporate groups which are cheaper to operate should grow larger, other things equal. We therefore also expect a negative correlation between overall group size and average travel time to affiliates. While employment is the only reliable proxy of affiliate size in the French data (see section 3.3 below), there are several proxies available at the group level, most importantly: employment, value added, or tangible investment.

3.3 Data

3.3.1 The geographical organization of corporate groups

The first ingredient of our analysis is the information system allowing us to recover the structure of French corporate groups. We rely on the LIFI⁹ files and use the information on the (direct and indirect) equity stakes of headquarters in affiliate companies reported in this data source. Prior to 1999, the LIFI files only covered companies of the private sector whose size was above at least one of three different thresholds, defined in terms of financial stakes in other firms (higher than 1.2 million euros), sales (60 million euros) or employment (500 workers). From 1999 onwards, these files are complemented with the Diane-Amadeus (Bureau Van Dijk) dataset, which is constructed from commercial court records and which covers smaller business groups. For most of our period of analysis, our file therefore contains almost exhaustive information about corporate groups operating in France.

We follow the standard approach in corporate finance since the classic contribution by La Porta et al. (1999) and define headquarters of potentially complex group structure as the main plants of units having the actual ultimate control over all assets in the group, *via* the direct or indirect ownership of more than half of the equity in any of the group affiliates.¹⁰ Affiliates correspond to all other plants of the considered group. However, previous empirical work (e.g. Aubert and Sillard, 2005, Picart, 2004) has documented that establishments (and even companies within groups) might be created, terminated and replaced for reasons that are uncorrelated

⁹The acronym “LIFI” stands for “LIaisons FInancières” (financial linkages). See e.g. Boutin et al. (2013) for a previous use of this dataset. Complementary exhaustive fiscal data (BRN files, also used in Boutin et al., 2013) provide the accounting information required in our empirical analysis.

¹⁰In cases where these headquarters are non-employer holdings, we rather choose the employer company that is most directly related to the holding, in terms of rank of control, and in cases where several companies meet this criterion, we select those having the largest share of executives (see section 3.3.2 below). The aim of this procedure is to locate (probabilistically) the upper management team of the entire group structure.

with the human resource management practices we want to focus on¹¹. We choose to abstract from such phenomena by aggregating the information across all plants of a same group, having the same activity (at the 1 digit level) and located in the same municipality (“*commune*”) into a single “affiliate” unit.¹²

This dataset allow us to complement figure 3.1 in the introductory part and provide in figure 3.2 a synthetic description of the geography of remote control, as of 2011. Panel (A) provides for each commuting zone, the share of businesses that are under the control of headquarters located outside the commuting zone, i.e. further away than the standard daily commuting patterns. This share is higher than 15% in most commuting zones, and above 20% in a number of zones located in the northern half of the country. This fact is driven by the disproportionate “sphere of corporate influence” of Paris over this part of the country.¹³ The employment weighted version of the indicator presented in panel (B) shows that in most of the country, more than 30% of employment is managed at arm’s length. This share rises to rates above 50% in a significant number of commuting zones, mostly located in the northern part of the country.

3.3.2 The organization of the workforce within corporate groups

We complement the previous files with exhaustive worker level information sourced from the DADS¹⁴ files. These files are available from 1993 onwards, and include roughly 14 million workers per year in the recent period. They allow us to track economically active plants within each group and provide us with a rich description of their internal workforce and wage structure.

Most importantly, the classification of occupations in the DADS files allows us to contrast the workforce allocated to production activities with the workforce allocated to managerial activities, both at headquarters and affiliates. This distinction is in particular required to test the empirical predictions of section 3.2.2. We interact this typology of functions with the indicators of hierarchical layers proposed in Caliendo et al. (2015), in order to test the predictions of section 3.2.3 (see Table 3.1).

Figure 3.3 describes the structure of the workforce obtained with these defi-

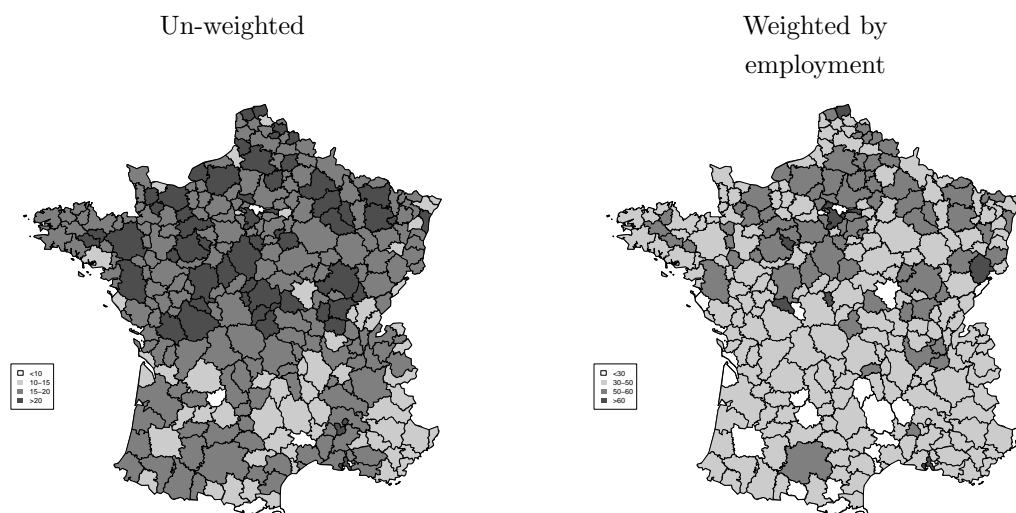
¹¹In particular, firm and plant identifiers also change when the legal status of companies evolves, most often because of regulatory constraints (e.g. upper bounds on the admissible number of shareholders for certain legal forms, *etc.*) or for fiscal or administrative reasons which are entirely orthogonal to the mechanisms described in section 3.2. Our aggregation procedure also enables to abstract from plant transfers occurring on very short distances (within the same municipality), for reasons related to e.g. building capacity, etc.

¹²This aggregation is not drastic, since there are ca. 36,000 such municipalities across France. Its main benefit is to increase by a little bit the power of our setting by increasing the number of years an affiliate unit is observed - 3.2 years on average. Notice also that the variations of travel time induced by HSR line openings are homogenous within municipalities, since they are typically served by only one single station.

¹³See appendix 3.B for a comparison of the “spheres of corporate influence” of different French cities showing the disproportionate weight of Paris, as compared with any other French city.

¹⁴The acronym “DADS” stands for “Déclarations Annuelles de Données Sociales”. See e.g. Caliendo et al. (2015) for a previous use of these files.

Figure 3.2: Share of affiliates under control of a HQ located outside the considered commuting zone (in percentages, by 2011)



Notes: The left panel describes the number of affiliates in each commuting zone that are under the control of a HQ located outside the zone, as a share of the total number of businesses (affiliates and HQs) located in each zone. The right panel describes the share of private employment in each commuting zone that is under control of an external HQ. This indicator can be interpreted as an employment weighted version of the previous.

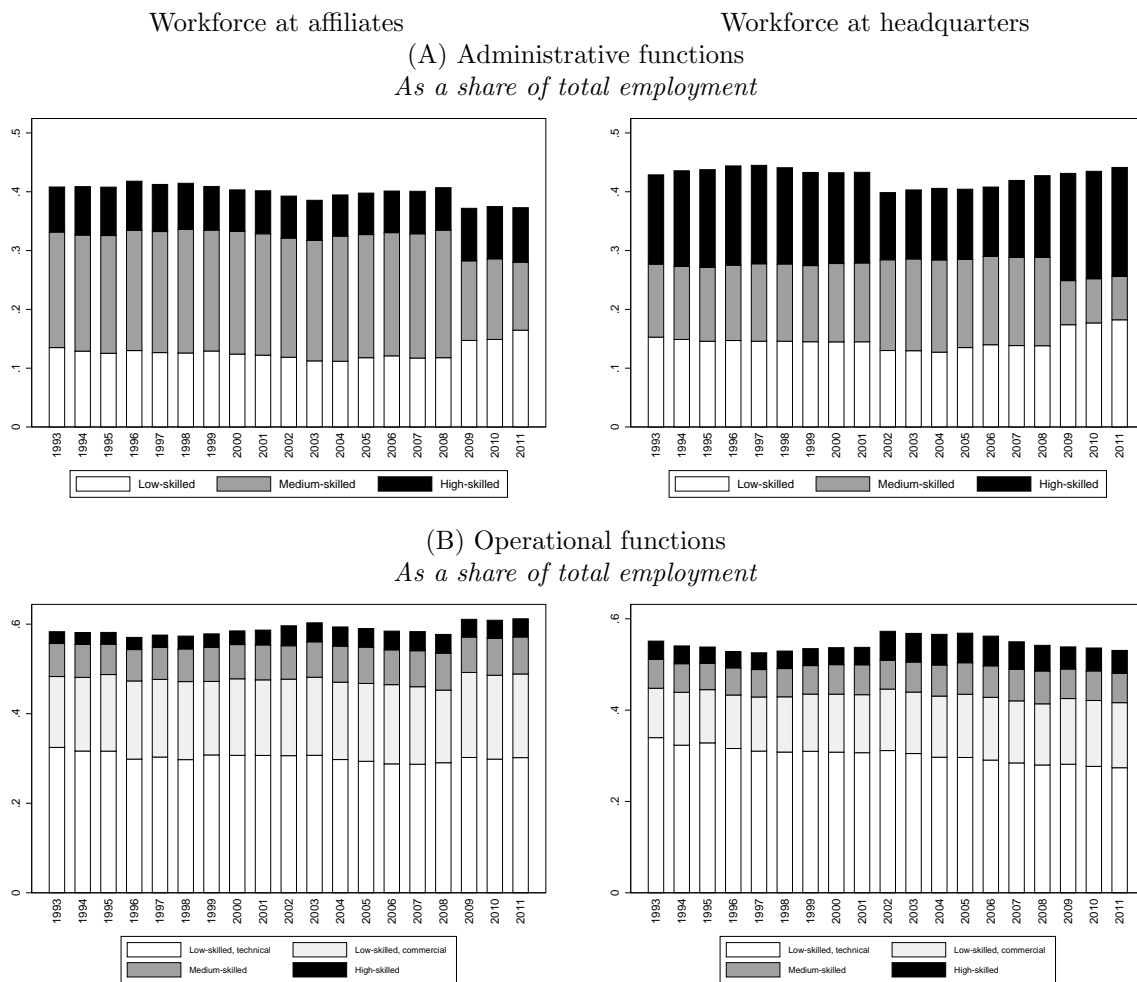
Sources: DADS and LIFI survey, covering the for-profit sector (except agricultural activities and workers of the personnel service industries directly employed by households).

nitions, both at affiliates and headquarters.¹⁵ Unfortunately, two methodological changes in the coding of occupations occurred in 2002 and 2009 and produced two breaks in the series.¹⁶ However, abstracting from this difficulty, panel (A) of figure 3.3 shows that managerial functions represent a similar share of the workforce, on average, at headquarters and affiliates. What differentiates HQs from affiliates sharply is not the weight of these activities, but the structure of skills *within* them. Headquarters employ 15 to 20% of their workforce in higher management positions, against 5 to 10% in the case of affiliates. Conversely, affiliates employ around twice as many middle managers (ca. 20%) as headquarters. In contrast, the structure of the workforce allocated to production activities (panel B in figure 3.3) is not highly contrasted between headquarters and affiliates: headquarters only tend to concentrate more high-skilled production workers. However, unreported complementary descriptive statistics confirm (unsurprisingly) that the structure of the workforce at affiliates is highly differentiated across industries. For example, skilled production and managerial workers represent 11% of the workforce in manufacturing industries and 21% in business services. In contrast, the share ranges between 6 and 8% in the personnel services, retail and trade, or transport industries. The share of managers

¹⁵In these graphics, employment in each occupation is measured in days (between start and end of the labor contract of each worker) to take part time work into account.

¹⁶Our empirical strategy, which saturates each regressions with rich temporal fixed effects, is relatively immune to this measurement issue (see section 3.4).

Figure 3.3: Structure of the workforce at affiliates vs. headquarters between 1993 and 2011



Notes: Employment is measured in days. The breaks in the series in 2002 and 2009 were generated by a change in the codification procedure for occupations in the DADS files.

Sources: DADS and LIFI, units which are part of geographically dispersed groups.

Table 3.1: Type of occupation and the French occupational classification (codes in parentheses)

	Management position	Production occupation
Low-skilled	Office workers (clerks, 54)	Skilled industrial and manual workers (62 and 63), drivers (64), skilled transport and wholesale workers (65), unskilled industrial workers (67)
Medium-skilled	Mid-level managers and professionals and related (42-46)	Technicians (47), supervisors and foremen (48)
High-skilled	Heads of businesses (2), top managers and professionals (35, 37). These correspond to the “managers” in section 3.2	Science and educational professionals (34), technical managers and engineers (38)

Note: we allocate commercial low-skilled workers to production activities, which is of particular relevance for the retail and trade industries. For medium and high-skilled workers, unfortunately, the classification available in our file does not allow to distinguish between managerial and commercial activities.

(high-skill managerial workers) is around 4% in all industries, except in the business services (10%).¹⁷

3.3.3 HSR network and rail travel times

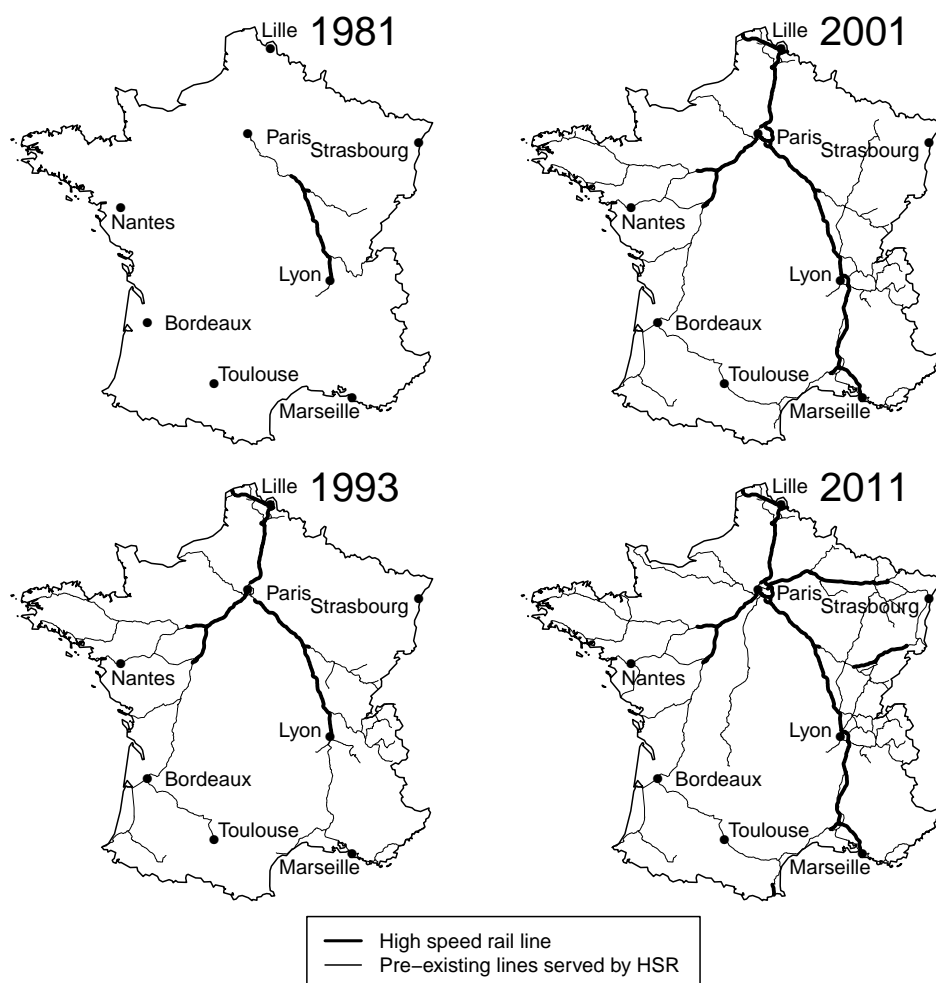
The last ingredient for our empirical analysis is the detailed information about the evolution of the rail network over time, which allows us to compute rail travel times between any two points of France, and more specifically, between the headquarters and affiliates of French corporate groups. We collected detailed information from the archives of the French national rail company and from its open-data platform, and complemented it with various technical publications available from rail fan web sites to reconstruct the expansion of the HSR network over time. The outcome of this task is represented on figure 3.4. The first HSR segment was opened in 1981 on the track between the two French largest cities, Paris and Lyon. This line was subsequently extended in 1994 and 2001 to ultimately reach Marseille on the Mediterranean Coast. The network was also extended towards the Atlantic coast in 1989-1990, towards Lille and London in 1993 and 1994, and towards Strasbourg and Frankfurt (in Germany) in 2007.¹⁸

The expansion of the HSR network had a huge impact on rail travel times across the territory because high-speed trains operate at twice the maximum standard rail speed, ca. 320km/h on the dedicated infrastructure. While the actual procedure we

¹⁷In the latter case, most probably part of them are in fact allocated to production (but non “technical”) activities, which might unfortunately generate some attenuation bias in our analysis.

¹⁸As of today, high-speed rail service also includes cross-border services to UK, Belgium, Netherlands, Germany, Switzerland, Italy and Spain. However, this feature of the network is not analyzed in the present paper, since we are not able to locate HQs abroad with our data.

Figure 3.4: Evolution of the French HSR network between 1981 and 2011

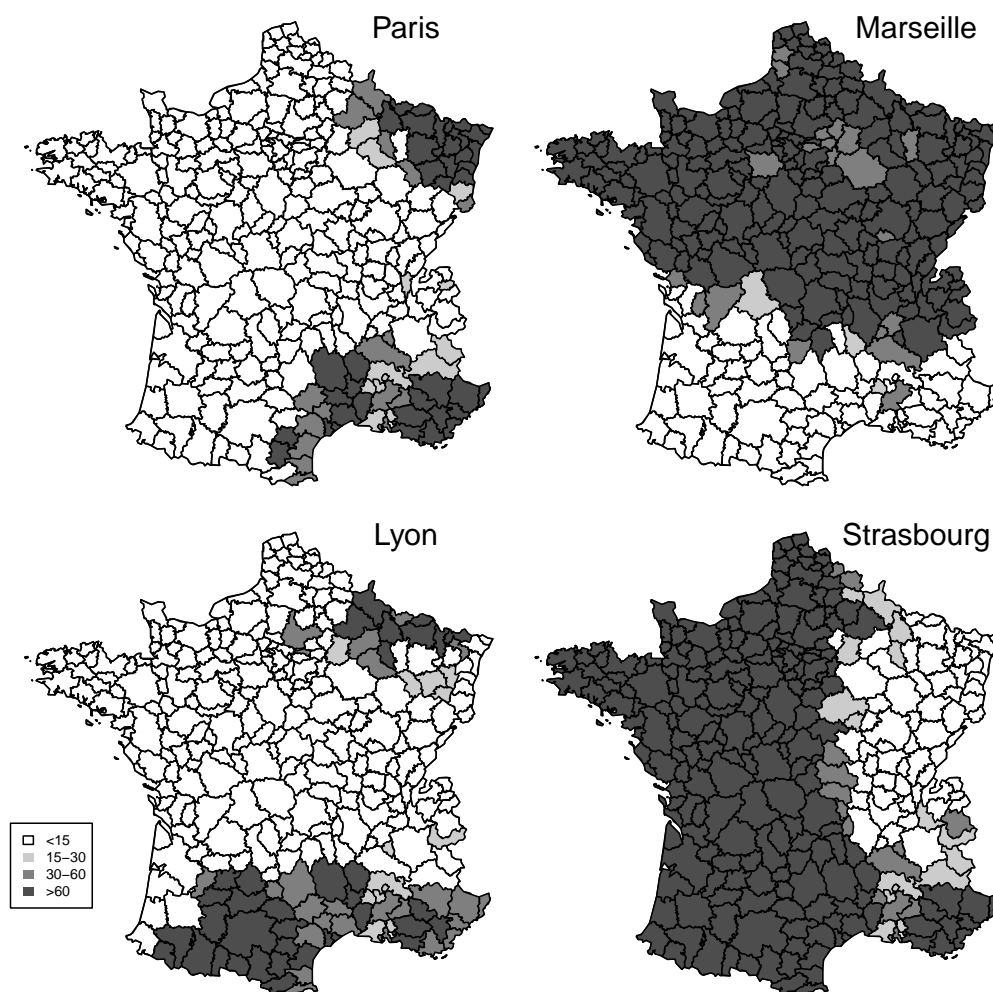


Sources: Archives and open-data platform of the French national rail company; rail fan web sites. Authors' computations.

implemented to compute rail travel times is relegated to appendix 3.C, figure 3.5 shows how the expansion of the HSR network translated into reductions in travel times for selected destinations over our period of analysis. Between 1993 and 2011, new line openings mainly benefited the Eastern and Southern regions, which experienced the largest accessibility gains, both in terms of time and geographical range. Paris experienced the symmetric gains towards the Mediterranean zone and the Eastern zone. A few examples enable to gauge orders of magnitude: rail travel time between Marseille and Paris decreased from 6h40 to 4h40 in 1982, to 4h18 in 1994 and ultimately to 3h00 in 2001. Between Strasbourg and Paris, travel time decreased from 3h55 to 2h20 in 2007, when the Eastern line opened.

In our empirical analysis, all of the indicators of travel times between headquarters and affiliates are based on the above rail travel time computations, and all travel time variations (reductions) are driven by the opening or extension of new HSR lines. One important concern is of course that our indicator misses all travel time reduc-

Figure 3.5: Reduction in rail travel times to selected destinations (in minutes, between 1993 and 2011)



Sources: archives and open-data platform of the French national rail company; rail fan web sites. Authors' computations.

tions which might be driven by other means of transportation, and more specifically, by airlines. Appendix 3.A however shows that over our period of analysis, HSR became one of the most popular means of transportation for long distance travels, such that rail travel times became of practical relevance to managers. Appendix 3.A also shows that the airline industry did not seem to be a strong competitor of HSR over the same period, since it rather lost market shares in spite of the liberalization episode of the 90s. This is most presumably because travel time by air (incorporating access time to airports, which are often located in the outskirts of cities while train stations are typically located at city centers) is not lower than rail travel time for most domestic trips.¹⁹ In all cases, as also argued in detail in appendix 3.A, we expect our approximation to bias our regression results against finding any impact

¹⁹See Behrens and Pels (2012) for a similar argument in the case of the London-Paris passenger market.

of travel time on management practices.

A second, more minor problem is that HSR line openings were almost always associated with improved rail service beyond travel time.²⁰ For example, new and more comfortable coaches were most of the time introduced, with increased “workability” and a higher frequency of train services, at least for terminal cities.²¹ Conversely, the quality of service offered by standard rail might have suffered from the reallocation of resources of the rail company towards HSR. In our empirical analysis, such unobserved differential evolution of the rail services provided by HSR and standard rail (which is correlated with our indicator of travel time variation) would bias our estimates somewhat upwards.

3.4 Empirical strategy

3.4.1 Baseline identification strategy

Our equations of main interest are estimated at the affiliate level, and investigate the correlations between travel time and the various outcome variables discussed in section 3.2, typically: affiliate employment, the share of employment allocated to production activities, or the wages of those production workers. They take the following generic specification:

$$y_{ijlt} = \beta \cdot T_{ijlt} + \varepsilon_{ijlt} \quad (3.1)$$

where subscript i denotes the affiliate, j its headquarters, l denotes the commuting zone where the affiliate is located and t denotes time. T_{ijlt} denotes travel time between the affiliate and its headquarter and y_{ijlt} is the outcome of interest.

We first argue that travel time variations, which are all driven by the evolution of the HSR network, are less likely to be endogenous in our setting than *levels* of travel times, since the location of affiliates (relative to their headquarters) is potentially endogenously driven by unobserved characteristics that are also correlated with the outcome variables. For example, a higher specificity of the production of the affiliate²² might be correlated with both a higher proximity of the considered affiliate to its headquarters, and higher wages paid to its production workers. To address these concerns, we insert affiliate level fixed effects α_{ijl} into our regression

²⁰A related concern is that our results might be driven by standard market access mechanisms rather than by those described in section 3.2 if the new HSR infrastructure was accessible to freight and also impacted the transport of goods. This is however not the case of the dedicated HSR infrastructure, which is too fragile and sometimes too steep to be accessible to freight trains (because of their weight). Note furthermore that our empirical strategy would anyway address this potential concern (see sections 3.4.1 and 3.4.3).

²¹This last point is more debated for the case of certain smaller or middle size cities, more precisely those that are bypassed by HSR services while they were previously served by traditional rail service (FNAUT, 2011, Emangard and Beaucire, 1985). For example, the number of direct services per day from Paris to Charleville - Mézières fell from 7 to 3 in 2007, and from 9 to 4 between Paris and Tourcoing.

²²Regressions are estimated industry by industry to further address this concern and to investigate industry level heterogeneity.

framework, such that in all regressions, the relation between travel time and the various outcome of interest is identified by *changes* in travel time, namely those generated by the expansion of the HSR network over time.

However, this regression framework is still affected by the fact that the placement of the HSR infrastructure might be endogenous. For example, there might exist some local unobserved heterogeneity, e.g. local growth potential, which could have determined the governmental decision to build the HSR infrastructure, and which might be also correlated with our outcome variables, thus biasing our results. Furthermore, the HSR infrastructure itself might have boosted local growth, a phenomenon we want to control for in our regressions. We address these two issues using an identification strategy similar to Giroud (2013) and Giroud and Mueller (2015), by introducing large sets of commuting zone \times time “fixed” effects (α_{lt}), on top of the affiliate level fixed effects (α_{ijt}):

$$y_{ijt} = \alpha_{ijt} + \alpha_{lt} + \beta \cdot T_{ijt} + \gamma \cdot X_{jt} + \varepsilon_{ijt} \quad (3.2)$$

These many dummy variables capture the time varying *local* heterogeneity which could generate the above endogeneity problems. Notice that all of these fixed effects are identifiable in our setting, because the impact of variation in travel times on the outcomes of interest is identified *jointly* from the locations of affiliates *and* from the location of their headquarters, i.e. travel time is a *dyadic* variable. More intuitively, our identification strategy amounts to use as a control group, the set of local affiliates located in the same commuting zone as the considered affiliate: indeed, these affiliates are therefore exposed to the same local shocks, but they are unaffected (or differently affected) by travel time reductions because of a different location of their headquarters²³.

Such specifications that are saturated with fixed effects require the explanatory variables to be measured accurately, since fixed effects tend to amplify the attenuation bias arising from measurement errors.²⁴ However, we argue in appendix 3.C.3 that if travel times might be affected by some measurement problems, *variations* in travel times are likely to be measured much more accurately. Since identification in equation 3.2 is in differences, this is what is required. Furthermore, this problem should if anything lead us to minimize the true impact of travel time on our outcome variables.

In terms of the estimation method, the inclusion of several sets of high-dimensional fixed effects renders estimation non-trivial, despite the fact that equation 3.2 is fully linear. We choose to apply the iterative procedure proposed by Guimaraes and Portugal (2010): its principle is to iterate on sets of normal equations that are conveniently defined. The only practical constraint in our implementation of their methodology is to use *continuous* empirical proxies for the explanatory variables of main interest, in order to insure identification (see appendix 3.E for full details).

²³In the reported regressions, we exclude affiliates which are not part of a multi-implantation group, mainly for practical reasons (this reduces drastically our file size and the associated computing time). However, these observations could serve as additional controls for local shocks. Un-reported regressions show that results are basically unaffected by this choice.

²⁴This problem is well known in the literature about the estimation of production functions, where capital is typically not measured accurately (Griliches and Hausman, 1986).

3.4.2 Robustness Checks

We also provide a bunch of checks to test the robustness of our results to additional potential sources of endogeneity. First, additional controls for group level market conditions X_{jt} , both on the domestic and the international markets, are incorporated in all specifications to control for group-wide shocks which might be spuriously temporally correlated with travel time reductions (and our outcome variables) in our sample. This strategy is however not sufficient in cases where group-wide shocks are *endogenously* correlated with HSR line openings: this would happen, for example, in cases where the group lobbied in favor of certain HSR line openings and extensions.²⁵ To mitigate this concern, we first estimate regressions where we simply remove the most “suspicious” observations, namely the largest affiliates in each area. Second, we experiment with specifications where we remove large HSR beneficiaries, i.e. affiliates benefiting from HSR for more than 50% of the rail track to their headquarters.²⁶

3.4.3 Further evidence from un-realized lines

To mitigate an even broader set of endogeneity concerns, but at the cost of discarding a larger fraction of the sample, we also implement an additional strategy along the lines of Donaldson (2014). This strategy makes use of the fact that a governmental plan was drafted in 1991 (ahead of our period of analysis) and endorsed by the Prime Minister of the time. It described an ambitious network of HSR lines to be built in the years to come, based on local development and profitability criteria (Ministère de l'Équipement, 1991).²⁷ However, some of the lines were not implemented (Zembri, 1997). In some cases it was because their expected profitability was deemed too low:²⁸ we choose to discard these lines. For the remainder, non-implementation was explained by the fact that budgetary constraints imposed some phasing and rescheduling of projects, or because of unexpected technical difficulties²⁹ that are arguably exogenous in our empirical setting.

²⁵Giroud (2013) also considers the possibility of lobbying at the affiliate (rather than group) level. We think that this case is very unlikely for HSR given the cost of the infrastructure. However, the specifications suggested in the previous case, where we remove the largest affiliates or discard affiliates benefiting from HSR on a too large proportion of the track to their headquarters, would also mitigate the concern of lobbying at the affiliate level.

²⁶This strategy is very similar to what is implemented in Giroud (2013), in specifications where he only considers indirect flights where either the last leg of the flight (involving the plant's home airport) or the first leg of the flight (involving headquarters' home airport) remains unchanged.

²⁷See appendix 3.D for a map of the foreseen infrastructures. The lines which were ultimately realized as of 2011 are: the Eastern line, the Rhin/Rhone connection, the Parisian inter-connection, the Provence and Rhone-Alps line. Unrealized lines are: the Auvergne, Limousin and Normandy lines (featuring low expected profitability even in the initial blueprint document of 1991), and our actual “counterfactual lines”: Aquitaine, Brittany, Far South, inter-connection in the Alps, Mediterranean Coast (French Riviera), Languedoc-Rousillon, Midi-Pyrénées, Pays de la Loire and Picardy.

²⁸This is the case of lines connecting Paris to the center of the country (Auvergne, Limousin), or to Normandy, which all had expected profitability below 4.3% - the minimum attained for actually implemented lines (Eastern line).

²⁹For example, in some cases, expensive art works or deviations were required by local authorities or lobbyists to preserve the environment (e.g. protected areas, vineyards, *etc.*) - see Zembri (1997).

We suggest that affiliates which would have benefited from the latter subset of the 1991 plan are likely to have the same un-observables (potentially correlated with travel time reductions) as affiliates which actually benefited from realized HSR lines. In more technical terms, this would imply that conditional on being part of the 1991 plan, travel time is orthogonal to the error term in equation 3.2. We therefore propose to estimate our baseline specification on the corresponding sub-sample, after discarding affiliates which could never expect benefiting from the HSR technology because of their location relative to their headquarters. This is an even more flexible procedure than simply inserting the variable indicating insertion into the 1991 plan as a new control, as a strict implementation of the Donaldson (2014) methodology would imply. In our specific setting, it also clarifies the fact that the coefficient of interest is in fact mainly identified from this sub-sample of affiliates. Indeed, the actually implemented HSR network is broadly nested into the 1991 plan, such that there remains very little variation in travel times in the sub-sample of affiliates outside the 1991 plan to contribute to identification. Notice that this procedure is not strictly speaking an instrumental variable strategy. Indeed, we do not argue that the placement of the 1991 plan was more exogenous than the placement of the actually implemented network, as would have been required for an instrumental variable. We only suggest that the factors determining actual implementation (i.e. the mapping between the 1991 plan and the actually implemented network) are likely to be relatively orthogonal to our relation of interest, such that the information about insertion into the 1991 plan captures the unobserved heterogeneity potentially generating remaining endogeneity issues.

3.4.4 Sample descriptive statistics

Table 3.2 provides a comprehensive description of our sample. About 40% of the affiliates of geographically dispersed corporate groups benefit from HSR on the track to their headquarters. This high share is mainly driven by the fact that the first HSR lines, opened between 1981 and 1993, connected particularly dense areas (Paris and Lyon). Only 4% of affiliates of geographically dispersed corporate groups benefited from the rail travel time reductions induced by the HSR network expansion which occurred between 1993 and 2011. In terms of corporate groups, we obtain that a slightly higher share (7%) were affected via at least one of their affiliates.³⁰ These affiliates are on average located further away from their respective headquarters than affiliates which did not experience any rail travel time reduction: this is due to the fact that HSR is typically a long distance mean of transportation (section 3.3.3 and appendix 3.A). Their headquarters are also more often located in Paris, which is explained by the shape of the HSR network: it is typically organized as a spider web centered on the capital city (see our earlier comment of figure 3.4). Table 3.2 shows that the average travel speed between an affiliate and its headquarters is around 80 km/h when the HSR technology is not available, and around 110-120km/h when it is available on at least part of the track. The latter value is much lower than the

³⁰Figures 3.10 and 3.12 in appendix 3.F complement table 3.2 and provide a full description of the distribution of travel time reductions at the dates of the main line openings, as well as the precise geographical location of affiliates which benefited from them.

HSR commercial speed (320km/h), which indicates that most affiliates only benefit from HSR on a small portion of the track to their headquarters.

Table 3.2 also provides a breakdown of the industry structure of our sample. It shows that the sample of affiliates which benefited from the 1994 line opening was somewhat specific and presented a higher share (than the population average) of affiliates active in manufacturing and transport industries. This specificity is explained by the industrial specialization of the areas which became connected to the HSR network at that date, and translates into somewhat differentiated structural characteristics: larger affiliate sizes, and somewhat lower shares of production workers.

Table 3.2: Summary statistics for affiliates of multi-location firms – 1993-2011

	Affiliates experiencing reductions in travel time between 1993 and 2011				Affiliates never affected
	1993-2011	At main years of change:			
		1993/1994	2000/2000	2006/2007	
<i>Geography:</i>					
Distance to HQ (km)	402	475	482	326	195
HQ in Paris	40%	46%	37%	37%	34%
<i>Travel times (hours):</i>					
Travel time	3.84	4.13	4.11	3.10	2.06
Speed	-	115	117	105	95
HSR user	82%	100%	100%	100%	41%
<i>Employment of affiliates:</i>					
# of employees	38	40	31	31	32
Share of prod. workers	65%	61%	63%	66%	66%
Prod. wkr wage (k€/yr)	15.5	14.8	14.5	16.9	15.9
Share of managers	6%	8%	7%	6%	7%
<i>Industry affiliation:</i>					
Manufacturing industries	19%	24%	17%	16%	19%
Personnel services	9%	12%	9%	7%	11%
Retail and trade	48%	35%	46%	51%	46%
Business services	17%	20%	20%	19%	18%
Transport	7%	10%	8%	6%	6%
<i>Counts of business units:</i>					
Nb obs (aff. × year)	198,088	5,802	9,728	7,253	2,109,167
Nb affiliates	28,207	//	//	//	699,928
Nb HQs (groups)	9,114	2,161	3,568	2,429	126,745

Notes: The main “years of change” correspond to the opening dates of the Northern line (1993/1994), of the connection to Marseille (2000/2001) or of the Eastern line (2006/2007).

Sources: DADS and LIFI; business units (HQs or affiliates - see section 3.3.1 for definition) which are part of multi-location groups between 1993 and 2011. Employment is measured in days of work but is reported in this table as headcount equivalents (days divided by 360).

3.5 Results at the affiliate level

3.5.1 Main specification

Table 3.3 contains our main results at the affiliate level. Petersen and Rajan (2002) suggest that the mechanisms relating organizational structure and the use of information presented in section 3.2 are most relevant in industries where information (e.g. about investment opportunities) to be transmitted is particularly “soft”. In their view, this is the case when activity relies heavily on relationship building, such as consulting, research and new product development, and more generally service industries. Consistently with this hypothesis, we obtain that the negative relationship between the functional specialization of affiliates and travel times (column 1) and between affiliate size (as measured by production workers, in column 4) and travel times are significant in all industries, but highest in industries related to services: personnel services, business services and transport. In the latter industries, the functional specialization of affiliates into production activities increases by around 2 percentage point when travel times to their headquarters decrease by one hour. In the personnel and business service industries, the functional shift is associated with increases in production capacity (as measured by production workers) of more than 5 percent by hour saved on travel times. Since the average travel time between affiliates and headquarters is around 2 hours (table 3.2) and given that average affiliate size is around 15 and 30 in the personnel and business services industries respectively, this indicates that the average cost of geographical dispersion in terms of production capacity amounts to roughly 1.5 production jobs in the personnel services industries, and to 4 production jobs in the business services industries. However, these effects are exactly compensated by the symmetric decreases in managerial jobs (most often, high-skilled managerial jobs), such that the overall impact on total affiliate employment is not significant.

In the manufacturing, retail and trade industries, point estimates are lower, but still highly significant. In these sectors, the functional specialization of affiliates into production activities increases by a little less than 1 percentage point when travel times to their headquarters decrease by one hour. Production capacity as measured by production workers increases by around 2 percent per hour saved, which translates into an average cost of the geographical dispersion of around 2 jobs in manufacturing industries and two thirds of a job in the retail and trade industries. These results broadly confirm the empirical findings of Giroud (2013) and Giroud and Mueller (2015) for the US manufacturing industry.

Last, in column (5) of table 3.3, we investigate the relationship between travel times and the wage of production workers. The obtained coefficient is most often positive, but only significant in the manufacturing and business services industries. This seems unsurprising, since we expect the knowledge based mechanism in section 3.2.3 to be more relevant in those most skill intensive industries, which feature twice as many skilled workers as other industries.³¹ Associated magnitudes are relatively large: the average geographical dispersion within French corporate groups, requiring 2h of travel time between affiliates and headquarters, would translate into

³¹See section 3.3.2 for basic descriptive statistics.

Table 3.3: Impact of travel time reduction to HQ on the structure of the workforce at affiliates from 1993 to 2011

	Structure of		Affiliate		Wage (ln)
	the workforce (shares)		employment (ln)		
	Production workers	Managers (high-skilled)	Total	Production workers	Production workers
	(1)	(2)	(3)	(4)	(5)
Manufact. industry	−0.009*** (0.002) <i>426,595</i>	0.002* (0.001) <i>426,595</i>	−0.013 (0.009) <i>426,595</i>	−0.019* (0.010) <i>408,861</i>	0.008** (0.004) <i>408,861</i>
Personnel services	−0.020*** (0.005) <i>241,846</i>	0.018*** (0.002) <i>241,846</i>	0.005 (0.016) <i>241,846</i>	−0.055*** (0.019) <i>227,936</i>	0.009 (0.008) <i>227,936</i>
Retail and trade	−0.008*** (0.002) <i>1,045,869</i>	−0.002*** (0.001) <i>1,045,869</i>	−0.022*** (0.005) <i>1,045,869</i>	−0.030*** (0.006) <i>965,013</i>	−0.004 (0.002) <i>965,013</i>
Business services	−0.018*** (0.003) <i>401,844</i>	0.000 (0.002) <i>401,844</i>	−0.009 (0.011) <i>401,844</i>	−0.070*** (0.015) <i>329,68</i>	0.013* (0.008) <i>329,68</i>
Transport	−0.016*** (0.005) <i>138,865</i>	0.012*** (0.003) <i>138,865</i>	0.018 (0.016) <i>138,865</i>	0.006 (0.022) <i>115,205</i>	0.001 (0.009) <i>115,205</i>

Note: the number of observations of the regression is in italic. All regressions include affiliate \times headquarter level fixed effects, as well as local (commuting zone) \times time fixed effects to control for the local market conditions. Regressions also include group level exports in total sales to capture the cycle on international markets (but all results are robust to the exclusion of these controls). Sources: DADS and LIFI; affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

a 2% increase in production costs driven by the higher wages paid out.

3.5.2 More on economic magnitudes: an illustrative quantification exercise

Table 3.4 enables to gauge the magnitudes implied by the regression results in table 3.3 in a more intuitive way. In this table, we compute the adjustments that would take place in the population of affiliates which are in activity in the last year of our panel (2011), in the counterfactual situation where the HSR network would be reduced to the lines already in service in 1993, at the beginning of our estimation period, or if it was fully closed (i.e. back to the rail network of the 1980's). Of course, this simple exercise does not take account of the potential response of competing modes of transportation. It is also essentially a partial equilibrium exercise, but as we will show, the aggregate impacts we estimate are not large enough to be likely to generate large general equilibrium effects, even on local labor markets.

Table 3.4: Magnitudes at the affiliate level: organizational impact of HSR as of 2011

			Manuf. indus. (1)	Person. serv. (2)	Retail. trade (3)	Bus. serv. (4)	Trans- port (5)
Affiliates benefiting from HSR in 2011	#		10,698	8,322	30,274	15,566	4,489
	%		0.438	0.413	0.435	0.518	0.491
Mean	2011 (observed)		3.307	3.315	3.347	3.351	3.502
travel time to HQ	w/ 1993 network		3.721	3.732	3.775	3.792	3.956
	w/ 1980 network		4.673	4.801	4.714	4.861	4.996
Prod. jobs	2011/1993	jobs	0.196***	0.224***	0.057***	0.342***	0.288***
		ppt	0.004***	0.008***	0.003***	0.008***	0.007***
	2011/1980	jobs	0.633***	0.787***	0.212***	1.168***	1.020***
		ppt	0.012***	0.030***	0.011***	0.026***	0.024***
Manager jobs	2011/1993	jobs	-0.046*	-0.204***	0.017***	-0.005	-0.212***
		ppt	-0.001*	-0.008***	0.001***	-0.000	-0.005***
	2011/1980	jobs	-0.150*	-0.719***	0.064***	-0.018	-0.750***
		ppt	-0.003*	-0.027***	0.003***	-0.000	-0.017***
Wages of of prod. jobs	2011/1993	€	-4,224**	-1,142	327	-3,978*	-438
		ppt	-0.003	-0.004	0.002	-0.006	0.001
	2011/1980	€	-13,744**	-4,108	1,234	-14,687*	-1,574
		ppt	-0.011	-0.013	0.005	-0.020	-0.002

Sources: Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011. Magnitudes are computed using estimates reported in table 3.3.

Results obtained from this simple exercise are suggestive:³² for the average affiliate in the service industries, the functional shift induced by the availability of the HSR network (as a whole) amounts to the shift of roughly one job from managerial (and more precisely, high-skill managerial) activities to production activities. This represents a reallocation of ca. 3% of the workforce for the average affiliate size. In the other industries, given the differences in the estimated coefficients in table 3.3 and the differences in average affiliate sizes, the magnitude of the functional shift is only 20% of a job. Furthermore, we obtain that total savings in terms of production labor costs induced by the HSR network are of the order of magnitude of half the cost of a production job for the average affiliate in the manufacturing and business services industries.

3.5.3 Common trend assumption and non-linearities

The discussion of our identification strategy in section 3.4.1 shows that our setting is similar to a difference-in-differences setting, where we contrast affiliates experiencing changes in travel times to their headquarters with affiliates located in the same

³²The specific impact associated with the subset of HSR lines opened after 1993 is unsurprisingly lower (by about one fourth) than those discussed in the main text.

area which did not experience the same changes, because of a different location of their headquarters. To test the common trend identifying assumption underlying such a setting, and to investigate potential anticipation effects (or conversely, lagged adjustment processes), we run a specification where we include lags and leads of our indicator of travel time. For compactness, we only discuss here the results obtained for our indicator of functional specialization (the share of employment allocated to production activities) in the first three columns of table 3.5, but the discussion would also hold for our other outcome variables.

If anything, we detect anticipation effects in the business and personnel services industries;³³ lagged adjustments only occur in the transport industry. Overall, although the simultaneous inclusion of three indicators of travel times is probably too demanding in our setting, this experiment shows that the response to changes in travel times occurs mostly in the year where affiliates experience them in the manufacturing, personnel services, retail and trade and business services industries.

Columns (4) to (6) in table 3.5 propose a different experiment. In this second specification, we investigate whether our main results in table 3.3 might hide potential non-linearities. We split our travel time variable into three different classes: short travels, lasting less than 3 hours, medium length travels, lasting between 3 and 5 hours and which would still be feasible (round trip) in one day, and longer travels, lasting more than 5 hours. We obtain that in all industries, the relation between travel time and functional specialization is low and insignificant for the shortest travels. In contrast, the relation is always highly significant for medium range travels, as well as for the longest trips (except in the case of the retail and trade industry). These findings are consistent with the fact that the market share of HSR is highest for the longest trips, as shown in table 3.A of appendix 3.12.

3.5.4 Alternative Identification Strategies

Table 3.6 presents a series of important experiments, where we vary the identification strategy in order to test the robustness of our findings. As previously, for compactness, results are only presented for our indicator of functional specialization (the share of employment allocated to production activities). Column (1) simply replicates the results of table 3.3 as a benchmark. In columns (3) and (5), we only use each third or fifth year in our panel, in order to identify the impact of travel time on longer time differences than in the baseline specification. The cost of this strategy is obviously the drastic decrease in sample size that is induced, but results are fully preserved in this experiment: we actually tend to obtain higher point estimates when increasing the length of the time difference. This finding might however partly be driven by the selection of survivors that is induced by this experiment.

In columns (5) and (6), we provide two experiments aiming at testing whether our results might be driven by “lobbyists”, i.e. by a very specific sub-population

³³Unreported regressions using a wider time window show that these anticipation effects do not exceed one year.

Table 3.5: Impact of travel time reduction to HQ on the share of production jobs in affiliates – Anticipation *vs.* adjustment effects and non-linearities from 1993-2011

	Anticipations and adjustments			Non-linear impact of travel time		
	Travel time at:			Travel time at t:		
	$t - 1$	t	$t + 1$	shorter than 3h	3h to 5h	longer than 5h
	(1)	(2)	(3)	(4)	(5)	(6)
Manufacturing	0.000 (0.003)	-0.008* (0.004)	-0.001 (0.004)	-0.004 (0.003)	-0.009** (0.004)	-0.015*** (0.004)
Personnel serv.	-0.012* (0.007)	-0.011 (0.008)	-0.002 (0.007)	-0.008 (0.007)	-0.030*** (0.007)	-0.024** (0.009)
Retail & trade	-0.004* (0.002)	-0.006** (0.003)	0.002 (0.002)	-0.001 (0.002)	-0.020*** (0.003)	-0.004 (0.003)
Business serv.	-0.012*** (0.004)	-0.010** (0.005)	0.001 (0.004)	-0.008* (0.005)	-0.010** (0.005)	-0.038*** (0.005)
Transport	0.011 (0.007)	-0.007 (0.008)	-0.023*** (0.007)	-0.009 (0.006)	-0.023*** (0.007)	-0.021** (0.009)

Note: the dependent variable is the share of production jobs. All regressions include affiliate \times headquarter level fixed effects, as well as local (commuting zone) \times time fixed effects to control for the local market conditions. Regressions also include group level exports in total sales to capture the cycle on international markets. Same samples and numbers of observations as in table 3.3. Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

of affiliates which might have influenced the placement of the HSR infrastructure because they would receive disproportionate benefits from HSR, as explained in section 3.4.2. In columns (5), we estimate a specification where we remove the largest affiliates in each commuting zone, assuming that the latter are most likely to be at the source of the endogeneity concerns related to potential lobbying activity. Our results are however fully preserved in this regression. In column (6), we propose another experiment which is directly inspired from Giroud (2013).³⁴ In this specification, we only use affiliates which only benefit from HSR for less than 50% of the track to their headquarters. These observations are less likely to have lobbied in favor of the HSR line than those benefiting from it on the entire track to their headquarters. Again, results are fully preserved in this experiment: if anything, we obtain higher point estimates than in the baseline specification.

Last, column (6) of table 3.6 contains the evidence obtained from unrealized lines (see section 3.4.3). The rationale behind this alternative strategy is almost opposite to the two previous experiments: here, we restrict our estimation sample to affiliates having the same probability to have behaved as lobbyists when the 1991 governmental plan was decided. Again, results are preserved, and somewhat amplified in the cases of the manufacturing and business services industries.

³⁴The setting in Giroud (2013) is very similar to ours, since this author estimates the effects of headquarters' proximity to plants on plant-level investment and productivity using the opening of US airlines. To mitigate concerns related to lobbying, he proposes specifications where he only considers indirect flights where either the last leg of the flight (involving the plant's home airport) or the first leg of the flight (involving headquarters' home airport) remains unchanged.

Overall, results obtained from the different identification strategies in columns (4) to (6) suggest that our baseline specification with high-dimensional fixed effects controlling for shocks at the commuting zone level seem to be sufficient to address the problem of the endogenous placement of the HSR infrastructure.

Table 3.6: Impact of travel time reduction to HQ on the share of production jobs in affiliates from 1993 to 2011 – Alternative identification strategies

	Baseline	Longer time differences:		Removing largest	Partial HSR track only	1991 plan only
	(1)	3 years (2)	5 years (3)			
Manufacturing industry	−0.009*** (0.002) <i>426,595</i>	−0.009** (0.005) <i>156,189</i>	−0.017** (0.008) <i>86,121</i>	−0.008*** (0.002) <i>423,677</i>	−0.014*** (0.003) <i>369,587</i>	−0.019*** (0.004) <i>209,537</i>
Personnel services	−0.020*** (0.005) <i>241,846</i>	−0.009 (0.011) <i>89,653</i>	−0.049*** (0.018) <i>53,552</i>	−0.020*** (0.005) <i>238,931</i>	−0.022*** (0.007) <i>209,326</i>	−0.026*** (0.007) <i>116,566</i>
Retail and trade	−0.008*** (0.002) <i>1,045,869</i>	−0.006* (0.003) <i>382,129</i>	−0.002 (0.005) <i>230,819</i>	−0.008*** (0.002) <i>1,043,053</i>	−0.008*** (0.002) <i>917,541</i>	−0.007** (0.003) <i>464,929</i>
Business services	−0.018*** (0.003) <i>401,844</i>	−0.016** (0.006) <i>148,698</i>	−0.035*** (0.012) <i>90,025</i>	−0.017*** (0.003) <i>399,065</i>	−0.030*** (0.004) <i>334,041</i>	−0.025*** (0.005) <i>202,592</i>
Transport	−0.016*** (0.005) <i>138,865</i>	−0.032*** (0.009) <i>50,795</i>	−0.024* (0.015) <i>30,714</i>	−0.016*** (0.005) <i>136,052</i>	−0.024*** (0.007) <i>117,185</i>	−0.015* (0.008) <i>65,123</i>

Note: The number of observations of the regression is in italic. All regressions include affiliate \times headquarter level fixed effects, as well as local (commuting zone) \times time fixed effects to control for the local market conditions. Regressions also include group level exports in total sales to capture the cycle on international markets.

Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

3.5.5 Adverse effect of travel time to other affiliates

In table 3.7, we follow Giroud and Mueller (2015) and investigate whether the adjustments described in table 3.3 might be affected by travel time reductions affecting the other affiliates of the group. To that end, we insert into the regression, variables describing average gains in travel time for other affiliates in the group.

Columns (1) and (2) investigate potential adverse effects on affiliates' production capacity (as measured by employment allocated to production). Theory on internal capital markets predicts that if the group is financially constrained, then a decrease in the relative cost of operating other affiliates should boost their growth, but lead to a decline in the resources allocated to other affiliates (Stein, 2002), which would

negatively affect their size. Consistently with this mechanism, we obtain that while our earlier estimates of table 3.3 are preserved in column (1) for the relationship between affiliates' own travel time and affiliate size, their production capacity is on average negatively affected by gains in travel times at other affiliates. Point estimates for the latter coefficient are of the same order of magnitude as those obtained for affiliates' own travel time, which suggest that the reallocations of production capacity are of the same order of magnitude.

In contrast, columns (3) and (4) show that functional specialization tends to be slightly fostered (but not impaired) by gains in travel time at other affiliates, except in the business services industries. This finding might be driven by the fact that HQ manager time that is saved at other affiliates (due to shorter travels)³⁵ is partially reallocated to remote affiliates, thus decreasing the need for local managers and fostering their functional specialization. Overall, table 3.7 suggest that HQ managerial time might be a particularly scarce resource (or a particularly costly input) in large and spatially dispersed groups.

Table 3.7: Impact of travel time reduction to other affiliates in the group from 1993 to 2011

<i>Dependent Variable:</i>	Production jobs (ln)		Share of prod. workers	
	Own	Gains at	Own	Gains at
	travel time	other affiliates	travel time	other affiliates
	(1)	(2)	(3)	(4)
Manufacturing ind.	-0.023** (0.010)	-0.023*** (0.002)	-0.009*** (0.002)	-0.000 (0.000)
Personnel services	-0.058*** (0.019)	-0.026*** (0.004)	-0.020*** (0.005)	0.002** (0.001)
Retail and trade	-0.053*** (0.021)	-0.035*** (0.005)	-0.007*** (0.002)	0.002*** (0.000)
Business services	-0.088*** (0.015)	-0.066*** (0.004)	-0.018*** (0.003)	-0.002*** (0.001)
Transport	-0.008 (0.022)	-0.021*** (0.005)	-0.016*** (0.005)	0.002** (0.001)

Note: All regressions include affiliate \times headquarter level fixed effects, as well as local (commuting zone) \times time fixed effects to control for the local market conditions. Regressions also include group level exports in total sales to capture the cycle on international markets. Same samples and numbers of observations as in table 3.3.

Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

³⁵We will show in section 3.6 that managerial jobs which disappear at affiliates are in fact on average transferred to HQs, such that gains in travel times increase the resources in HQ managerial time both *via* time saved during travels and *via* transfers of managerial jobs to HQs.

3.6 Descriptive extensions at the group Level

3.6.1 Specification

Some important theoretical predictions hold at the group level rather than at the affiliate level, most notably predictions relating travel time to overall group size and profitability (section 3.2.4). We therefore complement the previous regressions with specifications on the level of the entire group:

$$y_{jt} = \alpha_j + \sum_r \alpha_{rt} \cdot \delta_{jrt} + \beta \cdot T_{jt} + \gamma \cdot X_{jt} + \varepsilon_{jt} \quad (3.3)$$

In this equation, identification of the impact of average travel time to affiliates T_{jt} on group size or profitability is still identified from variations thanks to the inclusion of the group level fixed effects α_j . We also insert a set of dummy variables³⁶ α_{rt} capturing market conditions in the areas where the group operates. However, since corporate groups in our sample are typically geographically dispersed, these dummy variables are not mutually exclusive, such that it is not possible to implement the same estimation procedure as previously (appendix 3.E). To render estimation computationally tractable with standard OLS routines, we have to aggregate the circa 300 commuting zones into the 22 French metropolitan regions prevailing over the period. Additional controls X_{jt} in equation 3.3 include the number of sites to be managed (including the headquarters themselves, in logarithm), such that all results are to be interpreted “per implantation”. As for regressions at the affiliate level, we also insert export intensity (as previously) to capture international market conditions when relevant.

An important concern for these regressions estimated at the group level is that average travel time to affiliates T_{jt} might evolve for two different types of reasons: either because travel times decrease as the HSR network expands (as previously), or because the scope of the group evolves, i.e. affiliates enter or exit. This aspect is the main weakness of our regressions at the group level: indeed, we show in appendix 3.G that adjustments on this “extensive margin” are not orthogonal to the variations in travel times induced by the HSR network, but this concern is difficult to address (Giroud, 2013).³⁷ As a first attempt to control for this problem, we split the travel time term into two parts:

³⁶Results are robust to the use of employment shares in each zone rather than simply dummy variables indicating where the group operates.

³⁷This concern was already present, though less severe, in section 3.5.5.

$$\begin{aligned}
 T_{jt} &= \frac{\sum_{i \in \mathcal{F}_{jt}} T_{(ij)t}}{\text{Card } \mathcal{F}_{jt}} - \underbrace{\frac{\sum_{i \in \mathcal{F}_{jt_0}} T_{(ij)t_0}}{\text{Card } \mathcal{F}_{jt_0}}}_{\substack{\text{Normalization term} \\ \text{(factored out by group level fixed effects)}}} \\
 &= \underbrace{\left(\frac{\sum_{i \in \mathcal{F}_{jt}} T_{(ij)t}}{\text{Card } \mathcal{F}_{jt}} - \frac{\sum_{i \in \mathcal{F}_{jt}} T_{(ij)t_0}}{\text{Card } \mathcal{F}_{jt}} \right)}_{\text{Change in average travel time}} + \underbrace{\left(\frac{\sum_{i \in \mathcal{F}_{jt}} T_{(ij)t_0}}{\text{Card } \mathcal{F}_{jt}} - \frac{\sum_{i \in \mathcal{F}_{jt_0}} T_{(ij)t_0}}{\text{Card } \mathcal{F}_{jt_0}} \right)}_{\text{Change in group spatial dispersion}} \quad 3.4)
 \end{aligned}$$

where \mathcal{F}_{jt} denotes the set of affiliates in group j at date t and \mathcal{F}_{jt_0} the set of affiliates in group j in the first year the group is observed in our data (date t_0). As previously, T_{ijt} is travel time between affiliate i and the headquarters of group j at date t . Of course, the spatial dispersion term is affected by variations in travel times induced by the wider availability of the HSR technology, but most likely only marginally, as suggested in appendix 3.G: for example, travel times only explain around 5 percentage point of a total of 43% of affiliate exits to be explained.³⁸ In spite of this limitation, this term allows us to purge the term measuring changes in travel times from changes in geographic dispersion at the group level.

3.6.2 Results

Results depicting the correlations between travel times or geographic dispersion and group size and profit are reported in table 3.8. We obtain first that the indicator of spatial dispersion is consistently negatively correlated with all indicators: group size as measured by value added, employment or investment, and profit margin. Since all regressions include controls for the number of affiliates, this means that on average, the production units of geographically dispersed corporate groups are smaller, and present a lower operational profit margin than production units of concentrated corporate groups.³⁹

In terms of our indicator of main interest, travel times, the most robust pattern is the negative relationship that is obtained between travel times and profit margin across all industries. Table 3.9 provides the associated orders of magnitude using the same counterfactual exercise as in section 3.5.2: we obtain that if multi-location groups active in 2011 could not rely on HSR for some reason and only used the 1980 rail network, then their profit margin would decrease by 0.5 percentage point on average (and even by 2 percentage points in the transport industry), which is sizable. The correlation obtained between investment and travel times is also consistently negative, which is consistent with Giroud (2013), but results are only significant in the retail, trade and transport industries. Last, we obtain a negative relationship between travel times and group size as measured by value added or employment in the

³⁸Market conditions have much more explanatory power in the regressions modeling affiliate reshuffling of appendix 3.G.

³⁹Note however that it does not imply that *total profits* of geographically dispersed groups are lower: by revealed preferences, if such structures exist, then the converse is necessarily true.

Table 3.8: Spatial dispersion and performance at the group level from 1993 to 2011

	Value Added (ln)	Employ- ment (ln)	Invest- ment (ln)	Profit Margin
Travel time				
Manufacturing ind.	-0.067*** (0.014)	-0.026* (0.014)	-0.037 (0.035)	-0.008** (0.003)
Personnel services	-0.107*** (0.024)	-0.074*** (0.025)	-0.064 (0.067)	-0.010* (0.006)
Retail and trade	-0.049*** (0.013)	-0.049*** (0.012)	-0.146*** (0.034)	-0.011*** (0.003)
Business services	0.029** (0.014)	0.082*** (0.015)	-0.015 (0.030)	-0.007** (0.003)
Transport	-0.017 (0.024)	0.040* (0.024)	-0.232*** (0.059)	-0.017*** (0.005)
Spatial dispersion				
Manufacturing ind.	-0.017*** (0.002)	-0.032*** (0.002)	-0.020*** (0.004)	-0.003*** (0.000)
Personnel services	-0.044*** (0.002)	-0.042*** (0.002)	-0.003 (0.006)	-0.004*** (0.001)
Retail and trade	-0.017*** (0.001)	-0.028*** (0.001)	-0.013*** (0.004)	-0.004*** (0.000)
Business services	-0.041*** (0.002)	-0.059*** (0.002)	-0.066*** (0.004)	-0.003*** (0.000)
Transport	-0.008** (0.003)	-0.025*** (0.003)	-0.034*** (0.007)	-0.002*** (0.001)

Note: All regressions include group level fixed effects, as well as dummy variables describing in which region the group operates, interacted with years (to capture local market conditions). Regressions also include the logarithm of the total number of sites (HQ and affiliates) as well as group level exports in total sales to capture the business cycle on international markets. Number of observations: 225,942 for manufacturing industry, 131,829 for personnel services, 406,310 for retail and trade, 249,263 for business services and 52,936 of transport.

Sources: DADS and LIFI, groups owning affiliates in different locations (commuting zones) for at least one year between 1993 and 2011.

manufacturing, personnel services and retail and trade industries. In those sectors, point estimates that are obtained for value added are consistently higher than those obtained for employment, which implies that higher travel times are associated with lower labor productivity - or conversely, that reduced travel times allow managerial organizations to be more efficient and parsimonious in labor. In the business services and transport industries, we do not obtain negative correlations between group size and travel times, but the result for labor productivity still holds.⁴⁰

To conclude, table 3.10 provides a simple test at the headquarter level of the overall delegation story that was suggested by regressions at the affiliate level. More precisely, we check that the geographical dispersion of corporate groups is associated with fewer administrative workers, and more precisely fewer high-skilled managers at HQs relative to affiliates, which would be consistent with increased delegation of

⁴⁰In the business services and transport industries, we actually obtain a positive correlation between size and travel times, which could be driven by a particularly strong productivity effect.

Table 3.9: Magnitudes at the group level – operational impact of HSR as of 2011

			Manuf. Indus. (1)	Person. Serv. (2)	Retail, Trade (3)	Bus. Serv. (4)	Trans- port (5)
Groups		#	2,723	1,303	4,527	5,216	869
benefiting from HSR		%	0.461	0.333	0.363	0.513	0.527
Average	in 2011		2.544	2.447	2.242	2.406	2.858
travel time	w/ 1993 network		2.811	2.704	2.494	2.710	3.232
to HQ	w/ 1980 network		3.384	3.287	2.966	3.402	3.972
Employ- ment	2011/1993	jobs	3.082*	4.456***	2.134***	-6.193***	-4.031*
		ppt	0.007*	0.019***	0.012***	-0.025***	-0.015*
	2011/1980	jobs	11.031*	16.706***	7.777***	-20.174***	-14.062*
		ppt	0.022*	0.062***	0.036***	-0.082***	-0.045*
Invest- ment	2011/1993	k€	114	27	51***	7	2488***
		ppt	0.010	0.016	0.037***	0.004	0.087***
	2011/1980	k€	429	103	194***	36	5136***
		ppt	0.031	0.054	0.106***	0.015	0.258***
Profit margin	2011/1993	ppt	0.002**	0.003*	0.003***	0.002**	0.006***
	2011/1980	ppt	0.006**	0.008*	0.008***	0.007**	0.019***

Sources: DADS and LIFI, groups owning affiliates in different locations (commuting zones) for at least one year between 1993 and 2011. Magnitudes are computed using estimates reported in table 3.8.

authority to local managers of affiliates when travel times are higher. The obtained patterns are fully consistent with this hypothesis in all industries. Furthermore, point estimates are higher than those that were reported for adjustments at the affiliate level in table 3.3: this would imply that the downward adjustments in managerial resources at remote affiliates induced by decreases in communication costs are more than compensated by the symmetric adjustments in managerial resources at headquarters.

3.7 Conclusion

In this paper, we documented the impact of travel time between affiliates and headquarters of geographically dispersed corporate groups on the management of such business organizations. Guided by theory, we tested these predictions on the population of French corporate groups, using the expansion of the High Speed Rail network as an arguably exogenous shock on internal travel times. We obtained that reduced travel times are associated with increases in affiliates size and foster functional specialization, particularly in the service industries where information to be transmitted is arguably softer (Petersen and Rajan, 2002). Results are however also significant in the trade and manufacturing industries, but point estimates are lower. Our estimates allow to quantify the impact of the expansion of the HSR network on

Table 3.10: Spatial Dispersion and the balance of managerial jobs at HQ *vs.* Affiliates from 1993 to 2011

	Total (ln)	Share in total managerial jobs	High-skilled (ln)	Share of high-skilled
Travel time				
Manufacturing ind.	-0.045** (0.018)	-0.012*** (0.004)	-0.095*** (0.018)	-0.026*** (0.005)
Personnel services	-0.114*** (0.038)	-0.041*** (0.008)	-0.163*** (0.048)	-0.027*** (0.009)
Retail and trade	-0.085*** (0.016)	-0.035*** (0.004)	-0.167*** (0.017)	-0.022*** (0.005)
Business services	0.001 (0.016)	-0.040*** (0.003)	-0.057*** (0.018)	-0.025*** (0.005)
Transport	0.010 (0.029)	-0.008 (0.007)	-0.041 (0.033)	-0.019** (0.008)
Spatial dispersion				
Manufacturing ind.	0.004* (0.002)	0.004*** (0.000)	-0.009*** (0.002)	0.000 (0.001)
Personnel services	-0.034*** (0.001)	-0.011*** (0.005)	-0.046*** (0.001)	-0.008*** (0.004)
Retail and trade	-0.018*** (0.002)	-0.004*** (0.000)	-0.025*** (0.002)	-0.001*** (0.001)
Business services	-0.009*** (0.002)	-0.003*** (0.000)	-0.019*** (0.002)	0.001* (0.001)
Transport	0.008** (0.004)	0.008*** (0.001)	-0.009** (0.004)	0.000 (0.001)

Note: All regressions include group level fixed effects, as well as dummy variables describing in which region the group operates, interacted with years (to capture local market conditions). Regressions also include the logarithm of the total number of sites (HQ and affiliates) as well as group level exports in total sales to capture the business cycle on international markets. Number of observations: 225,942 for manufacturing industry, 131,829 for personnel services, 406,310 for retail and trade, 249,263 for business services and 52,936 of transport.

Sources: DADS and LIFI, groups owning affiliates in different locations (commuting zones) for at least one year between 1993 and 2011.

the management of remote affiliates. We obtain that for the average affiliate benefiting from the infrastructure, moving back to the 1980 rail network without HSR would induce a shift of roughly one job from administrative to operational activities in service industries (with the associated increase in the productive capacity of the considered affiliate), against 20% of a job in other industries (retail, trade or manufacturing). We also obtain that affiliates in the manufacturing and business services industries experienced decreases in production labor costs, of around half the cost of a production job for the average affiliate. At the group level, our regressions suggest that the impact on the operational profit margin ranges from 0.5 to 1.5 percentage points depending on the industry.

We think our analyzes provide precise evidence about the nature of the managerial costs implied by the geographic dispersion of corporate groups, but leave several questions open. First, our descriptive analyzes of affiliate openings and closures suggest that the question of the relationship between communication costs and

the decisions relating to corporate groups' overall geographic organization is most likely a fruitful domain of research. We leave the thorough rigorous economic and econometric treatments of these aspects for future research. Second, we focused in this paper on within group, HQ to affiliate communication. Natural extensions of our work would consider communication between affiliates, and more importantly, communication with external stakeholders, such as suppliers and customers as in Bernard et al. (2015) and investors. These aspects could drive large productivity and profitability impacts as well.

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3.A The business travels of managers: HSR or Air?

3.A.1 Managers represent a significant share of the customers HSR

Table 3.11 is constructed from the most recent survey data about transports and provides a breakdown of the clients of each mode of transportation in 2008⁴¹. It shows that most HSR travels have a private motivation, with only 35% of them having a business motivation - which is a lower share than in the case of air or standard rail travels. However and strikingly in the case of HSR, this contribution of business travels to total HSR traffic is largely driven by managers: they account for 20% of *total* HSR domestic travels, and to more than half of *business* HSR travels. Altogether, these figures suggest that managers, despite their low weight in the workforce (typically 9 to 10%) are a non-negligible segment of HSR customers, in particular among “business” customers.

Table 3.11: Long distance domestic travels in 2008 for each mode of transportation by type of user and main purpose (in %)

<i>Main purpose:</i> <i>Type of users:</i>	Private Trips			Business Trips	
	All	<i>High income</i>	<i>Managers</i>	All	<i>Managers</i>
Car	86	33	10	14	5
HSR	65	29	11	35	20
Other Rail	46	13	5	54	19
Bus	89	20	1	12	1
Air	54	28	14	45	32

Note: This survey was collected at the household level. Income is measured at the household (not respondent) level. “High income” corresponds to the top quartile of the income distribution (total household income, normalized by units of consumption). The category “Managers” describes the occupation of the respondent. Managers account for 20% of all HSR trips. This represents 20/35 = 57% of HSR business trips.

Sources: SOES, Transport and travel survey, 2008.

The disproportionate contribution of managers to HSR travels is first driven by the fact that they travel more in absolute terms: they contribute more to business travels, whatever the mean of transportation, than their relative weight in the workforce. Second, their contribution is even more disproportionate for means of transportation dedicated to long distance travels: HSR and planes.

⁴¹For homogeneity concerns, we restrict the analysis to domestic trips, since in this paper we focus on domestic HSR routes.

3.A.2 The market share of HSR for long distance business travels: 1994 vs. 2008

Table 3.12 investigates what is the precise “market” of HSR, and what are its main competitors⁴². First, the market share of HSR is highest, among domestic business travels, for the longest trips: it reaches 42% on the segment of very long distance travels (longer than 800km), on which airlines capture the second largest market share (33%). For travels of intermediate length, the market share of HSR is still of 24%, but the contribution of other rail is larger (31%), while it is negligible in the category of the longest trips. Symmetrically, air becomes a residual category in the range of travels of intermediate length. Altogether, these results suggest that over the recent period, HSR is one of the most popular mean of transportation for the longest domestic business travels.

Table 3.12: “Market share” of each mode of transportation by market segment - Domestic business travels only

<i>Unity:</i> <i>Distance:</i>	Number of trips				Distance
	<200km	200 to 800km	> 800km	All	
Market shares in 1994					
Car	77	62	17	68	56
HSR	0	19	12	8	14
Other rail	21	11	9	16	12
Bus	1	2	7	2	2
Air	0	6	43	5	13
Not answered	1	1	11	2	3
<i>Total</i>	<i>100</i>	<i>100</i>	<i>100</i>	<i>100</i>	<i>100</i>
Market shares in 2008					
Car	76	37	15	56	42
HSR	2	24	42	13	26
Other rail	21	31	9	25	20
Bus	1	2	0	1	2
Air	0	6	33	4	10
Not answered	0	2	0	1	1
<i>Total</i>	<i>100</i>	<i>100</i>	<i>100</i>	<i>100</i>	<i>100</i>

Note: Both surveys are collected at the household level. In 2008, HSR travels account for 13% of all trips (26% when weighted by distance), while air travels account for 4% of all trips (10% when weighted by distance).

Sources: Transport and communication survey, 1994, and SOES, Transport and travel survey, 2008.

⁴²Unfortunately, the sample size of the survey does not allow to analyze the break-down of managers’ travels by distance in a statistically meaningful way, especially for the shortest trips. Therefore, we only provide a description of the aggregate of all business travels and assume that the discrete choice patterns of managers does not deviate too much from them (which for the longest trips at least is a reasonable assumption).

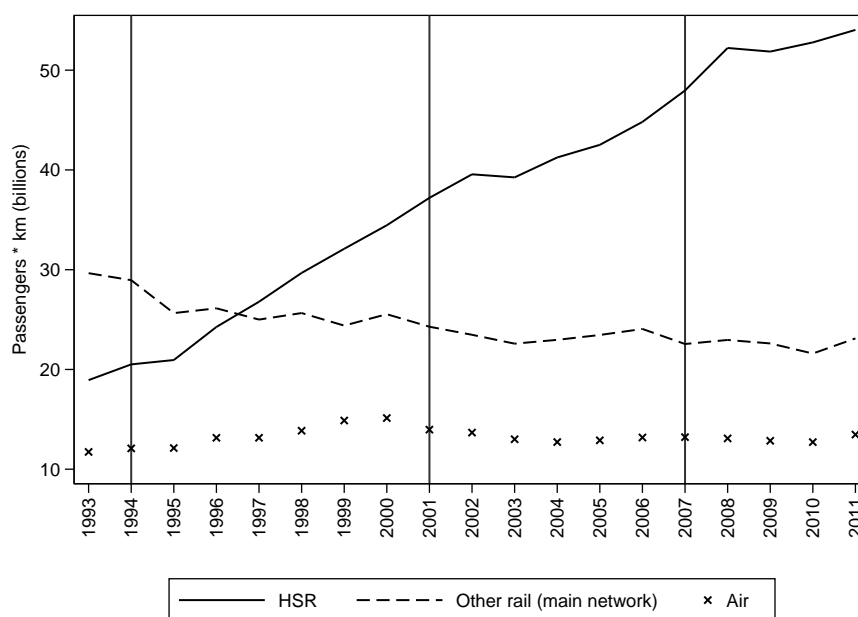


Figure 3.6: Market shares of rail and air for the passenger market (business and non-business), 1993 - 2011

Sources: compiled by SOeS - Ministry in charge of Transports from rail operators and DGAC.

Table 3.12 also provides some information about market shares at an earlier period, 1994, which corresponds to the beginning of our period of observation. It shows that the extension of the HSR network and its wider availability was accompanied by large gains in market shares. This fact is further confirmed by figure 3.6, which shows that HSR traffic experienced a steep growth while traffic by air and standard rail did not increase much.

This large market penetration by HSR is consistent with the fact that HSR was adopted quickly by a significant share of professional users when it was introduced as a new transportation device.

3.A.3 Further evidence from the evolution of the airline industry

This section documents the aggregate evolution of the airline industry over our period of study: in spite of a structural liberalization episode between 1994 and 2000, the evolution of the industry did not affect massively the options available to managers for their business travels during the period.

The airline industry was marked by an important liberalization episode between 1994 and 2001⁴³, which witnessed the entry of several airlines on the French market (including the domestic market), and the global increase of the supply of domestic

⁴³For a description, see for example the report to the French Senate Senate (2001).

flights. This pattern is documented on figure 3.7, which shows that while entry (and the supply of new flights) increased steeply between 1994 and 2000 (panel (A)), the number of air passengers however did not follow the same pattern and remained almost stable over the period (panel (B)). Due to a lack of demand, the number of domestic flights decreased quickly between 2000 and 2004, to go back to the supply level of 1993. Figure 3.6 shows that in contrast, transport by rail experienced a massive increase over the period, with implied volumes that are several orders of magnitude larger than total air traffic (as measured by total number of passengers, even when weighted by distance traveled).

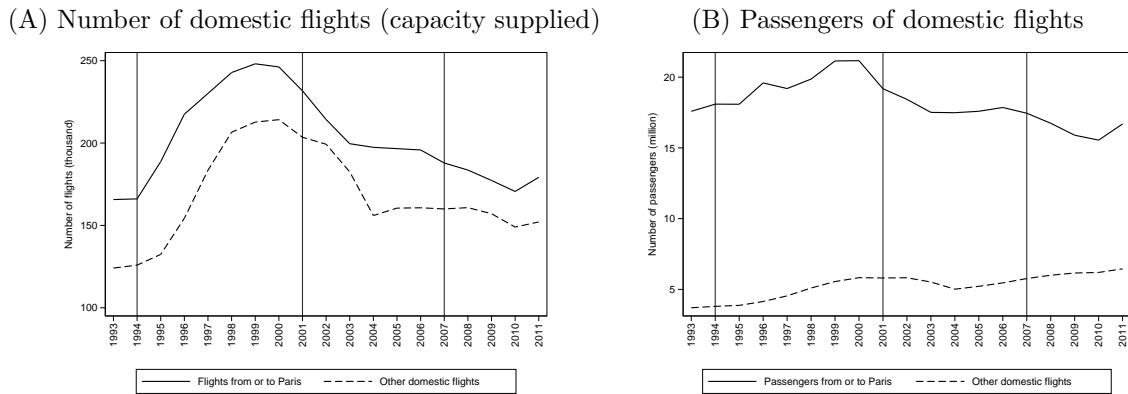


Figure 3.7: Supply and demand in the airline industry, 1993 to 2011

Sources: *Flux de Trafic Commercial (Commercial Traffic Flows), 1986 - 2013*, DGAC (published in June 2014).

These patterns are suggestive of the fact that in spite of the liberalization episode, airlines did not actually gain large market shares over other means of transportation, in particular rail. Figure 3.8 shows that there was no huge change in the relative price advantage of airlines over rail, neither over our period of analysis (given the parallel evolution of the two respective price indices), nor today (when comparing fares for selected destinations). Over the recent period, the number and frequency of connections by air is lower than the number and frequency of connections by rail, and many destinations served by HSR are simply not served by airlines.

3.A.4 Discussion of potential biases

What is the likely impact of overlooking air connections in our computations of travel times? If anything, this should produce an attenuation bias, which we expect to be small given the previous developments. More precisely:

- Whenever managers use air connections (be they new or not) rather than rail, then HSR travel time reductions are irrelevant, which generates attenuation bias in our setting.
- When airline connections appear (resp. disappear), then managers' demand might reports to air (resp. rail) in absence of rail travel time reduction. Travel

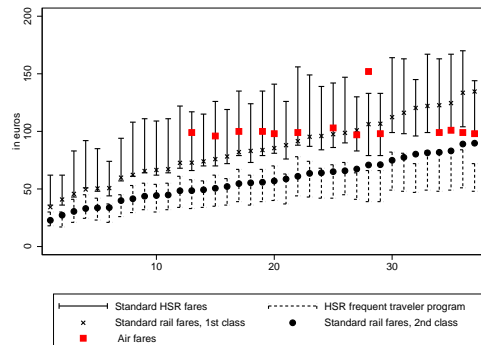
Figure 3.8: Prices of Transport by Rail and Air, between 1993 and 2011

(A) Aggregate Consumption Price Indices
Computed by the Statistical Institute
(base year 1998)



(B) Selected Prices Posted Online
as of March 2015

for 38 Destinations with Departure from Paris



Sources: Panel (A): INSEE.

Panel (B): Online available prices as of March 2015, for 38 selected destinations (with departure from Paris) sorted by distance: Reims, Arras, Le Mans, Tours (St-Pierre-Des-Corps), Lille, Valenciennes, Dijon, Angers St-Laud, Metz, Poitiers, Nancy, Mâcon-Loché TGV, Rennes, Besançon-Viotte, Nantes, Niort, Lyon Part-Dieu, Angoulême, Lyon St-Exupéry TGV, Strasbourg, La Rochelle, Mulhouse, Valence TGV, Chambéry Challes-les-Eaux, Annecy, Grenoble, Bordeaux St-Jean, Lorient, Brest, Avignon TGV, Nîmes, Dax, Aix-en-Provence TGV, Montpellier, Marseille St-Charles, Toulon, Toulouse-Matabiau, Nice. Air fares are retrieved from the website of Air France (Hop!), the leading airline company.

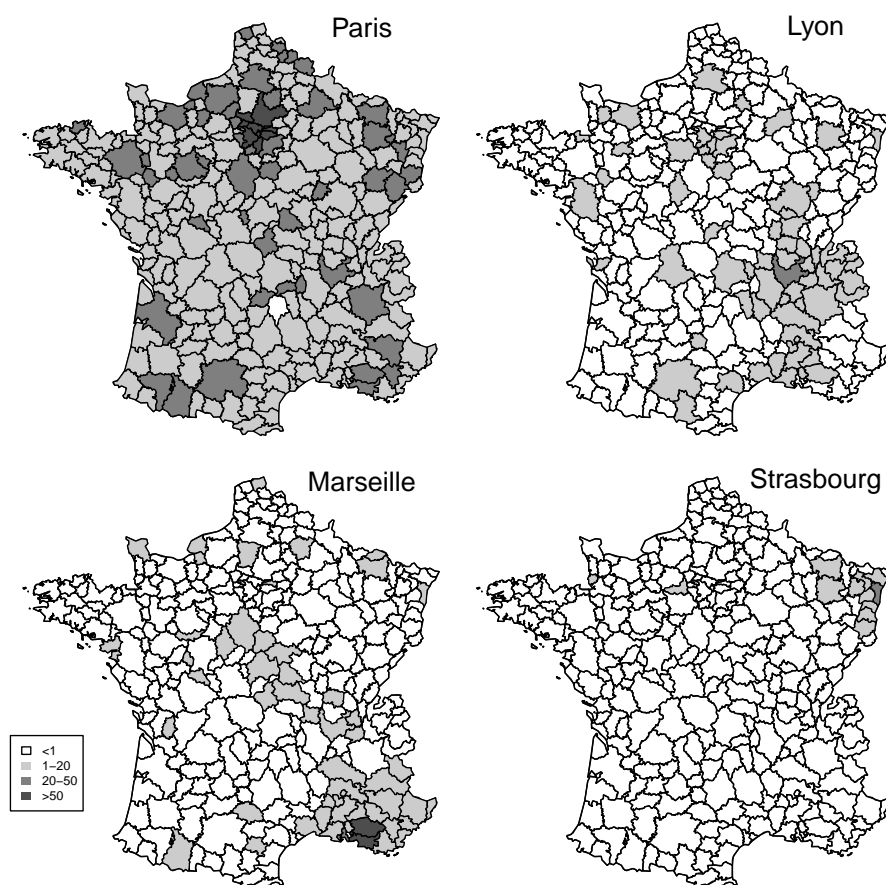
time might change (increase or decrease) in absence of HSR travel time variation, which would also generate attenuation bias in our setting.

- When airline closures are related to HSR line openings, then actual travel time might decrease by less than what we compute, or even increase. This would also generate attenuation bias in our setting.
- Amplification biases could be generated by airline openings as *simultaneous* responses to the opening of new HSR lines, in association with shorter travel times by air than by rail. We however think that these events were relatively rare. Furthermore, these upward biases are most likely low if travel time achieved by HSR is close to travel time by air (including access to airport or train station, see the discussion in section 3.3).

3.B Remote corporate control in France: the disproportionate weight of Paris

Figure 3.9 provides a break-down of the indicator of figure 3.2 (section 3.3.1) and describes in each commuting zone, the share of employment under control of HQs located respectively in Paris, Lyon, Marseille or Strasbourg. The main take-away of this comparison is the disproportionate weight of Paris in terms of corporate control. In terms of geographical range, the map shows that its sphere of “corporate influence” is particularly wide ranging, since most of the territory is reached by Parisian headquarters. Lyon and Marseille also reach very distant areas, but only occasionally and with a lower weight in terms of local employment.

Figure 3.9: Share of total private employment under control of HQs located in four French cities, in 2011 (



Notes: the four maps describe the share of private employment in each “employment zone” that is under control of HQs located, respectively, in Paris, Lyon, Marseille or Strasbourg (i.e. the sphere of “corporate influence” of these four large French cities).

Sources: DADS and LIFI, covering the private sector (except agricultural activities and workers of the personnel service industries directly employed by households).

3.C Computation of rail travel times

This section describes the construction of rail travel times between headquarters and affiliates. We relied on a two-step procedure:

- First, we simplify the network of trains stations and select only one “main station” by commuting zone.
- Second, we collected and constructed time tables for the resulting list of 316 stations.

Rail travel time between an affiliate and its headquarters is then measured by rail travel time between the respective “main stations” of the commuting zones where they are located. Travel time between headquarter or affiliate and “main station” is neglected for two reasons:

- It is typically short, and it does not vary over time (while our identification strategy typically relies on *variations* in travel times - see section 3.4).
- Furthermore, managers who are asked to travel for professional reasons might depart from home rather than from work: in such cases, travel time between headquarter or affiliate and “main station” is not the relevant quantity. In absence of precise information about the location where managers live, and from which station they might depart, the main station in each commuting zones is by construction⁴⁴ the best statistical guess we can get about it. This fact therefore also motivates our choice to allocate each headquarter and affiliate to the main station in their commuting zone, although the alternative of choosing the closest station is discussed in detail in section 3.C.3 below (it does not make much difference since the two candidate stations under each alternative are typically very close).

3.C.1 Selection of the “main station(s)” in each commuting zone

We select a set of 316 “main stations”, among the set of stations which existed in 1993, using the following criteria:

- In cases where only one station in the considered commuting zone is served by HSR, we select it as its “main station”.
- In cases where several stations in the considered commuting zone are served by HSR, we select the station having the highest long distance traffic using an adequate score based on the number of long distance services that are available in each station.⁴⁵

⁴⁴Commuting zones are defined “as the geographical area within which most of the labor force lives and works, and in which establishments hire most of their workforce”.

⁴⁵To be more precise, we computed traffic scores as of 2013 (for data availability reasons), based on the total number of services available in each station, but giving less weight to services with many local stops. The score is computed as the sum of the squared average distance between any two consecutive stops for each service available in the considered station.

- In cases where none of the stations in the considered commuting zone is directly served by HSR, we select the station having the highest long distance traffic score.
 - In the rare cases of ties, we select the station located in the most populated municipality.
 - Stations that were specifically built to accommodate HSR services during our period of estimation replace the previous “main station” in their employment zone from the date they enter into service.
- Overall, these “new HSR stations” have a large impact on rail travel times.⁴⁶

3.C.2 Time tables between “main stations”

We then collected past and current timetables in order to recover the fastest train service between any two directly connected “main stations”:

- Travel times for train services which remained “local” over the entire period (i.e. those implying no HSR service) were simply approximated by the 2013 timetables, which are available on the open-data platform of the national rail company.
- For long distance services, we relied on rail fan web sites and the archives of the national rail company, as well as on the evaluation reports of the French Ministry for Transportation (LOTI reports). These sources enable us to assess train travel times both *before* and *after* HSR line openings.

The obtained database contains travel times between any two directly connected stations, at any date between 1980 and 2013⁴⁷. We then complement it with travel times between any two *indirectly* connected stations, assuming that each train change takes 15 minutes (a rather lower bound).

3.C.3 Discussion

In table 3.13, we compare travel times obtained with our baseline procedure, where we allocate each affiliate or headquarter to the main station in their respective commuting zone, with an alternative procedure where we allocate each affiliate or headquarter to the closest main station (not necessarily located in the same commuting zone). The main take-away of this table is that *absolute* travel times are somewhat altered by this alternative choice of measurement, since the average discrepancy for treated affiliates is 12 minutes (while the median is only 2 minutes). However, *changes (reductions)* in travel times are far less affected, since they only occur via new HSR line openings and therefore fundamentally rest on stations served by HSR, which are the same in the two procedures. The precise distribution of these changes in our estimation sample is plotted on figure 3.10.

⁴⁶Which legitimates the investment required by their construction...

⁴⁷This time span encompasses our period of analysis and enables us to run the “placebo” robustness checks presented in section 3.5.3.

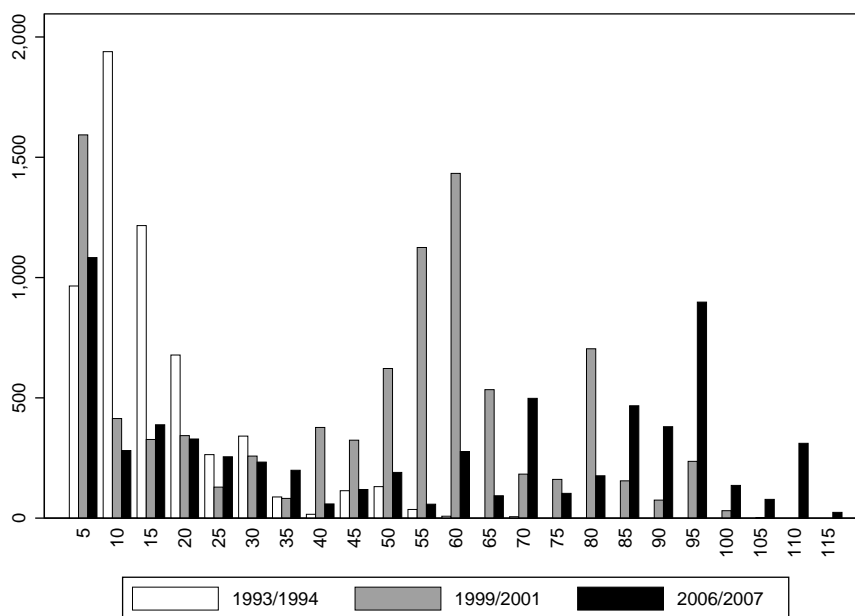
Table 3.13: Travel Times with Alternative Computing Procedures

Affiliates \times years	No travel time change in our sample			Reduction in travel time in our sample		
	Com. Zone	Clos- est	Diffe- rence	Com. Zone	Clos- est	Diffe- rence
Computation of travel time from/to:						
Distance to HQ (km)	160		-	450		-
HQ in Paris	29%		-	36%		-
<i>Comparison of computing procedures:</i>						
Same zone (station) as HQ	42%	40%	(4%)	0%	0%	(0%)
Travel time after change (minutes)	95	98	7	232	240	12
Variation in travel time (minutes)	0	0	0	35	32	6
Share of firms with travel time change	0%	0%	(0%)	100%	81%	(19%)
Share of aff. benefiting from HSR service	33%	32%	(2%)	100%	96%	(4%)
Share of new HSR users	0%	0%	(0%)	19%	16%	(5%)
Direct track	72%	68%	(7%)	47%	37%	(13%)
1 change	14%	16%	(8%)	25%	26%	(16%)
2 changes or more	12%	15%	(4%)	28%	37%	(11%)

Notes: The classification in columns between affiliates \times years experiencing (or not) changes in travel times is based on the computation of travel time between commuting zone which is actually used in our regressions. Notice that around 40% of the affiliates \times years experiencing no change in travel time are located in the same commuting zone as their headquarters. For the 60% which are not located in the same commuting zone as their headquarters: average distance to HQ is 273km, average travel time (between employment zones) is 162.3 minutes, and the share of direct tracks (between employment zones) is 52.5%.

Sources: DADS and LIFI, affiliates (see section 3.3.1 for definition) which are part of multi-location groups between 1993 and 2011.

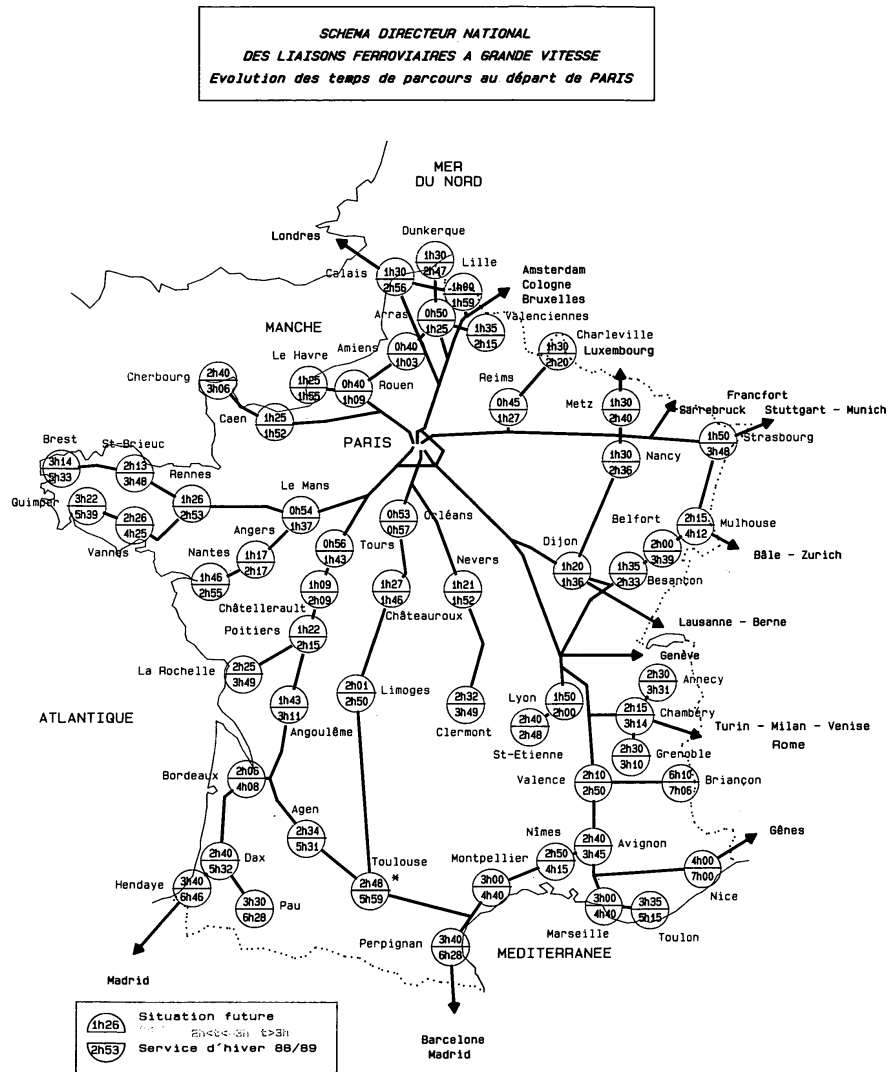
Figure 3.10: Distribution of travel time reductions at main dates of HSR line openings – Northern line (1993/94), connection to Marseille (2000/01) and Eastern line (2006/07)



Note: Values are expressed in minutes. Firms which did not experience any change in rail travel time at the respective dates were excluded: observations in the bin labeled by “0” correspond to strictly positive travel time reductions, but that are smaller than 5 minutes.
 Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

3.D Map from the 1991 governmental blueprint document

Figure 3.11: Map of the HSR lines proposed in the 1991 governmental blueprint document



Sources: Ministère de l'Équipement (1991), French Ministry for Equipment.

3.E Details of the estimation method

The principle of the estimation method is to iterate on three sets of normal equations that are conveniently defined. Let us first rewrite equation 3.2 in matrix format as:

$$\underbrace{Y}_{y_{ijlt}} = \underbrace{X B}_{\beta \cdot T_{ijl} + \gamma \cdot X_{(ij)lt}} + \underbrace{D_I A^I}_{\alpha_{ij}} + \underbrace{D_{LT} A^{LT}}_{\alpha_{lt}} + \underbrace{\epsilon}_{\epsilon_{ijlt}} \quad (3.5)$$

where X is a vector encompassing our *continuous* treatment variable and additional *continuous* explanatory variables (typically group exposure to international demand conditions, i.e. export intensity), D_I is the vector of the affiliate level dummies (α_i), D_{LT} the vector of the commuting zone \times year dummies (α_{lt}), B , A^I and A^{LT} are the corresponding parameters to be estimated. The three sets of normal equations are defined as follows:

$$\begin{pmatrix} B = (X'X)^{-1}X'(Y - D_I A^I - D_{LT} A^{LT}) \\ A^I = (D_I'D_I)^{-1}D_I'(Y - X B - D_{LT} A^{LT}) \\ A^{LT} = (D_{LT}'D_{LT})^{-1}D_{LT}'(Y - X B - D_I A^I) \end{pmatrix}$$

The algorithm is initiated at $A_{(0)}^I = 0$ and $A_{(0)}^{LT} = 0$. The first of equation provides the first estimated value for $B_{(1)}$, which is plugged into the second set of equations to get $A_{(1)}^I = A_{(1)}^I(B_{(1)}, A_{(0)}^{LT})$. Then $B_{(1)}$ and $A_{(1)}^I$ are plugged into the third set of equations to get $A_{(1)}^{LT}$. This procedure is iterated until the sum of squared residuals no longer decreases.

The benefit of the decomposition of normal equations into the three sets above is that it renders the estimation computationally tractable. Only the first set of equations requires an actual matrix inversion, but it is of relatively low dimension. It can be performed by simple OLS on the modified dependent variable $Y - D_I A_{(n-1)}^I - D_{LT} A_{(n-1)}^{LT}$. The two last sets of equations simply correspond to the computations of means: of the variable $(Y - X B_{(n)} - D_{LT} A_{(n-1)}^{LT})$ by affiliate across years (classes generated by D_I), and of the variable $(Y - X B_{(n)} - D_I A_{(n)}^I)$ by commuting zone \times time across affiliates (classes generated by D_{LT}). This algorithm, which consists in iterating sequentially across each set of equations, falls into the class of so-called “partitioned” algorithms (“zigzag” iterations) which has been analyzed in full length by Smyth (1996): while the iteration process is slow in general (unless covariates are orthogonal, but this is not the case in our setting), the zigzag iteration is found to admit a global convergence result.

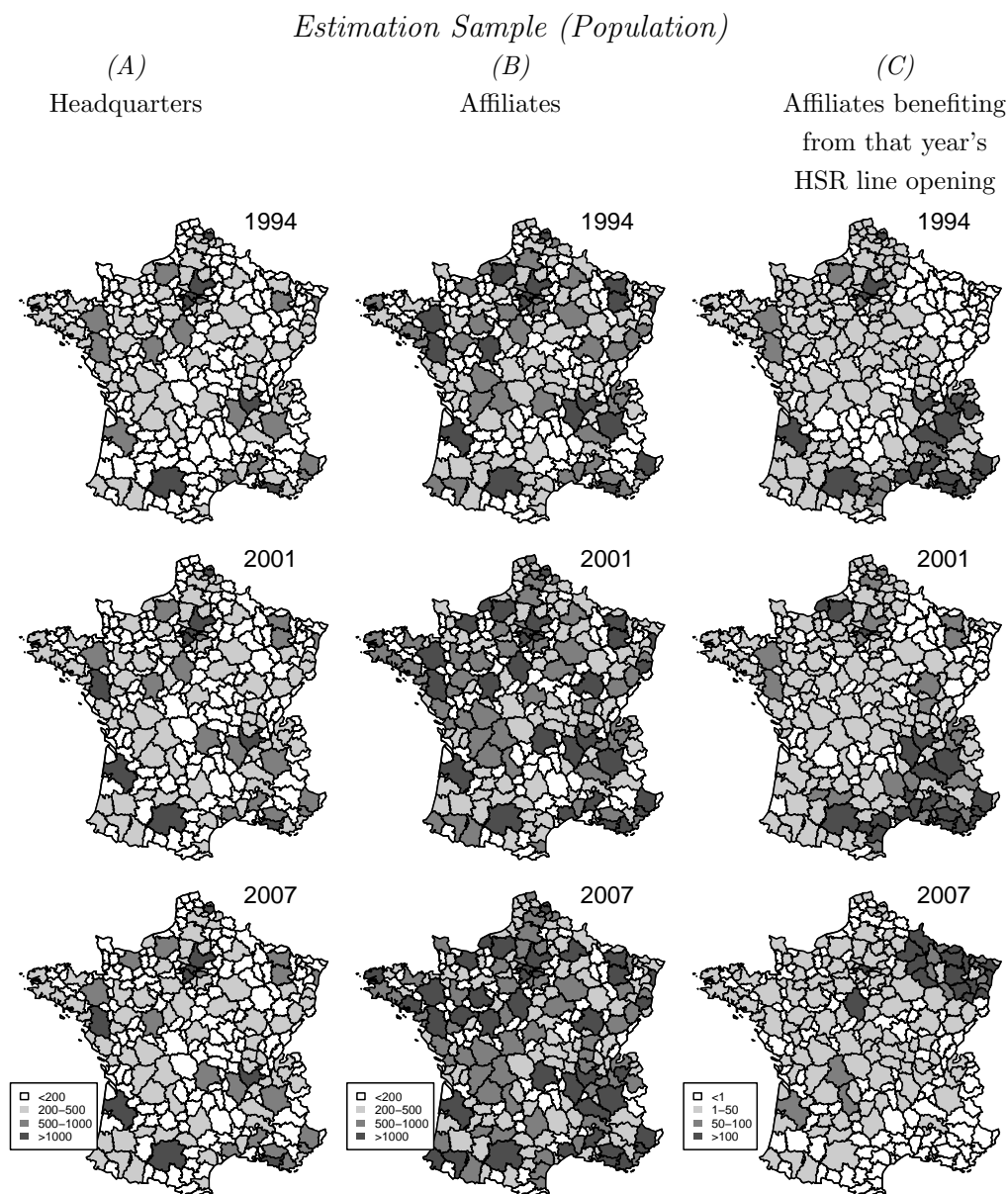
To compute the correct standard errors associated with the estimate of B , Guimaraes and Portugal (2010) apply a result derived by Abowd et al. (2002), who show that the total number of identified “fixed” effects is given by $N^I + N^{LT} - G$, where G is what they call (in their application) the number of “mobility groups” (classes) generated by the two sets of fixed effects, D_I and D_{LT} . In our case, this simply corresponds to the partition by geographical zones, i.e. $G = L$. The formula for the computation of standard errors is then given by:

$$V(\hat{\beta}) = \frac{SSR}{(N - N^X - N^I - N^{LT} + L) \cdot N \cdot s_{\text{time}}^2 \cdot (1 - R_{\text{time}}^2)}$$

where N is the total number of observations, N^X is the number of variables in X , N^I is the number of affiliates and N^{LT} is the number of commuting zones \times time. Last, s_{time}^2 is the sample variance associated with the travel time variable and R_{time}^2 is the coefficient of determination obtained from a regression of travel time on all other remaining explanatory variables.

3.F Geographical repartition of the estimation sample

Figure 3.12: Localization of business units benefiting from rail travel reductions



Sources: Matched DADS files and LIFI survey; business units (HQs or affiliates - see section 3.3.1 for definition) which are part of multi-location firms between 1993 and 2011.

Note: The different dates correspond to the opening date of the Northern line (1993/1994), of the connection to Marseille (2000/2001) and to the opening date of the Eastern line (2006/2007).

3.G Descriptive analysis of the extensive margins

If proximity between affiliates and headquarters facilitates monitoring and information transmission, one might expect that it also matters on the “extensive margin”, for affiliate creations and destruction. These events are not explicitly taken into account in our main regression framework of section 3.4, although in practice they are well captured by the affiliate level fixed effects (which “purge” regressions from most of the selection bias which could arise). A thorough econometric treatment of these events would require the specification of a discrete choice model of implantation across commuting zones, which would not fit our identification strategy. We therefore leave this aspect for future research but propose as in Giroud (2013) a series of regressions describing the relation between affiliate creation or destruction and travel time.

3.G.1 Affiliate creations

Table 3.14: Travel time and the reshuffling of affiliates: characteristics of entries from 1993 to 2011

<i>Dependent variable:</i>	Distance (ln) (1)	Travel time (2)	HSR (to HQ) (3)
Distance (ln)			0.155*** (0.000)
Employment of affiliate (ln)	-0.058*** (0.002)	-0.033*** (0.002)	0.006*** (0.000)
Gains in travel times at other affiliates	0.084*** (0.003)	-0.002 (0.004)	-0.002*** (0.001)
Affiliate entry(ies) in same group(dummy)	-0.124*** (0.009)	-0.153*** (0.012)	-0.113*** (0.002)
Affiliate exit(s) (dummy)	-0.127*** (0.009)	-0.081*** (0.012)	-0.000 (0.002)
Other affiliates are HSR users	1.960*** (0.008)	1.693*** (0.011)	0.117*** (0.002)
Total number of affiliates in group (ln)	0.046*** (0.002)	0.056*** (0.002)	0.011*** (0.000)
Mean (un-logged) dependent variable	272	2.872	0.504
Observations	323,689	323,689	323,689

Note: All regressions include industry (2 digits) \times local (commuting zone) \times time fixed effects to control for the local market conditions, as well as (4digit) industry dummies. Regressions also include the (ln) number of affiliates in the group as well as the group level exports in total sales to capture the cycle on international markets.

Sources: DADS and LIFI affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

Our treatment of affiliate creations follows Giroud (2013). We simply choose to describe the characteristics Z_{ijlt}^{CREA} of the new affiliates⁴⁸ and investigate whether

⁴⁸Affiliate “creation” correspond to actual creations or to acquisitions in our setting. We only

geographic distance, travel time or (conversely) the availability of HSR on the track to headquarters are related to different characteristics of the group which might also “tap” into the scarce HQ managerial resources that are to be shared across all affiliates: the size of the created affiliate, the total number of affiliates within group, *etc.* (X_{ijt}). The estimated equation writes:

$$Z_{ijlt}^{CREA} = \alpha_{lt} + \gamma \cdot X_{ijt} + \varepsilon_{ijlt} \quad (3.6)$$

In this equation, commuting zone \times time fixed effects (α_{lt}) are still identified and allow to control very precisely for local shocks.

Results are reported in table 3.14. We obtain that larger affiliates, which are likely to be more difficult to manage remotely (or for which the “size of stake” might be larger) tend to be created closer to their headquarters, whatever the indicator of distance. Similarly, when other affiliates experienced gains in travel time, thus freeing some HQ managerial resources, then affiliates tend to be created at a greater distance, and they are less likely to be created at proximity of the HSR network. This suggests that when more managerial resources become available, then the trade-off underlying the location choice of affiliates (between higher gains from local market proximity and higher managerial costs from geographical dispersions) shifts in favor of market proximity. Last, affiliate churning (creation but also destruction), which might be demanding in terms of headquarters’ managerial resources, are both negatively correlated with distant affiliate creations. Un-reported regressions show that all of these results also hold industry by industry.

3.G.2 Affiliate destruction

For affiliate destruction, we propose a more powerful treatment which is a direct extension of the regression framework in section 3.4:

$$EXIT_{ijlt} = \alpha_j + \alpha_{lt} + \beta \cdot T_{ijlt} + \gamma \cdot X_{jt} + \varepsilon_{ijlt} \quad (3.7)$$

where $EXIT_{ijlt}$ is a dummy variable indicating that the considered affiliate is exiting from the group⁴⁹ and notations are otherwise similar to those in equation 3.2. The main difference with equation 3.2 is however that the inclusion of affiliate \times group fixed effects would capture too much heterogeneity because affiliates are on average present for only 3.2 years in our panel. We therefore replace the affiliate \times group fixed effects in equation 3.2 with group level fixed effects, which preserve the identification of the β coefficient from variations in travel time. As previously, commuting zone \times time fixed effects (α_{lt}) are still identified and allow to control very precisely for local shocks. Equation 3.7 is then estimated as a linear probability model, using the same estimation procedure as in section 3.4.1, and obtained results are presented in table 3.15.

consider affiliate creation in pre-existing groups, which were furthermore already operating in the same industry (to abstract from global market entry decisions).

⁴⁹Affiliate “destruction” corresponds to actual closures or to resale of affiliates, while the group itself still operates in the same industry, with other affiliates (to abstract from more global market exit decisions).

We obtain that everything else equal, more distant affiliates (in terms of travel times) have a higher probability to be terminated, either because they are more difficult to manage and/or because they are less profitable - or simply because they are also “politically” distant (Bassanini et al., 2015). These correlations tend to be higher in business service industries, where they could explain around 5 percentage point for the average travel time (2 hours). This remains a small part of the entire share (47%) of exits to be explained in this industry. We also obtain that affiliates who benefited from gains in travel time thanks to the HSR network expansion were less likely to get terminated, while the probability of closure was also highly significantly reduced by gains at other affiliates, which tends to confirm the resource constraints story in section 3.5.5.

Table 3.15: Travel time and the reshuffling of affiliates – Probability of exit of affiliates of multi-location corporate groups only from 1993 to 2011

	Manufacturing Industries (1)	Personnel Services (2)	Retail and Trade (3)	Business Services (4)	Transport (5)
Travel time	0.017*** (0.000)	0.014*** (0.001)	0.014*** (0.000)	0.022*** (0.000)	0.017*** (0.001)
Gains since entry	-0.014*** (0.004)	0.012** (0.005)	-0.024*** (0.002)	-0.015*** (0.004)	-0.023*** (0.006)
Gains at other aff.	-0.011*** (0.001)	-0.018*** (0.001)	-0.010*** (0.001)	-0.011*** (0.001)	-0.016*** (0.001)
Share of exits	0.428	0.354	0.415	0.465	0.423
Observations	426,487	241,677	1,045,416	401,694	138,778

Note: All regressions include headquarter (i.e., group) level fixed effects, as well as local (commuting zone) \times time fixed effects to control for the local market conditions. Regressions also include group level exports in total sales to capture the cycle on international markets.

Sources: DADS and LIFI, affiliates (see definition in section 3.3.1) which are part of multi-location groups between 1993 and 2011.

Chapter 4

A Long-Term Evaluation of Enterprise Zones: The First Generation of the *“Zones Franches Urbaines”*

This chapter is cowritten with Pauline Givord (Insee and Crest) and Simon Quantin (Insee).

4.1 Introduction

The provision of locally-targeted tax credits and subsidies to kickstart sustainable economic development has become a widely used policy tool. Indeed, the first so-called “Enterprise Zone” programs were implemented in the UK in the 1980s, and others followed in several US states and elsewhere. In France, the French Enterprise Zone policy “*Zones Franches Urbaine*” (ZFU hereafter) first came into existence in 1997. This program grants temporary but remarkably generous tax incentives to small firms which choose to locate in economically distressed areas. The rationale guiding policy makers when opting for a ZFU program is quite simple: reductions in tax are meant to offset the numerous disadvantages associated with deprived areas, such as the shortage of a skilled labor force, the lack of public services, a dearth of inputs, and poor market potential. The ZFU initiative may stimulate local economic activity, by attracting firms that will employ the locally resident workforce, and may “revitalize” these neighborhoods by improving the local amenities (health services, convenience stores...). The spinoff effects ought to include increased local demand, and greater incentive for other new firms to choose the same location because of agglomeration economies. Once this initial boost had been delivered, the ZFU initiative was expected to terminate.

However, as stressed by Neumark and Simpson (2015) in a critical review of the already large economic literature on place-based policies, the theoretical foundations of these policies have not been well established (Kline and Moretti, 2014).

As well, these programs may have potential adverse effects such as inducing firms to hire workers who are already involved in work-based networks, instead of targeting local unemployed people for their hires; and they may have negative externalities on neighboring localities which are often not much better off than the ZFU itself. The variety of empirical evaluations of these policies, which mostly focus on their impact on employment, reflects the ambiguity of the theoretical mechanisms. Although most evaluations find no significant increase in employment (see for instance Bondonio and Greenbaum, 2007 or Neumark and Kolko, 2010, for the US enterprise Zones, or Accetturo and de Blasio, 2012 for the *Italian “Patti Territoriali”*), a significant minority do (Ham et al., 2011, Busso and Kline, 2008). For France, several papers focus on the second wave of the ZFU program, implemented in 2002. They obtain a significant but small impact on firm locations and related employment (see for instance Givord et al., 2013 or Mayer et al., 2012). Still, the breadth of the empirical literature on place-based programs notwithstanding, many questions remain unanswered. Neumark and Simpson (2015) identify a research agenda, suggesting several fields where evidence capable of guiding policy is still lacking. Investigating the long-term effect of these programs is the first of them, as one of the main challenges of place-based policy is to generate self-sustaining economic gains. The other open questions include: a more precise identification of “what the effects are” and who gains and who loses from the policy-based question; and “isolating features of policies that make them effective”.

This paper derives from this research agenda. More specifically, we focus on the first wave of French ZFU created in 1997, to evaluate whether this initiative was able to yield long-term economic activity. We try to assess whether the program has had a positive impact on the living conditions of the inhabitants of these disadvantaged neighborhoods. More specifically, while most previous related studies focus on overall firm employment, we analyze resident employment separately from non-resident employment. We also focus on firms providing local services, as one stated objective of the ZFU initiative was to give the local population better access to the sort of “basic” services (physicians, convenience stores, and tradesmen like plumbers...) that are more likely to suffer hardship from being located in distressed urban areas (small market potential in low-income neighborhoods, low accessibility for non-local employees, high rates of criminality).

Interestingly, while we use similar geolocated data and the same propensity score method as previous empirical evaluations, which focused on the second wave of this initiative, the results we obtain for the first wave of the French ZFU initiative are very different from their findings. While Givord et al. (2013) observe very little impact on the number of plants and employment, we observe on the contrary that the first ZFU initiative caused these outcomes to respectively double and triple over a five-year period, compared to the baseline level that would have been achieved without tax rebates. These surprising results are robust to an alternative identification strategy, relying on a discontinuity rule in the selection process (only the most populated areas were selected).

Apart from this short-term comparison, our results cover a much longer period (almost twenty years) than previous studies. We highlight that the short-term assessment of the French ZFU initiative may differ sharply from the medium and

long-term ones. We observe that the number of firms newly located in ZFU increases the first year, and stays at a high level for the next four years. But during this period, the pace of firm closures progressively grows and finally overtakes the pace of new firm locations. This suggests that firms that do choose to locate in a ZFU may be non-economically viable ones, likely to fail when they are not subsidized anymore. Besides, firms are free to relocate outside the ZFU after the end of the program: the full tax cuts are granted to a given firm for only five years. Indeed, a significant part of the ZFU effect flows from firm relocations, suggesting the presence of a windfall effect. This result challenges the intuition that a ZFU can induce a change in the economic spatial equilibrium, by creating a “virtuous circle”. And the fact of the matter is that, while the ZFU initiative was originally planned to be temporary, its lifespan has been prolonged repeatedly.

Concerning the situation of the inhabitants of these disadvantaged areas, our results suggest that the ZFU initiative achieves its objectives, but only on a limited scale. This in turn suggests a lack of clear targeting of the ZFU initiative. Overall, the employment level does increase in the ZFU thanks to tax cuts. It is 3 times higher than its counterfactual level after five years, and a proportion of this employees actually live in the municipalities in which the disadvantaged areas are located. However, while the ZFU initiative explicitly includes a clause favoring local hiring, in practice the share of local residents employed in firms located in ZFU tends to decrease over the period. As regards local amenities, we do observe a positive effect on location decisions by firms in the corresponding sector (trade, health or community services), as the number of these firms increases by 50% after ten years. But this figure is much smaller than the corresponding one for the “footloose” firms of the business services sector (office cleaning, security, IT services...). The number of these firms who do not operate locally and may easily leave the areas when tax breaks end, increases by 300% after ten years thanks to the ZFU initiative.

The paper is organized as follows. Section 4.2 presents the French ZFU program, the ZFU areas, and the quantification of the magnitude of the financial incentives provided by the program. The data are briefly presented in the following section. Identification issues are discussed in section 4.4. Section 4.5 displays the results and section 4.6 concludes and discusses our findings.

4.2 The French Enterprise Zones

4.2.1 Selection of the Enterprise Zones

Urban decay has become a main topic of French public debate since the 1980s. A range of policies have been implemented in response to social and economic problems experienced in the deprived outskirts of France’s cities. Indeed, the so-called “social fracture” (*fracture sociale*) was an important theme of the 1995 presidential campaign, with the social and economic circumstances in deprived urban areas being identified as the main causes. The “stimulus for cities” law (*Pacte de relance de la ville*), passed in 1996 by the newly elected Government, aimed at addressing the issue of urban decay and reducing inequalities between urban neighborhoods.

This law resulted in the implementation of tax cuts for businesses located in

those deprived areas. More precisely, this policy instituted a three-tier classification scheme for disadvantaged urban areas. The first tier is known as ZUS (“Deprived Urban Areas” or “*Zone Urbaine Sensible*” in French). They correspond to the 757 most deprived areas in France,¹ according to various indicators of socio-economic development (in particular, high concentrations of social housing and high unemployment rates). The second tier, the ZRU program (“Urban Renewal Areas” or “*Zone de Revitalisation Urbaine*” in French) includes the most disadvantaged ZUS ranked by a global index of their social and economic position. This index takes into account the unemployment rate, the population size, the proportion of unskilled people, the proportion of young people and the potential tax revenue (product of the tax base by the medium tax rate) of the city. It corresponds to the product of the four first indicators divided by the fifth one. 436 ZRU were designated in 1996.² Finally, the third tier is constituted by ZFU (“Urban Free Zones”). These zones are chosen in a two-stage process: only the most populous ZRU are eligible, the official threshold being 10,000 inhabitants, and out of that set the most deprived ZRU, as defined by the same global rating, are designated as Enterprise Zones. In 1997, during the first phase of this initiative, 44 areas received the ZFU designation, followed by an additional 41 in 2004 and 15 more in 2007.

Figure 4.1 illustrates, in the case of the Paris metropolitan region, the uneven local distribution of the unemployment rate, as well as the location of some ZFU. This region is the wealthiest in France, but the unemployment rate varies markedly amongst municipalities. The inner northeast suburbs of Paris are a site of concentrated economic difficulty. This large sector apart, municipalities characterized by high rates of unemployment are spread throughout the region. The ZFU are located in such economically distressed municipalities, but not always. This is explained by the fact that the unemployment rate in some neighborhoods (the relevant geographical level for ZFU) may largely exceed the one estimated at the municipality level. Besides, due to the political bargaining involved, hence the need to disperse targeted areas across France, the designation of ZFU does not rely on a deterministic way on the index indicating the social and economic position. The upshot is that the ZFU are uniformly spatially distributed across France, while urban deprived areas are mostly concentrated in a limited number of municipalities.

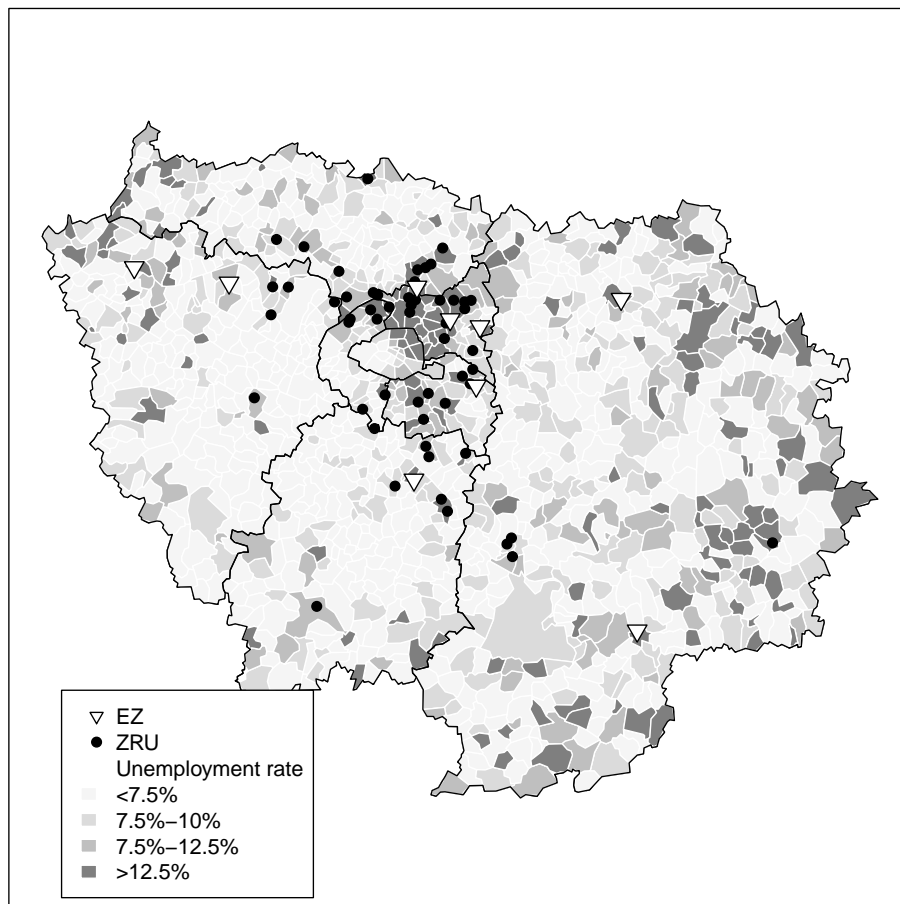
4.2.2 Advantages granted by the ZFU policy

Enterprise Zones offer remarkably generous incentives (deep tax cuts on property, labor and business taxes). They target only small firms (with less than 50 employees, with an additional condition on the volume of sales), whether located in the area prior the introduction of ZFU policy or not (see Table 4.1 and Appendix A for details). Full exemption is granted for a minimum of five years. In comparison to the tax relief available in ZFU, the ZRU and ZUS designations provide much shallower tax credits. The ZRU program provides limited tax cuts, for newly created firms only and over a shorter period (one or two years after startup, depending on the tax).

¹717 in continental France and 40 ZUS in French overseas departments. 4.73 millions of people lived in ZUS according to 1990 census data.

²416 in continental France and 20 in French overseas departments in 1996.

Figure 4.1: ZFU location and unemployment rate in 1990 in the Paris region



Source: 1990 Census.

Table 4.1: Tax cuts granted by the French enterprise zone program

Conditions	ZRU	ZFU
Payroll tax exemptions		
Plant eligibility	With up to 50 employees	
Employee eligibility	New hires	All employees
Exemption	Permanent and fixed-term contracts longer than 12 months	
Duration	Fraction of salary ≤ 1.5 times the minimum wage	
	1 year	5 years
Corporate income tax exemptions		
Eligibility	New firms	All
Exemption	100 % the first 2 years, decreasing the next 3 years	100 % during 5 years
Local business tax exemptions		
Eligibility	New firms (< 150 employees)	All (< 50 employees)
Exemption	100 % during 2 years	100 % during 5 years
Local property tax exemption		
Eligibility	None	All
Exemption	100 % during 5 years	

Source: Legislative texts (*Journal officiel*, 1995).

Payroll tax exemption applies to all employees in ZFU, while it is limited to newly hired employees in ZRU. Finally, the ZUS program merely allows local authorities to exempt firms from local business taxes, without making it mandatory.

The first generation ZFU were implemented in 1997 and scheduled for five years. As initially planned, the policy ended in 2001, businesses had to locate in an Enterprise Zone before December 31, to benefit from the tax exemptions. However, the ZFU policy was reactivated in 2003 and has been kept alive continuously since then. New areas were designated successively in 2004 and 2013, for a total number of 100 ZFU today.

The financial incentives depend on the actual financial burden for small firms, and on the structure of their revenues and costs. To assess the actual generosity of this program, we simulate the benefit using individual databases that provide accurate information (see Appendix B). According to these simulations, payroll tax exemptions account for the largest share of tax reductions. In 1997, the median cut in payroll taxes associated with ZFU was 6,000 euros and this cut accounted for about 15% of the median wage bill (see Table 4.7 in the appendix). This relative advantage was slightly reduced after the introduction of national payroll scheme changes in 2003 but ZFU remain attractive. Under the scheme of payroll taxes in use since this date, the median gain for firms from being located in a ZFU still accounts for about 12% of the median labor cost (with an amount of 4,500 euros).

Eligible firms also benefit from a full exemption from corporate income tax, up to a limit that cannot exceed 20,000 euros per year. In practice, a closer look at real data suggests that this exemption is not as appealing as it may seem. Before the implementation of ZFU, more than three quarters of small firms did not pay any

corporate income tax. For those which did pay a strictly positive corporate income tax, the median amount paid was 3,700 euros (see Table 4.8 in the appendix).

4.3 Data

We exploit two exhaustive administrative datasets to gather rich information on firm demography (number of plants) as well as employment (see details in the appendix C).

The French business register (SIRENE) follows all French firms and plants. It displays the location of each plant, its firm's legal status, its industry and year of creation, at January 1. This register also tracks plant creations and relocations during a whole year. It thus enables us to specify whether a new plant location is an actual creation or a relocation of an existing plant. It also allows us to identify when businesses cease activity. Above all, SIRENE locates precisely all plants in continental France. Thus, we can accurately identify which plants have settled in a ZFU and those which have not, which is crucial as ZFU do not correspond to administrative boundaries (see also Givord et al., 2013). Indeed, using data even at the level of the smallest French administrative subdivision (the municipality, or *commune*) would have yielded an underestimation of the impact of tax exemptions, because plants which benefit from ZFU tax breaks would have been grouped with plants which do not (see Table 4.13 in the appendix).

The second dataset (DADS) is an exhaustive administrative employer-employee database with information on the workforce of plants. Employment can be measured in various ways at plant level: full-time equivalents over a year, number of employees at any point of time or as of 1 January. We use this latter measure, which is consistent across years and consistent with the French business register. The DADS thus provides a measure of local employment, meaning employment in plants located in the area. Moreover, DADS allows us to split our employment measure into low-skilled, skilled and high-skilled employment, and also into resident and non-resident employment.

These data allow us to probe the long-run effects of Enterprise Zones, as well as temporal delays or extenuations. SIRENE and DADS are available from 1995 to 2013. This means that we observe data at least 2 years before the introduction of the ZFU tax exemptions and up to 16 years after. Finally, the 1990 Population Census allows us to measure socio-demographic variables used for the designation of an area as an Enterprise Zone. For this evaluation, the data have been aggregated at the three-tier classification levels presented in section 4.2: ZFU, ZRU and ZUS.

4.4 Identification issue and empirical strategy

4.4.1 Identification issue and process of selection to ZFU system

We restrict our estimation sample to the comparison set of non-beneficiary areas (i.e. the control group) to ZRU, which are areas most similar to ZFU. Panel data

Table 4.2: Average socio-economic indicators of the urban areas

<i>Zone type:</i>	ZFU	ZRU		ZUS
<i>Population:</i>		> 10,000	< 10,000	
Number of zones	45	69	282	321
Unemployment rate	21.9	18.2	24.3	17.9
Percentage of social housing	64.4	63.7	66.0	61.1
Percentage of foreign people	21.8	16.3	20.0	17.2
Percentage of unskilled people	43.1	36.4	45.8	37.9
Percent. of young people (< 25)	46.7	43.2	45.5	41.2
Average potential tax revenue (in €)	2,707	3,212	2,609	3,438

Source: 1990 population Census.

allow us to eliminate the potential fixed effect specific to each area. More precisely, our main variables of interest are (log) outcome-level differentiated based on data from 1995, meaning two years prior to the introduction of the tax exemptions (the aim of using such a lag is to avoid capturing potential anticipation effects of the measure, for instance).

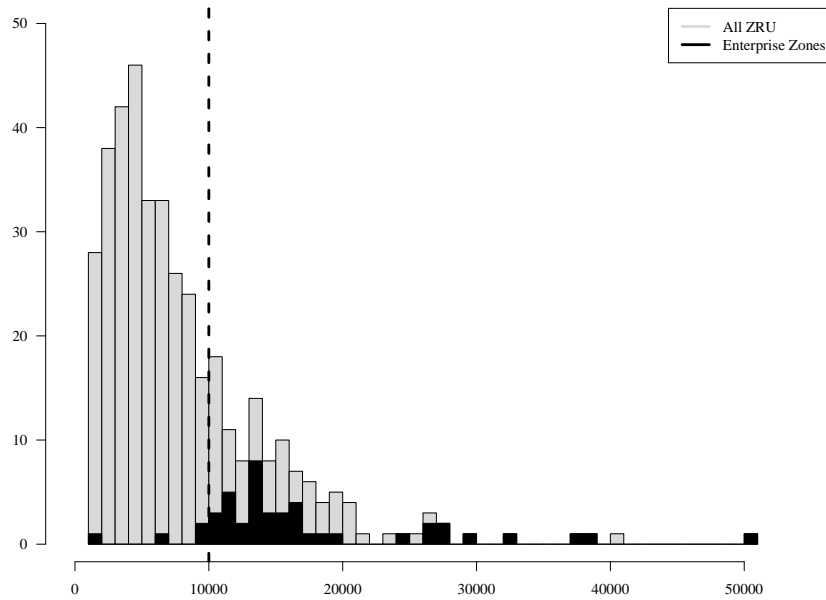
Time-differentiation is not sufficient to accurately estimate the causal impact of the ZFU system. Indeed, the ZFU were chosen among ZRU suffering from multiple economic handicaps that may also have had an impact on the economic perspective. However, the two-step assignment process does provide us with an identification strategy.

First, the eligibility condition based on the size of the areas (in terms of inhabitants estimated in the 1990 population census) ensures that non-ZFU areas comparable to ZFU in terms of socio-economic development can be found. Indeed, almost all ZFU have more than 10,000 inhabitants (see Figure 4.2).³ This assumption is supported by descriptive statistics on socio-economic characteristics (see Table 4.2). For each criterion (unemployment rate, percentage of social housing, percentage of young people, foreign people and unskilled people in the area, and potential tax revenue in the municipality in 1996), average figures in small ZRU (meaning those populated by less than 10,000 inhabitants) appear close to ZFU; in some cases, they are worse. For instance, the average unemployment rate is 22% in ZFU while it is “only” 18% in big ZRU but 24% in small ZRU. The proportion of unskilled people is 43% in ZFU, while it is 36% (respectively 46%) in big (respectively small) ZRU.

Second, as we know and measure the characteristics used in the ZFU designation, we can accurately control for differences arising from this selection process. This suggests the choice, common in this literature, of an estimation based on the propensity score method. However, we adapt the estimation to take into account the discontinuity introduced by the eligibility threshold. Moreover, this eligibility condition provides an alternative identification strategy based on regression discontinuity that we will use as a robustness check of our results.

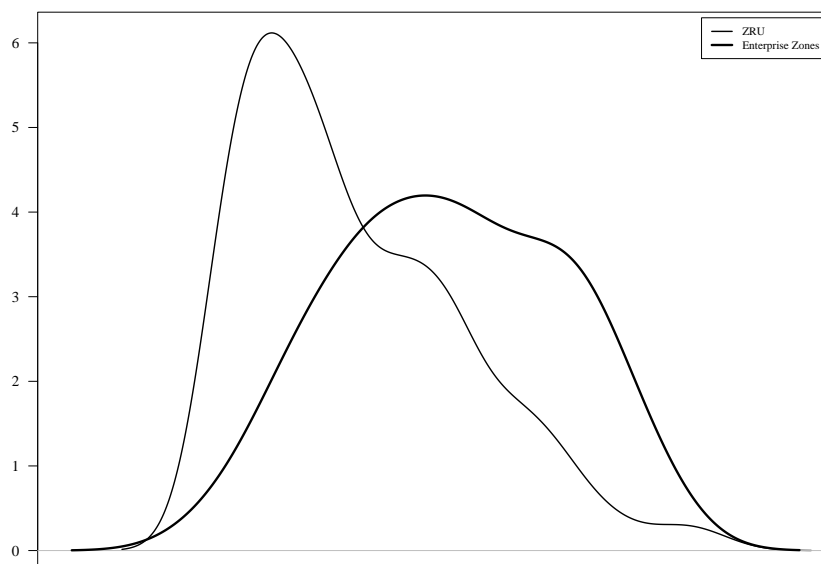
³With the exception of four areas: two very small zones that were merged to bigger ZFU, and two areas that are just below the threshold, with 9,538 and 9,927 inhabitants, respectively.

Figure 4.2: Distribution of ZFU and ZRU according to the number of inhabitants



Source: 1990 Population Census.

Figure 4.3: Score density for the treated and control groups



Source: 1990 Population Census.

4.4.2 Estimator based on the propensity score

Subclassification on the propensity score and regression

In practice, we compare the evolution of outcomes in ZFU by using areas that do not benefit from the ZFU initiative, but are similar in terms of socio-economic characteristics. More specifically, our main assumption is the standard “conditional independence assumption” (CIA) (or unconfoundedness assumption) which states that, in the absence of the policy, no difference would have been observed in the evolution of outcomes in zones with comparable observable characteristics. As we use outcomes in temporal differences, this method is often named “conditional difference in differences”.

As shown by Rosenbaum and Rubin (1983a), if the CIA holds for observables X , it also holds for the propensity score $P(T_i = 1|X)$ (i.e. the probability of an area being designated as a ZFU, conditional on observables). In practice, we use as control variables the indicators formally used for the designation of ZFU, but also other indicators that may have an impact on both designation and economic outcomes, namely the proportion of foreigners and executives in the area, as well as the proportion of stable households and the amount of social housing.

However, as our sample size is small, simple propensity-score matching might lead us to compare units with different observable characteristics (as areas with close propensity scores may still have different observable characteristics). To address this issue, we adopt a strategy that combines regression and propensity score methods for the final estimate of the impact of the ZFU. More precisely, we define four strata corresponding to the level of the propensity score, and perform a linear regression using observable covariates X . As discussed by Imbens and Wooldridge (2009), the linear regression (originally suggested by Rosenbaum and Rubin, 1983b) helps to eliminate potential remaining bias and to improve precision. Within each block, the propensity score does not vary much, and covariate distributions are on average similar between both groups. This insures that the regression function will not extrapolate, perhaps erroneously, into regions outside the data range. The estimate of average treatment effect on the treated (ATT) corresponds to the weighted average of these local estimates.

Formally, and using notation posited by Imbens and Wooldridge (2009), we perform the linear regression in each stratum j :

$$\Delta_{1995} \log(Y_{it}) = X_i \beta_j + \delta_j T_i + u_{ij} \quad (4.1)$$

If we denote J the number of strata (four in our estimates), the final estimate of the impact of the tax subsidies on the ZFU δ_{ATT} corresponds to:

$$\hat{\delta}_{ATT} = \sum_{j=1}^J \frac{N_{jZFU}}{N_{ZFU}} \hat{\delta}_j$$

and an estimate for its variance is:

$$\hat{V} = \sum_{j=1}^J \left(\frac{N_{jZFU}}{N_{ZFU}} \right)^2 \hat{V}_j$$

where $(\hat{V}_j)_{j=1,\dots,J}$ corresponds to the estimated variances of $(\hat{\delta}_j)_{j=1,\dots,J}$ (assuming that the residuals for different strata are independently distributed, which is a standard assumption in this kind of method) and N_{jZFU} and N_{ZFU} respectively denote the number of ZFU in strata j and in the whole sample. We introduce the number of inhabitants of the area as an additional covariate in (4.1), as an informal test of the assumption of conditional independence of outcome and size. It is never significant.

Propensity score estimation

Because of the eligibility condition based on the number of inhabitants in the area, we adapt the estimation of the propensity score to this specific setting. This size condition reinforces the credibility of our identifying assumption, but it can impact the estimation of the propensity score. Indeed, it leads to a censoring for the observed status (ZFU or not) of an area. If some observable characteristics used for the score are correlated with the size of the area (or, to put it differently, if the distribution of observables is not the same in small and large areas as shown in Table 4.2 for instance), a “naive” estimation of the propensity score may lead to biased estimates of the correlation between observed covariates and the score.

Formally, we can assume that the fact of being selected as a ZFU, T_i , depends on covariates X , but an area is selected as a ZFU if $T_i = 1$ and $S_i > \underline{S}$. As we wish to evaluate the impact of being selected as a ZFU, we are interested in estimating the propensity score $P(T_i = 1, S_i > \underline{S}|X)$, which may be decomposed as $P(T_i = 1|X_i, S_i > \underline{S})P(S_i > \underline{S}|X)$. Under mild assumptions,⁴ we can estimate both components separately.

In practice, this means that we estimate as a function of the covariates both the probability of being a ZFU (restricting the sample to areas with more than 10,000 inhabitants) and the fact of having more than 10,000 inhabitants. The second estimation has no causal interpretation, but corrects for misspecification due to differences in the distributions of the covariates in large and small areas. In both cases, we rely on logistic specifications. These estimations are provided in Table 4.3. The first column displays the estimated impacts, for a ZRU with more than 10,000 inhabitants of various socio-economic criteria, on the probability to be included in the ZFU program (i.e. $P(T_i = 1|X_i, S_i > \underline{S})$). As expected, this probability is an increasing function of the unemployment rate and the proportion of young and unskilled people, and a decreasing function of the potential tax revenue of the municipality, as it corresponds to the criteria in the selection process. ZFU are also characterized by a higher proportion of social housing and foreigners, and a lower proportion of executives and stable households. We now turn to the estimates of the impact of the same range of criteria on the probability for a ZRU to have more than 10,000 inhabitants (i.e. $P(S_i > \underline{S}|X)$); the results are displayed in the second column. They suggest that the distribution of these variables (in particular, proportion of executives, potential tax revenue and proportion of unskilled people) are not the same in small and in large ZRU.

⁴Meaning that in the absence of this eligibility condition, the fact of being ZFU T_i is independent of being a “big” area D_i conditional on the characteristics X , where the dummy $D_i = 1_{S_i > \underline{S}}$ indicates whether the size is higher than 10,000 inhabitants or not. Indeed one may easily show that the likelihood of the observations $(D_i, T_i D_i)$ is separable in both components.

Table 4.3: Logit regressions for propensity score estimation

<i>Dependent variable:</i>	Zone selected as ZFU (1)	More than 10,000 inhabitants in the zone (2)
Pct. of executives	−12.78 (43.33)	54.28*** (17.87)
Pct. of stable households	−1.69 (1.06)	0.89* (0.47)
Unemployment rate	13.24* (7.38)	−2.24 (2.76)
Pct. of young people	16.22* (8.90)	4.99 (3.26)
Pct. of social housing	−3.45* (2.06)	−0.85 (0.73)
Potential tax revenue	−0.001*** (0.73)	0.0003** (0.0001)
Pct. of foreigners	14.22*** (5.45)	−0.62 (1.87)
Pct. of unskilled people	61.86** (27.53)	22.36* (12.05)
Pct. of unskilled people ²	−78.58** (33.18)	−32.10** (14.40)

Notes: The first column corresponds to logit regression of the conditional probability of being selected as ZFU, restricted on the areas with more than 10,000 inhabitants. The second column corresponds to the logit regression of the fact of having more than 10,000 inhabitants. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: 1990 population Census.

Finally, the estimated propensity score for one ZRU corresponds to the product of the two predicted probabilities (see Table 4.3), given the observed covariates of this area. Figure 4.3 shows the density of the propensity score for both the treated and control groups. The treated group contains 45 zones, and the control group contains 351 zones. As expected, we observe two distinct modes, meaning that the distributions of the covariates are different in both groups. However, the common support is large, meaning that comparable areas may be found for most ZFU.

4.4.3 Regression Discontinuity Design

We also use the eligibility threshold to propose an alternative strategy. Indeed, the probability that a given ZRU will be chosen to benefit from the ZFU program increases sharply at the 10,000-inhabitant threshold (see Figure 4.7 in the appendix). The design is only “fuzzy,” as the ZFU selection process is not a deterministic function of population size : numerous large ZRU have not become ZFU, and a few small ZRU have become ZFU. Our setting is very similar to that of Battistin and Rettore (2008), where endogenous selection occurred amongst a pool of eligible units. One threat to the validity of this approach arises when the selection variable can be manipulated by economic agents in order to be on the “favorable” side of the threshold. In our case, this selection variable was measured in the 1990 population census, meaning five years before the selection process, so manipulation appears very unlikely.

The fuzzy estimator can be obtained using a two-stage-least-square on the linear regression (for detail, see Imbens and Lemieux, 2008), restricting the estimation sample to units in a small neighborhood to the left and right of a threshold S , defined with a bandwidth h by $[\underline{S} - h; \underline{S} + h]$.

Formally, we estimate

$$\Delta_{1995} \log(Y_{it}) = \alpha + \delta T_i + \beta(S_i - \underline{S})1_{S_i > \underline{S}} + \gamma(\underline{S} - S_i)1_{S_i < \underline{S}} + \gamma X_i + u_i \quad (4.2)$$

by two-stage least squares using the indicator $1_{S_i > \underline{S}}$ as an excluded instrument.

A common tradeoff has to be made between increasing precision using a large bandwidth at the risk of using non-comparable units, and a small bandwidth that shrinks the estimation sample. In our case the tradeoff is constrained by the initial small sample size: for instance considering the rather narrow window [9,000;11,000] inhabitants left us with only 34 areas (including 5 ZFU) that compromise statistical analysis. We have tested several sizes of the window around the threshold and have verified that only the precision is altered by this choice. As is commonly done, we correct for potential dependency in the selection variable by a linear specification. Because of the small sample size, it appears difficult to control for more complex dependence of outcomes on size areas using a polynomial specification of higher order. For the same reason, it is not possible to include variables used in the selection process of ZFU, in contrast to a more flexible method such as the propensity score method. For this reason, and also because it makes our results more directly comparable to the results obtained by previous studies that evaluate the (second wave of) the ZFU initiatives, we thus keep the propensity score matching method as our main specification.

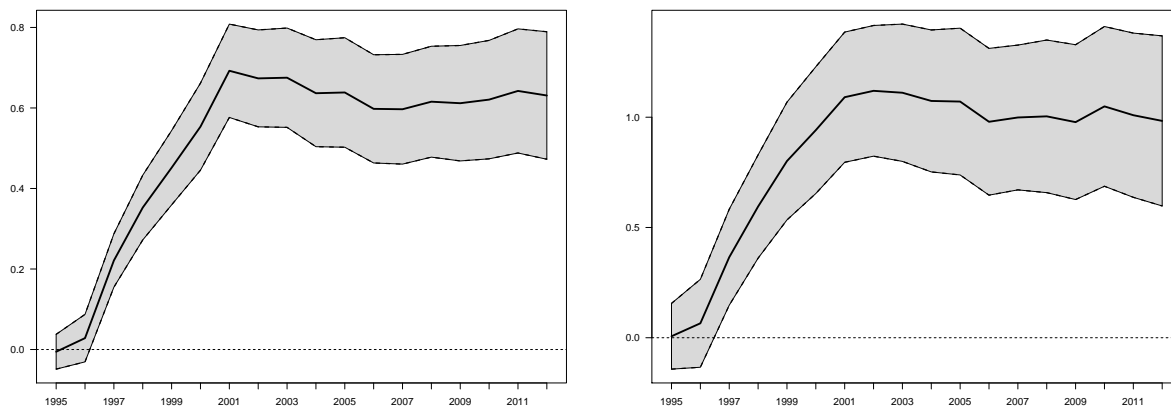
4.5 Results

4.5.1 Short-term effects

According to our results, Enterprise Zones have a strong impact on economic activity in targeted areas. Figure 4.4 displays the cumulative impact of the ZFU program over time on number of plants and (salaried) employment. Tax exemptions result in a steady rise in the number of firms over the first five years. In 2001, the estimate of the impact of ZFU on the time-differentiated log number of plants located in Enterprise Zones is 0.7. This means that the level reached in 2001 is $e^{0.7} \approx 2$ times higher than the level that would have prevailed without the policy. The estimated impact of ZFU on salaried employment is similar. From 2001, the number of salaried employees in ZFU firms is 3 times higher than its counterfactual level, according to our estimates. More concretely, a back-of-the-envelope estimate derived from our results suggests that the whole program would have resulted during the first five years in the location of around 11,000 firms employing 50,500 workers.⁵ It is worth stressing that this effect strongly exceeds the findings of previous studies that evaluate the second-generation of the French Enterprise Zones (see Rathelot and Sillard, 2008, Givord et al., 2013). We discuss this point in section 4.6.

⁵The impact of the ZFU in the outcome in level corresponds to $(1 - e^{-\delta t})Y_{i,t}$.

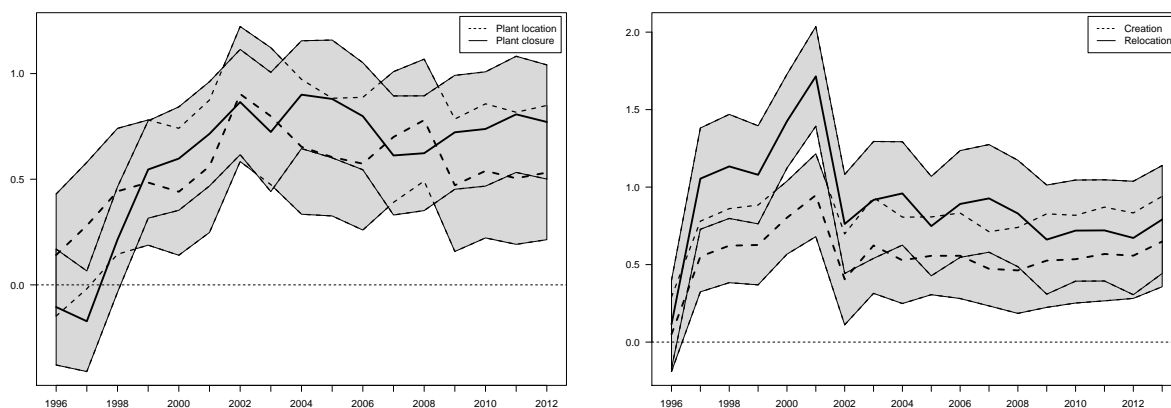
Figure 4.4: Impact of ZFU on the stock of plants (left panel) and employment (right panel)



Note: detailed results are displayed in Tables 4.10 and 4.11 in the appendix.

Source: SIRENE and DADS, 1995-2013.

Figure 4.5: Impact of ZFU on the number of plant location and closure (left panel) and plant relocations and creations (right panel)



Note: detailed results are displayed in Tables 4.10 and 4.11 in the appendix.

Source: SIRENE and DADS, 1995-2013.

As illustrated by Figures 4.4 and 4.5, our estimates also confirm that prior to the implementation of the ZFU program, the trend of economic activity was similar in future ZFU and in zones used as a control group. Indeed, when applying the same estimation method to periods before the introduction of the ZFU initiative (corresponding a “placebo” or “falsification” test), we cannot reject the null hypothesis of a null impact of the ZFU before the implementation of tax exemptions in 1997. In addition, we provide a placebo test on a longer time period at the municipality level, since precisely located firm data are not available before 1995 (see Table 4.13 in the appendix). We highlight a similar trend between control and treated municipalities between 1990 and 1996, while we still find a positive impact of the ZFU program after 1997.

4.5.2 Long-term effects

Despite a promising start, several points cast doubt on the ability of the ZFU program to impart a long-lasting momentum to economic activity. First, the impact appears to stabilize for both employment and firm location after 2001. This can be explained by the fact that in-zone business locations are now canceled out by relocations outside the ZFU, and business closures. Between 1997 and 1999, ZFU had a notably higher impact on the creation than on the shutdown of companies with salaried employees. From 1999, the two levels are very similar (see left panel in Figure 4.5).

Besides, an increasing part of business location is due to relocation. The ZFU impact grows between 1997 and 2001, which are respectively the first year out and the fifth year out from the policy implementation. This trend is greater for relocations than for actual creations. More precisely, the estimated impact with respect to plant relocation rises from 1.1 in 1997 to 1.7 in 2001 whereas it only increases from 0.6 to 0.9 for real creations (see right panel in Figure 4.5). To put it another way, in 2001 the number of firm relocations in ZFU was 5.6 times higher than the level that would have prevailed without tax exemptions, while the number of true business creations was 2.6 times higher than its counterfactual level. Firm creations are still predominant among firm locations (at its highest, the share of relocations is 35% in 2001 while it was 19% in 1995).⁶

Furthermore, whereas both newly located and existing plants benefit from tax exemptions, the ZFU initiative has no impact on employment in existing plants (see Table 4.11 in the appendix). The higher level of employment (compared to the level that would have prevailed in the absence of the ZFU policy) appears to be solely due to plants established in ZFU after 1995.

Finally, the number of plant locations, and especially relocations, increases sharply in ZFU in 2001 but this impact declines in the subsequent year. This suggests that firms do anticipate the end of tax exemptions scheduled for 2001, the terminal date for the policy when it was first announced. Businesses were required to locate in a ZFU before December 31, 2001 to benefit from tax exemptions. The return of the

⁶In 1995, 35 firms were created and 8 firms were relocated per ZFU on average. Using our estimates for 1997 (resp. 2001), we find that 74 (resp. 92) firms were created and 26 (resp. 48) firms were relocated per ZFU.

conservative party to power in 2002 led to the reactivation of the ZFU program after 2003 and firm creation almost recovered to the initial level. This temporal profile suggests that ZFU would not have created economic momentum in targeted areas that outlives the (costly) tax incentives.

4.5.3 Impact on ZFU residents

The original purpose of ZFU was to contribute to urban renewal. The rise in economic activity of firms was viewed as a way to improve the situations of local residents. Concerning this primary objective, our estimates suggest that the ZFU program suffers from a lack of accurate targeting. Indeed, low priority outcomes are much more affected by the ZFU program than the hiring of residents and the development of local services.

Our data allow us to distinguish between resident employees, namely employees who live in the municipality in which the ZFU is located and who work in the ZFU,⁷ with non-resident ones. Indeed, local employment may increase both because more residents are hired, and also because of the hiring of commuters who live further afield. We observe that resident employment does increase at a steady pace between 1997 and 2002 thanks to the ZFU initiative (see Table 4.4). However, the share of residents in the total employment is significantly lower compared to the pre-treatment situation. In addition, with the exception of 1997, we do not find that the hiring of unemployed people is more frequent in ZFU, while the program aims to help them back into work. The finding is in line with Gobillon et al. (2012) who focus on the Paris metropolitan region specifically, and show that the ZFU initiative has only a small and non-persistent effect on the unemployment rates of people living in the cities targeted by the ZFU program. Finally, we find no evidence that people would more frequently settle down and live in the ZFU where they work after the program implementation. This suggests that the local employment clause has not been manipulated by employers asking their employees to move in the ZFU.

Additionally, we decompose the impact of ZFU on local employment by skill (see Table 4.5). As low-skilled residents are over-represented in ZFU and low-paid workers benefit from higher subsidies, a positive effect on low-skilled workers could be counted as an achievement of the ZFU program. We observe that the program does have a positive impact on unskilled workers. After five years, our estimates reveal that the quantity of unskilled employment in the areas is up to 3.1 times the level we should have expected in the absence of the policy. However, these figures are not significantly different from those observed for skilled employment.

Finally, we break down results at the industry level, in order to evaluate whether or not the ZFU program helps to improve local services. Policy makers originally intended to support local amenities, for instance, small retail shops such as convenience stores, and professional services such as physicians. These correspond to the industrial sectors defined respectively as "trade" and "health, education and community services." According to our results, the ZFU initiative has a positive impact on both sectors (see Table 4.5). However, the impact is smaller than the overall effect

⁷Resident employment can only be defined at a municipality level, as the place of residence of workers is not as precisely known as firm location.

Table 4.4: ZFU effect on changes in local employment compared to 1996 level

	Residents	Non-residents	% residents	% new residents	% previously unemployed
1996	0.018 [-0.126,0.161]	0.085 [-0.072,0.242]	-0.028 [-0.114,0.058]	-0.023 [-0.094,0.049]	0.128 [-0.239,0.495]
1997	0.224** [0.045,0.402]	0.391*** [0.201,0.581]	-0.081 [-0.191,0.029]	-0.034 [-0.114,0.046]	0.458** [0.107,0.809]
1998	0.424*** [0.228,0.62]	0.639*** [0.413,0.865]	-0.118** [-0.235,-0.001]	-0.024 [-0.1,0.052]	0.251 [-0.125,0.627]
1999	0.599*** [0.373,0.826]	0.815*** [0.543,1.088]	-0.119** [-0.228,-0.01]	-0.003 [-0.062,0.055]	0.041 [-0.302,0.385]
2000	0.821*** [0.592,1.05]	1.034*** [0.732,1.336]	-0.107* [-0.231,0.017]	-0.008 [-0.074,0.058]	0.138 [-0.212,0.487]
2001	0.943*** [0.707,1.178]	1.169*** [0.861,1.477]	-0.118* [-0.252,0.017]	-0.056 [-0.14,0.028]	0.186 [-0.164,0.535]
2002	0.987*** [0.737,1.237]	1.189*** [0.874,1.505]	-0.107 [-0.25,0.036]	-0.043 [-0.162,0.075]	0.187 [-0.231,0.605]
2003	0.988*** [0.734,1.242]	1.174*** [0.855,1.493]	-0.095 [-0.235,0.044]	-0.02 [-0.116,0.076]	0.127 [-0.281,0.536]
2004	0.933*** [0.651,1.215]	1.126*** [0.785,1.466]	-0.1 [-0.248,0.049]	-0.017 [-0.101,0.067]	0.056 [-0.3,0.412]
2005	0.915*** [0.634,1.197]	1.131*** [0.785,1.478]	-0.114 [-0.272,0.043]	-0.019 [-0.1,0.062]	0.085 [-0.271,0.441]
2006	0.805*** [0.523,1.088]	1.062*** [0.714,1.409]	-0.137 [-0.303,0.028]	0 [-0.078,0.079]	0.048 [-0.322,0.418]
2007	0.769*** [0.479,1.059]	1.067*** [0.718,1.416]	-0.159** [-0.31,-0.007]	0.013 [-0.059,0.085]	-0.021 [-0.373,0.33]
2008	0.749*** [0.469,1.03]	1.068*** [0.7,1.436]	-0.184** [-0.346,-0.022]	-0.019 [-0.093,0.055]	0.18 [-0.214,0.574]
2009	0.743*** [0.455,1.031]	1.076*** [0.697,1.454]	-0.197** [-0.377,-0.018]	0.022 [-0.051,0.095]	0.057 [-0.348,0.463]
2010	0.788*** [0.484,1.091]	1.163*** [0.776,1.55]	-0.231*** [-0.405,-0.058]	-0.008 [-0.078,0.062]	0.036 [-0.376,0.447]
2011	0.752*** [0.438,1.065]	1.158*** [0.767,1.549]	-0.233*** [-0.404,-0.063]	0.001 [-0.07,0.071]	-0.167 [-0.554,0.221]
2012	0.748*** [0.424,1.072]	1.121*** [0.725,1.516]	-0.216*** [-0.378,-0.054]	-0.01 [-0.079,0.059]	-0.075 [-0.468,0.317]
Nb obs ctrl/treat	351/45	351/45	351/45	351/45	351/45

Notes: Estimates of the fact of being ZFU on $\Delta_{1996} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1996. Estimations are based on propensity score and subclassification (five strata based on the propensity score) using all covariates in Table 4.3 and the number of inhabitants in the area. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

estimated. Indeed the number of trade plants is 1.5 time higher than its counterfactual, whereas the overall effect estimate suggests that the number of plants should have doubled thanks to the ZFU program. As well, a closer look suggests that business service firms are the most responsive to tax breaks. The impact of ZFU is impressive here, as the estimated impact for the number of business service plants in the area peaks at 1.4 in 2001, meaning that the observed level is 4.1 times higher than the counterfactual level. These plants correspond for instance to IT services or office cleaning services, meaning companies whose activities are not necessarily carried out in the neighborhood, but whose legal address can easily be located within the ZFU. Such companies may also relocate easily when they no longer benefit from tax exemptions.

4.5.4 Regression discontinuities design results and other robustness checks

We perform several robustness checks. First, as described earlier, RDD yields similar results. Second, we check that our control group provides an accurate counterfactual of a situation without local taxes.

As discussed above in section 4.4, the selection process for ZFU is suitable for a regression discontinuity method. Indeed, Figures 4.8 and 4.9 (in the appendix) suggest a discontinuous jump in the number of plants and the employment growth rate at around the 10,000-inhabitant threshold. Table 4.6 provides estimates for the number of plants⁸ using the fuzzy RDD. The first two columns correspond to the method based on propensity score and subclassification, the results of which have already been discussed. The next two columns both correspond to fuzzy RDD estimates. Column (3), is based on a regression on the whole sample correcting for dependence on size, corresponding to model (4.2). Column (4) is a two-stage least squares regression using a smaller window around the threshold.

The main conclusions remain unchanged. Using RDD, there is no significant difference between groups before the implementation of the program and the effect is similar and even higher in magnitude subsequently. However, running such RDD estimates entails a loss of precision; this is partly due to the small sample size, as the sample size is limited to 103 areas with 12 ZFU when restricting to a close bandwidth.

Finally, we check whether using ZRU as a control group does not lead to underestimate the impact of the ZFU policy. The counterfactual situation we want to measure is a total absence of any tax exemptions, which is not the case here, as firms located in ZRU benefit from some (limited) tax exemptions. To ensure this does not invalidate our identification strategy, we estimate the impact of the ZRU program, applying the same methodology as for ZFU, using disadvantaged urban areas that do not benefit from tax breaks as a control group (i.e. ZUS, the first tier of French urban policy, see section 4.2). According to our estimates, the tax exemptions provided by the ZRU program are inefficient at fostering economic activity (see the first two columns of Table 4.12). The evolutions of the stock of plants in

⁸For the sake of clarity, we present the estimates only for the stock of plants, but similar conclusions are obtained for all outcomes. Results available upon request.

Table 4.5: ZFU effect on changes in the number of plants compared to 1995 level by industry

	Business services	Trade	Health, education, community work	Construc- tion	Manufac- turing
1995	-0.005 [-0.13,0.121]	-0.025 [-0.09,0.04]	-0.007 [-0.06,0.045]	-0.004 [-0.128,0.119]	0.005 [-0.088,0.099]
1996	0.074 [-0.087,0.236]	0.023 [-0.064,0.11]	-0.029 [-0.117,0.059]	-0.02 [-0.176,0.135]	0.026 [-0.11,0.162]
1997	0.53*** [0.345,0.715]	0.167*** [0.065,0.27]	0.058 [-0.06,0.177]	0.242*** [0.071,0.413]	0.189** [0.036,0.341]
1998	0.877*** [0.652,1.101]	0.218*** [0.107,0.328]	0.161** [0.024,0.299]	0.443*** [0.261,0.624]	0.345*** [0.179,0.511]
1999	0.979*** [0.76,1.199]	0.286*** [0.161,0.411]	0.243*** [0.086,0.4]	0.561*** [0.362,0.759]	0.386*** [0.201,0.571]
2000	1.156*** [0.928,1.384]	0.344*** [0.213,0.475]	0.281*** [0.116,0.445]	0.649*** [0.414,0.883]	0.514*** [0.312,0.715]
2001	1.403*** [1.167,1.639]	0.424*** [0.288,0.561]	0.395*** [0.228,0.562]	0.774*** [0.534,1.014]	0.628*** [0.407,0.848]
2002	1.344*** [1.11,1.578]	0.41*** [0.263,0.557]	0.399*** [0.225,0.573]	0.815*** [0.578,1.051]	0.624*** [0.405,0.844]
2003	1.272*** [1.04,1.504]	0.423*** [0.27,0.577]	0.427*** [0.247,0.607]	0.73*** [0.486,0.975]	0.678*** [0.456,0.9]
2004	1.201*** [0.964,1.438]	0.378*** [0.213,0.543]	0.375*** [0.195,0.556]	0.66*** [0.399,0.921]	0.716*** [0.469,0.962]
2005	1.2*** [0.941,1.459]	0.39*** [0.23,0.549]	0.396*** [0.217,0.574]	0.652*** [0.387,0.917]	0.711*** [0.468,0.955]
2006	1.096*** [0.839,1.354]	0.356*** [0.202,0.511]	0.444*** [0.258,0.63]	0.592*** [0.333,0.852]	0.719*** [0.457,0.981]
2007	1.015*** [0.751,1.279]	0.386*** [0.236,0.536]	0.485*** [0.281,0.688]	0.564*** [0.309,0.819]	0.654*** [0.394,0.914]
2008	1.017*** [0.749,1.284]	0.373*** [0.223,0.522]	0.467*** [0.257,0.677]	0.675*** [0.416,0.934]	0.648*** [0.404,0.893]
2009	1.009*** [0.719,1.299]	0.378*** [0.218,0.539]	0.45*** [0.234,0.667]	0.645*** [0.383,0.908]	0.667*** [0.406,0.927]
2010	1.012*** [0.727,1.297]	0.401*** [0.231,0.571]	0.504*** [0.271,0.737]	0.578*** [0.316,0.839]	0.663*** [0.391,0.935]
2011	1.017*** [0.722,1.312]	0.409*** [0.24,0.577]	0.512*** [0.266,0.757]	0.603*** [0.344,0.863]	0.669*** [0.396,0.941]
2012	1.049*** [0.749,1.349]	0.386*** [0.202,0.57]	0.527*** [0.271,0.782]	0.574*** [0.302,0.846]	0.571*** [0.308,0.834]
Nb obs	351/45	351/45	351/45	351/45	351/45
ctrl/treat					

Notes: Estimate of the fact of being ZFU on $\Delta_{1995} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1995. Estimations based on propensity score and subclassification (four strata based on the propensity score) and using all covariates in Table 4.3 and the number of inhabitants in the area. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

Table 4.6: ZFU effect on changes in plant creations 1995 - robustness test

	Propensity score		RDD	
	(1)	(2)	(3)	(4)
1995	-0.005 [-0.049,0.038]	-0.004 [-0.082,0.074]	-0.072 [-0.283,0.14]	-0.053 [-0.174,0.068]
1996	0.028 [-0.031,0.087]	0.087** [0.017,0.156]	-0.22 [-0.532,0.091]	-0.129 [-0.289,0.031]
1997	0.221*** [0.155,0.287]	0.187*** [0.112,0.262]	-0.085 [-0.438,0.267]	0.036 [-0.143,0.214]
1998	0.353*** [0.272,0.433]	0.312*** [0.169,0.455]	0.153 [-0.193,0.5]	0.21* [-0.004,0.424]
1999	0.452*** [0.359,0.544]	0.435*** [0.26,0.609]	0.325* [-0.047,0.698]	0.334** [0.064,0.603]
2000	0.554*** [0.445,0.662]	0.595*** [0.376,0.813]	0.494** [0.094,0.894]	0.524*** [0.202,0.846]
2001	0.692*** [0.576,0.808]	0.759*** [0.505,1.013]	0.88*** [0.43,1.329]	0.773*** [0.358,1.189]
2002	0.673*** [0.553,0.794]	0.715*** [0.454,0.976]	0.82*** [0.351,1.289]	0.764*** [0.34,1.189]
2003	0.675*** [0.552,0.798]	0.726*** [0.447,1.005]	0.748*** [0.258,1.238]	0.744*** [0.309,1.179]
2004	0.637*** [0.504,0.769]	0.697*** [0.394,1.001]	0.729*** [0.215,1.242]	0.764*** [0.308,1.22]
2005	0.638*** [0.502,0.774]	0.715*** [0.396,1.034]	0.84*** [0.309,1.37]	0.829*** [0.387,1.271]
2006	0.598*** [0.463,0.732]	0.61*** [0.272,0.948]	0.844*** [0.315,1.374]	0.79*** [0.293,1.286]
2007	0.597*** [0.461,0.733]	0.651*** [0.303,1]	0.86*** [0.322,1.399]	0.752*** [0.263,1.241]
2008	0.616*** [0.478,0.753]	0.681*** [0.335,1.026]	0.825*** [0.282,1.368]	0.778*** [0.278,1.278]
2009	0.612*** [0.468,0.755]	0.68*** [0.342,1.018]	0.887*** [0.302,1.473]	0.844*** [0.321,1.368]
2010	0.621*** [0.474,0.768]	0.706*** [0.355,1.057]	0.893*** [0.306,1.479]	0.896*** [0.361,1.43]
2011	0.642*** [0.488,0.796]	0.762*** [0.406,1.119]	0.893*** [0.268,1.517]	0.971*** [0.415,1.527]
2012	0.631*** [0.473,0.789]	0.768*** [0.388,1.148]	0.929*** [0.3,1.558]	0.953*** [0.384,1.523]
Nb obs ctrl/treat	351/45	91/12	351/45	91/12
1st stage F-stat			22.95	14.93
Sample	all ZRU and ZFU	ZRU and ZFU between 7 000 & 13 000 h.	all ZRU and ZFU	ZRU and ZFU between 7 000 & 13 000 h.
Control variables	✓	✓	✓	

Notes: Estimates of the fact of being treated on $\Delta_{1995} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1995. Column (1) corresponds to the method based on propensity score and subclassification; columns (2) and (3) both correspond to a regression discontinuity design method (respectively regression on the whole sample correcting for dependance on size and 2sls local regression on a small window around the threshold); column (4) corresponds to the estimation of being ZRU by a method based on propensity score and subclassification using the sample of ZUS (excluding ZFU) as control group. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

ZRU are almost never significantly different from the the one observed for ZUS over the whole period.⁹ The ZRU program thus has no significant impact on economic activity and provides actually an adapted control group.

4.6 Discussion and conclusion

All in all, our results provide new evidence on the efficiency of place-based programs. The overall assessments are mixed, however. To the question “can such a program attract firms into disadvantaged areas,” the answer is clearly yes. French firms appear to be strongly reactive to tax breaks proposed by the ZFU initiative. The changes in the stock of firms and the local employment are impressive. Five years out from the introduction of the policy, the number of firms doubles compared to the level that would have prevailed without the tax exemptions. On the resident population, the consequences are also positive if not as impressive. We observe a sharp increase in resident and unskilled employment, as well as a clear but weaker rise in the location of firms providing local services.

However, analysis of the long term impacts mitigates these positive results. Indeed, the initial goal of the ZFU policy was not to subsidize local economies endlessly. The first financial impulse was expected to create self-sustaining economic activity, and was thus planned to be temporary. Gauged in this light, the ZFU initiative is less successful. After the first five-year period of tax exemptions, the flow of new firms, while still steady, does not lead to an increase in employment: this is due to a higher rate of business failure or relocations outside ZFU. In other words, once firms cease to benefit from the tax rebates, they seem to be replaced by new firms which enter the program for the first time and can thus benefit from full tax exemption. Indeed, the program was eventually restarted in 2003, meaning that the stabilization in employment level and firm stock observed since this date has been achieved at substantial cost. The brief attempt to halt the program in 2002 results in a significant decrease of firm location for this year. This highlights that the economic attractiveness of ZFU remains largely dependent on tax rebates. Besides, as already noted by previous research on the French ZFU, a large part of the inflow of new firms is due to relocation. Subventions do not create genuine new economic activity, and may have negative externalities on the not-so-advantaged neighborhoods close to the ZFU, as observed by Givord et al. (2013). Another drawback of the program may lie in its lack of clear targeting. The program has certainly achieved one of its main objectives, that of increasing resident employment and revitalizing the neighborhoods. But the impact appears relatively modest compared to the overall cost of the program (estimated at 300 million euros in 2001 according to an official report by the French Parliament). Besides, Gregoir and Maury (2012) for instance observe a negative impact on house prices in some French ZFU of the second wave, which

⁹We observe a small and non-significant impact on the firm stock from 2005 to 2009 and a significant impact on firm creations in 2007. This may reflect the fact that some ZRU became ZFU in 2004 and 2007. As our control group includes some treated areas, it might be “contaminated”: our main results could thus have been underestimated for the end of the period. Reassuringly, we obtain similar conclusions when excluding areas selected for the second and third generation of the ZFU program from the control group (see the last two columns of Table 4.12 in the appendix).

they interpret as a negative signaling effect of ZFU status on the population.

Many questions still remain open, about why and where EZ programs work. The optimal settings of such a place-based policies need to be evaluated, as they propose a wide range of services, tax rebates and subsidies on certain inputs. Recent papers emphasize strong discrepancies linked to the variety of tax cuts (Lynch and Zax, 2011), the services provided (Bondonio and Greenbaum, 2007), the manner in which the zone is managed (Neumark and Kolko, 2010), or the industry to which the firm belongs (Hanson and Rohlin, 2011, Burnes et al., 2012). For France, Briant et al. (2015) highlight the importance of geographic context in the success of the second wave of ZFU.

The mixed success of French place-based policy also raises some questions. In the short run, the impact of the first wave of the ZFU was much higher than that obtained in the second wave by Givord et al. (2013) with similar data and close identification strategy (see also Mayer et al., 2012). Several explanations can be adduced to explain the apparent decline in the attractiveness of ZFU. First, a large program of payroll tax cuts was implemented on a national level and reduced the tax gap between ZFU and the rest of the country. Second, after 2003 the subsidies were more strictly contingent on hiring local workers. This so-called local employment clause (“*clause d’emploi local*” in French) was already in effect between 1997 and 2001, but it may not have been strictly enforced (see Appendix 4.A). Difficulties in hiring skilled workers locally may have discouraged new firms from locating there.¹⁰

Lastly, land availability may have played an important role in this differential impact. Table 4.9 (in the appendix) shows that a larger part of the land is dedicated to industrial and commercial zones, that are likely to host companies, in the ZFU belonging to the first generation, compared to the subsequent ones. This proportion is even larger in fast-growing zones. We also notice that this share rises after the implementation of the program, at the expense of farmland. Consequently, there are grounds for thinking that a ZFU can only host a bounded number of firms, because of limited space. However, this bound is likely to be larger in zones belonging to the first generation. This could also explain why the impact of the program no longer increase after 2002.

¹⁰In 2008, according to a qualitative survey in the ZFU, companies in these zones declare major difficulties in hiring employees inside the area (and minor difficulties in hiring outside the area), as reported in Givord et al. (2013).

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4.A A brief description of the French ZFU tax cuts system

Plants located in ZFU as well as plants in ZRU benefit from several tax exemptions, the extent of which varies from ZFU to ZRU in terms of the amount concerned and the duration of the relief (see Table 4.1). The amounts and the eligibility conditions are modified yearly, but the main elements may be summarized as follows:

First, plants located in ZFU and ZRU benefit from exemptions for **employer payroll taxes** (occupational injury, transportation, housing, family benefit and social insurance contributions). Employees with open-ended or fixed-term employment contracts of more than 12 months are exempt from employer payroll taxes, on the fraction of their salary lying beneath 1.5 times the minimum wage (Smic). In 2006, the ceiling was lowered to 1.4 times the minimum wage. Tradesmen and shopkeepers benefit from a total exemption from health insurance contributions until the salary reaches a level of 1.5 times minimum wage. The duration of this exemption is only one year in ZRU, while in ZFU it comes from 5 years of completed exemption completed by decreasing exemption. Besides, the exemptions concern only new hires in ZRU while they benefit all salaried workers in ZFU, conditional upon the fact that the plant hired 20% of its labor force locally (“clause d’embauche locale”). This condition was not applied in practice in the first years of the ZFU, so in December 2000 a new law reinforced the firms’ obligations in this respect (a specific declaration is required to benefit from tax cuts, and their amounts were reduced for transferred jobs). In December 2002, the needed proportion of local hiring was increased from 20% to 33%.

All plants in ZFU benefit from a full exemption from **corporate income tax** for five years starting from the date they locate in the zone. However, this tax cut is restricted to profit below a certain amount, which implies a maximum gain limited to around 20,000 euros per year. In ZRU the exemption is limited to newly created plants in the area, which benefit from full exemption for 2 years and decreasing exemption for the next 3.

All plants in ZFU with less than 50 employees on 1 January 1997 (or at the time of the first location in the ZFU) also benefit from a full exemption from **local business tax** for five years. In ZRU the exemption concerns plants with less than 150 employees at the current date. This exemption is limited, however, and in ZFU the ceiling is much higher than in ZRU: FF3,000,000 (around 460,000 euros) per year in 2001 while it was only FF920,000 (around 139,000 euros) per year for companies created after 1997 in ZRU (and FF410,000 - around 62,000 euros- for companies present prior this date).

Finally, all buildings located in ZFU belonging to plants liable for the **property tax on buildings** are exempt for 5 years. No such exemption exists in ZRU. Companies in ZFU also benefit from additional exemptions on specific taxes, such as the tax on property transfer for shops (to a maximum FF700,000, i.e. around 107,000 euros), fees for the creation of new office buildings in Ile-de-France (Paris metropolitan region), or total exemption from local land tax for 5 years.

4.B A simulation of the amount of tax cuts at the firm level

In order to assess the magnitude of the incentives at the firm level, we use the individual tax returns database prior to the implementation of the program, and simulate tax cuts using the precise program tax cuts scheme. More specifically, we apply the precise scheme of the ZFU program, as described in Table 4.1, to the distribution of wages and sales in the firm observed in 1995 and compared it to the common tax scheme that applied to all French firms. Using the year 1995 also allows us to avoid having to reckon with any potential changes in the financial structure or wage distribution due to the implementation of ZFU. We perform the simulation on all French firms with less than fifty employees, meaning those eligible for the ZFU program.

The main components are payroll and corporate income tax exemptions. For the former, we use the DADS database that provides gross wages for each employee. We thus apply both national and ZFU payroll tax rates at the worker level, and consolidate these data to simulate the gain a firm derives from locating in ZFU (see Table 4.7). The national payroll tax rates have sharply decreased since 2003 (loi Fillon) for the lowest wages (Figure 4.B), and this change may have reduced the attractiveness of the ZFU somewhat. We thus apply both the tax scheme prevailing in 1997, and the one in force after the implementation of the loi Fillon. For corporate income tax exemptions, we observe the precise amount of corporate income tax paid by firms in 1995. At this date, most of the small firms were not paying corporate income tax (for instance because their yearly sales were too low). So instead of an average amount, we provide the proportion of these firms (meaning those for whom the exemption from tax cuts is not expected to have a direct incentive effect) as well as the median of the corporate income tax paid, conditionally upon having paid a strictly positive amount (see Table 4.8).

Table 4.7: Simulation of labor cost in French small firms and ZFU payroll tax cuts, 1997 and 2005 tax schemes. (thousand of euros)

	1997 payroll tax scheme		2005 payroll tax scheme	
	Labor cost (national level)	ZFU payroll tax cuts	Labor cost (national level)	ZFU payroll tax cuts
Total	40.4	5.9	38.3	4.5
<i>Manufacturing:</i>				
- Food	37.7	6	35.1	3.8
- Final goods	68.2	9.5	65.7	7.7
- Car	148.5	22.1	145.8	18.6
- Capital goods	111.8	15.8	109.2	13.5
- Intermediate goods	136.0	19.2	131.8	15.5
Construction	44.6	7.3	42.3	4.9
Energy	63.6	9.1	63.1	8.5
Trade	44.6	6.6	42.5	4.7
Transportation	70.5	10.6	67.8	8.4
Finance	42.4	5.8	41.3	4.7
Real estate	19.4	2.9	18.1	1.8
Business services	60.0	7.7	58.5	6
Household services	23.1	3.6	21.1	1.7
Health, educ.	23.5	3.8	21.9	1.8

Note: Using 1997 tax schemes (respectively 2005 tax scheme), the estimated median labor cost in French small companies is 40.4 thousand of euros (resp. 38.3). The estimated median payroll tax cuts for being in ZFU is 5.9 thousand of euros (resp. 4.5). Only eligible plants (with less than 50 employees) are considered.

Source: DADS 1995.

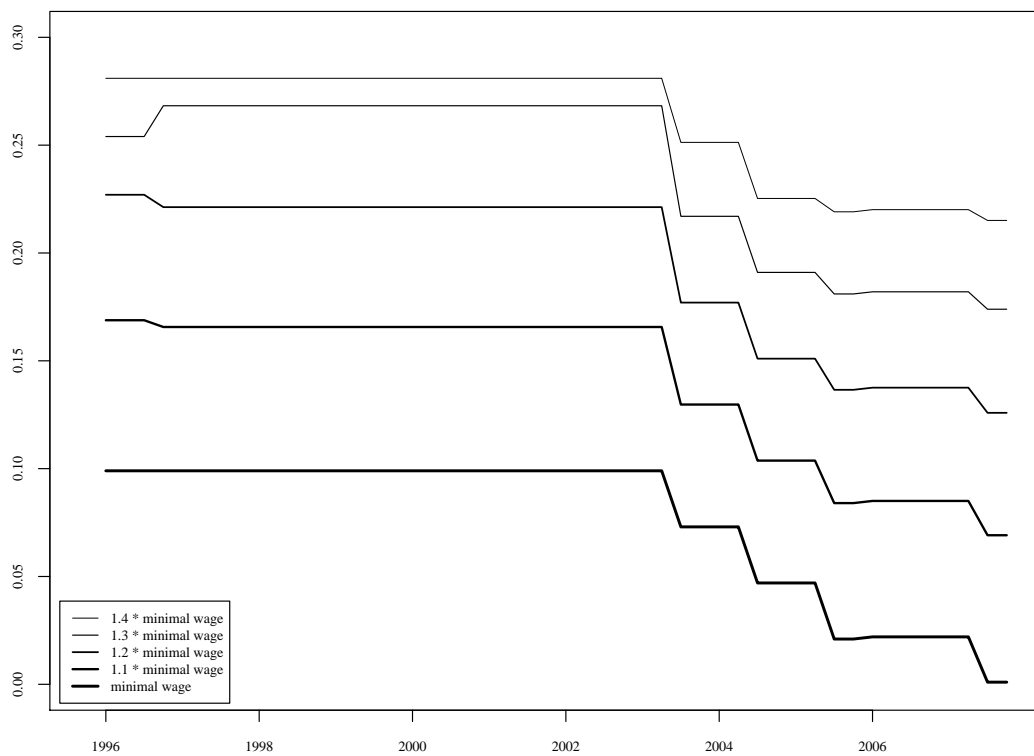
Table 4.8: Summary statistics on corporate income tax (1995)

	Share of untaxed firms (in %)	Median tax paid (for taxed firms) (in k€)
Total:	76.2	3.7
<i>Manufacturing:</i>		
- Food	84.9	3.8
- Final goods	68.5	3.5
- Car	52.0	6.1
- Capital goods	62.5	5.5
- Intermediate goods	55.1	7.2
Construction	83.7	3.2
Energy	58.9	7.5
Trade	73.9	3.8
Transportation	79.5	4.1
Finance	51.7	8.2
Real estate	83.0	2.6
Business services	60.5	3.8
Household services	85.4	2.0
Health, educ.	57.0	3.8

Reading note: for all French firms present in 1995, we estimate the proportion that did not pay any corporate income tax and estimate the median corporate income tax paid for those having a strict positive corporate income tax.

Sources: Fiscal database (BRN-RSI) 1995.

Figure 4.6: Gap between national payroll tax rate and ZFU payroll tax rate according to earning level.



4.C Database construction

Two administrative database from INSEE have been merged. The DADS database provides yearly employment for each company. The SIRENE database follows all French firms. It contents several files which provides the stock of companies on January 1st, each year, as well as firm relocation (the number of new firms created from the 1st of January to the 31st of December of each year). Each company is identified by a registration number. In case of relocation of the company, this registration number changes (in this case, the file corresponding to the flow of companies relates the new and the old registration number). More important, firms are precisely georeferenced. It is crucial for our study as the enterprise zone boundaries do not correspond to the usual administrative borders. The SIRENE database indicates whether the company is located within these boundaries or not, each year from 1995 to 2013.

As a convention, in the text the year t corresponds at a measure at January 1st, $t + 1$ for data on employment and number of firm (which are stock data), while it corresponds to the year t for data on business creation and relocation (which are flow data). This makes the reading of the results easier, as the ZFU effects are expected in 1997 for all data.

The data have been modified for the needs of the study. First, georeferenced creation and relocation data are yearly available over the whole period 1995-2013 while the precise location is missing for some years in the data providing the “stock” of firms recorded on January the 1st of each year. More precisely, the geolocation are not available in this database in 1996, 1998, 2000 and 2001. This information can be extrapolated from others year, however; we indeed have access to the precise identification number of each firm. This identification number change in case of a relocation, the presence of the same identification number in year t_1 and t_2 means that the firm have not moved over the period. To be more specific, consider the case of 1998 stock data, where all firms are registered, but without precise location. If this very same firm is already registered on database of the previous years, we can use the location variable available in these database. Otherwise, it means that the firm has just located in 1998: in this case we find a record in the database for creation and relocation, that contains a location variable for all years. All in all, geolocation, and more precisely ZFU location, can be retrieved for all firms.

Second, the geolocation is not always time-consistent: a company may be registered within a ZFU one year and not the other, even if it is located at the exact same address, and even if ZFU boundaries are not modified. This is due to some inaccuracies in the GIS. If rare, this missclassification can introduce noise in the estimation. When it is the case, we use, by convention, the first location.

Third, we take into account a subtlety of the enterprise zone boundaries. Recall that enterprise zones are selected among most disadvantaged urban areas (ZUS). In some rare cases, a ZFU merges more than one ZUS. As propensity score variables are available at the ZUS level and the ZFU level, our study unit is the ZUS. That is why our treated group contains 44 areas whereas only 38 ZFU where implemented in continental France in 1997. In addition, ZFU boundaries and ZUS boundaries may not exactly match. For the sake of consistency, we choose to restrict to companies

located within both a ZUS and a ZFU.

Finally, using the DADS database we define a measure of business closure rate. Indeed, as we know precisely the level of employment for each firm and each year, we are able to know when a firm dismisses its last employee. More concretely, a company with employees closes in year $t - 1$ if it declares at least one employee on year t and zero employee on year $t + 1$. This measure captures potential relocations outside the area as the identification number of the companies changes in this case. This measure is preferred to this provided directly by administrative data on bankruptcies (due to insolvency, *i.e.* when a company is no longer able to repay its debts), as it is not a reliable measure of discontinuance in business. Not all legal decisions to open bankruptcy proceedings (company filing for bankruptcy as part of legal proceedings) lead to liquidation. Besides, this captures only a little part of discontinuance of business. A plant can, for instance, put a stop to its activity because its owner decides to retire and his assets are not taken over. For the sake of comparison we also define an alternative measure for plant creation. The creation of business with salaried employees corresponds to plants which declare salaried employees for the first time.

4.D Supplementary Figures

Figure 4.7: Share of ZFU areas amongst ZRU as a function of the number of inhabitants

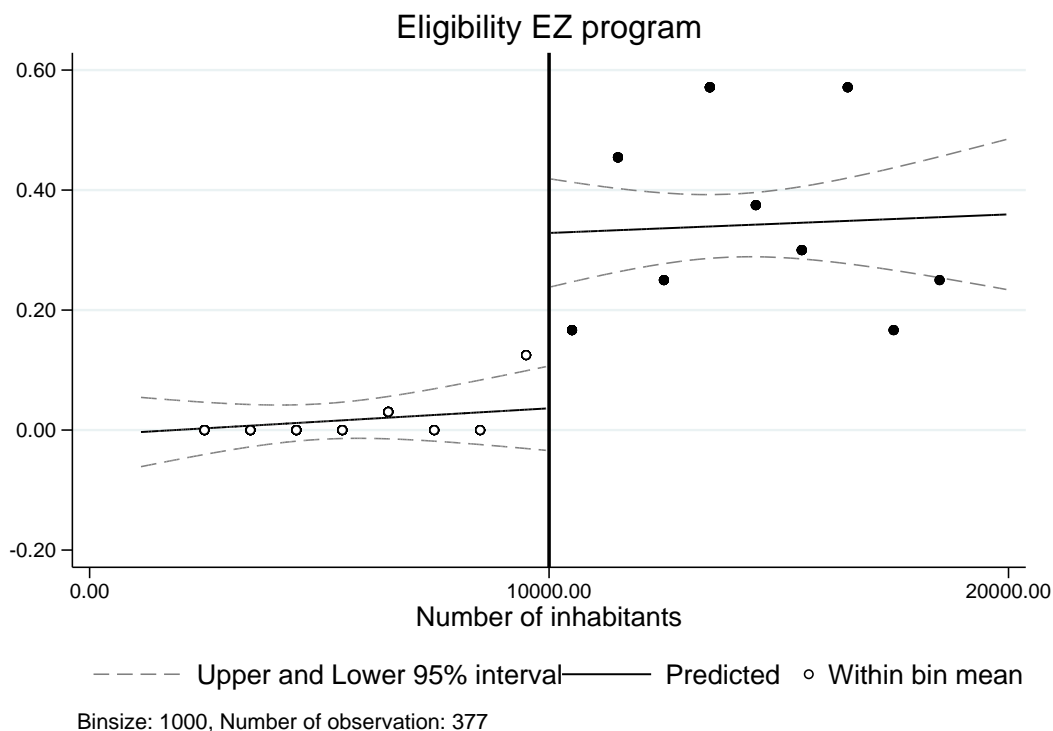


Figure 4.8: Growth rate of the number of plants between 1995 and 2001 - linear fit

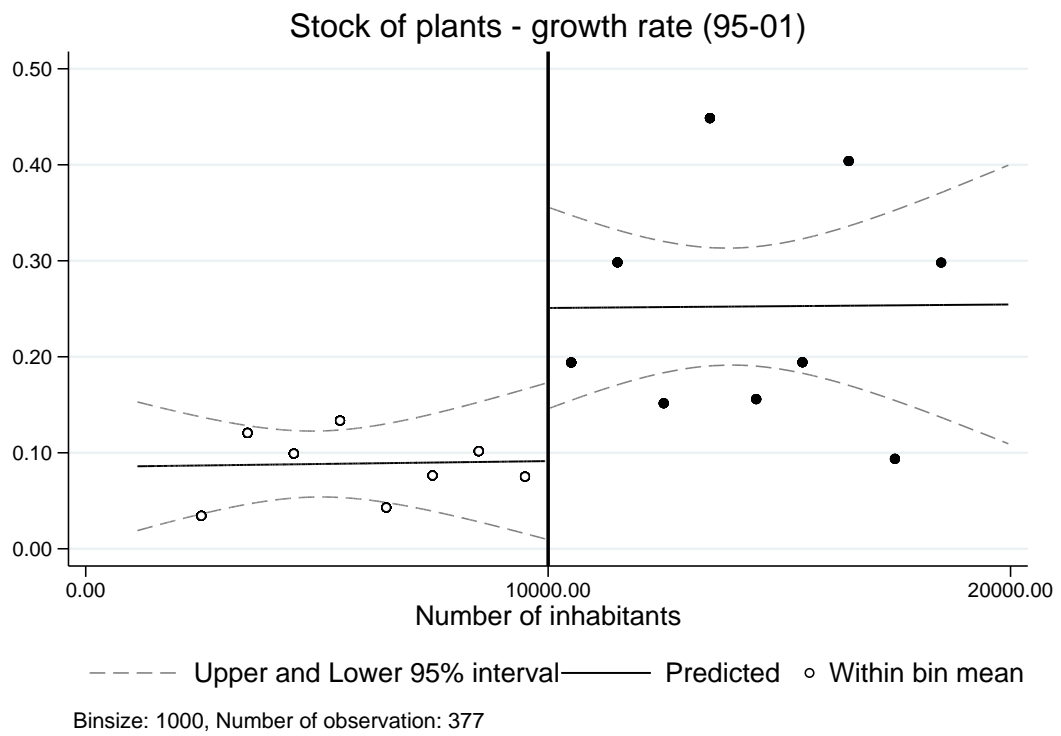


Figure 4.9: Growth rate of the employment between 1995 and 2001 - linear fit

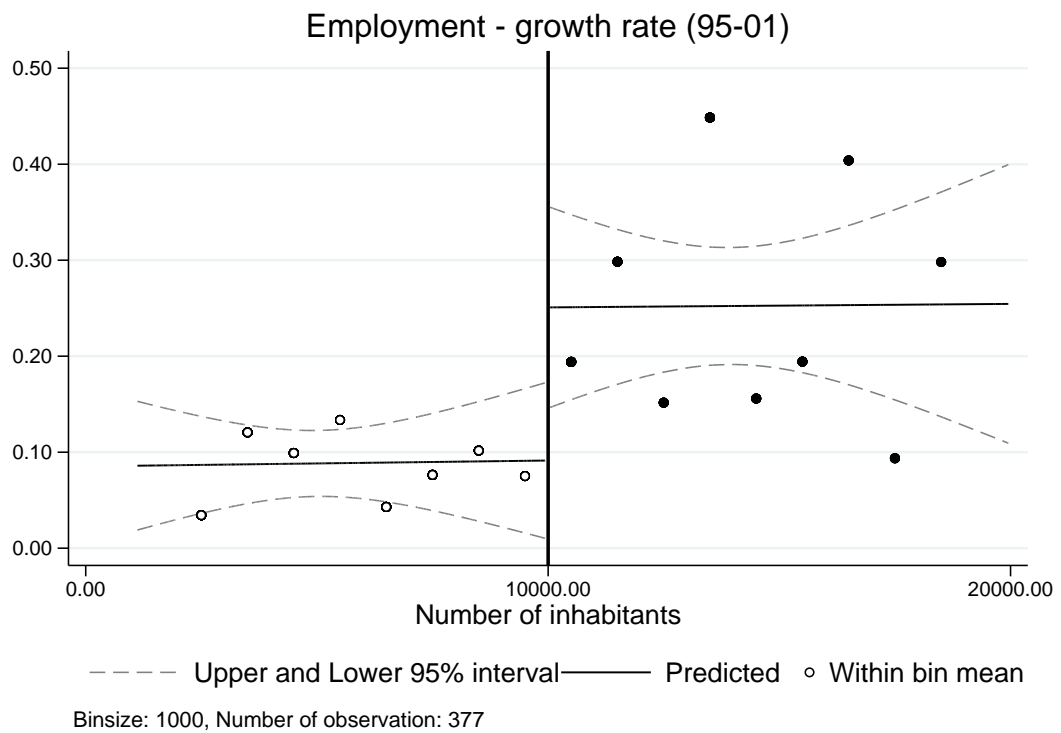


Table 4.9: Land use in the three ZFU waves from 1990 to 2006 (in percent)

Type of zone ZFU generation Growth path	ZFU			ZRU		
	1st	2nd	3rd			
	All	Slow	Fast			
Ind. or com. zone (1990)	17	11	24	12	10	10
Urbanized area (1990)	70	78	63	81	77	85
Natural area (1990)	2	2	1	2	3	1
Farmland (1990)	11	10	12	6	10	4
Ind. or com. zone (2000)	19	12	27	13	11	10
Urbanized area (2000)	71	78	64	81	74	86
Natural area (2000)	1	1	1	2	5	1
Farmland (2000)	8	8	8	4	10	3
Ind. or com. zone (2006)	21	14	29	14	14	10
Urbanized area (2006)	71	78	64	83	73	86
Natural area (2006)	1	1	2	1	5	1
Farmland (2006)	6	7	5	2	9	3
Ind. or com. zone (2012)	24	18	29	16	15	11
Urbanized area (2012)	70	74	66	82	71	86
Natural area (2012)	1	1	2	1	4	1
Farmland (2012)	5	6	3	1	9	2

Note: ZFU are considered as fast growing if the firm stock more than doubled between 1995 and 2002.

Source: Corine Land Cover, 1990, 2000, 2006 and 2012.

4.E Detailed results

Table 4.10: ZFU effect on changes in flows and stock of plants compared to 1995 level

	Stock of plant	Plant creation	Plant relocation	New Plant (DADS)	Plant closure (DADS)	
					all plants	established before 1995
1995	−0.005 [−0.049,0.038]					
1996	0.028 [−0.031,0.087]	0.05 [−0.19,0.291]	0.115 [−0.17,0.399]	0.141 [−0.148,0.43]	−0.105 [−0.379,0.169]	−0.043 [−0.336,0.251]
1997	0.221*** [0.155,0.287]	0.552*** [0.324,0.78]	1.055*** [0.728,1.381]	0.281* [−0.019,0.58]	−0.172 [−0.41,0.066]	−0.145 [−0.397,0.106]
1998	0.353*** [0.272,0.433]	0.622*** [0.383,0.861]	1.134*** [0.798,1.469]	0.443*** [0.145,0.741]	0.214* [−0.035,0.464]	0.041 [−0.229,0.311]
1999	0.452*** [0.359,0.544]	0.627*** [0.369,0.884]	1.079*** [0.763,1.396]	0.484*** [0.188,0.78]	0.546*** [0.316,0.776]	0.059 [−0.22,0.337]
2000	0.554*** [0.445,0.662]	0.803*** [0.567,1.038]	1.425*** [1.122,1.728]	0.441*** [0.14,0.741]	0.598*** [0.353,0.842]	−0.024 [−0.299,0.251]
2001	0.692*** [0.576,0.808]	0.948*** [0.68,1.215]	1.715*** [1.393,2.036]	0.561*** [0.248,0.875]	0.715*** [0.468,0.962]	0.041 [−0.218,0.299]
2002	0.673*** [0.553,0.794]	0.405*** [0.111,0.698]	0.762*** [0.443,1.082]	0.903*** [0.584,1.223]	0.865*** [0.616,1.114]	−0.041 [−0.331,0.248]
2003	0.675*** [0.552,0.798]	0.623*** [0.314,0.933]	0.917*** [0.54,1.294]	0.799*** [0.474,1.123]	0.724*** [0.441,1.006]	−0.075 [−0.386,0.235]
2004	0.637*** [0.504,0.769]	0.528*** [0.249,0.806]	0.959*** [0.626,1.293]	0.653*** [0.334,0.973]	0.9*** [0.645,1.155]	0.151 [−0.119,0.421]
2005	0.638*** [0.502,0.774]	0.557*** [0.306,0.808]	0.748*** [0.428,1.069]	0.605*** [0.326,0.883]	0.88*** [0.601,1.159]	−0.003 [−0.308,0.302]
2006	0.598*** [0.463,0.732]	0.557*** [0.281,0.833]	0.891*** [0.547,1.236]	0.573*** [0.259,0.888]	0.798*** [0.545,1.052]	0.019 [−0.281,0.319]
2007	0.597*** [0.461,0.733]	0.473*** [0.234,0.712]	0.927*** [0.58,1.274]	0.7*** [0.391,1.009]	0.612*** [0.331,0.894]	−0.329** [−0.627,−0.032]
2008	0.616*** [0.478,0.753]	0.463*** [0.185,0.74]	0.83*** [0.487,1.173]	0.78*** [0.491,1.068]	0.623*** [0.352,0.895]	0.125 [−0.169,0.42]
2009	0.612*** [0.468,0.755]	0.526*** [0.224,0.828]	0.661*** [0.309,1.013]	0.472*** [0.158,0.785]	0.722*** [0.453,0.992]	0.044 [−0.203,0.29]
2010	0.621*** [0.474,0.768]	0.535*** [0.252,0.818]	0.719*** [0.393,1.046]	0.54*** [0.222,0.857]	0.738*** [0.467,1.008]	−0.06 [−0.327,0.207]
2011	0.642*** [0.488,0.796]	0.568*** [0.266,0.871]	0.721*** [0.394,1.047]	0.504*** [0.192,0.817]	0.807*** [0.532,1.082]	0.059 [−0.238,0.355]
2012	0.631*** [0.473,0.789]	0.558*** [0.282,0.833]	0.672*** [0.306,1.038]	0.531*** [0.214,0.849]	0.771*** [0.501,1.041]	0.042 [−0.243,0.327]
2013		0.649*** [0.357,0.941]	0.791*** [0.443,1.139]			
Nb obs ctrl/treat	351/45	351/45	351/45	351/45	351/45	351/45

Notes: Estimates of the fact of being ZFU on $\Delta_{1995} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1995. Estimations based on regression and subclassification (four stratas based on the propensity score) using all covariates in Table 4.3 and the number of inhabitants in the area. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

Table 4.11: ZFU effect on changes in the employment compared to 1995 level, detailed by qualification and type of firms

	Total employ- ment	Low- skilled employ- ment	Skilled employ- ment	High- skilled employ- ment	Employ. of plans established before 1995	Employ. of plans established after 1995
1995	0.007 [-0.142,0.156]	0.013 [-0.167,0.194]	0.039 [-0.135,0.212]	0.007 [-0.159,0.172]	-0.046 [-0.16,0.067]	
1996	0.065 [-0.134,0.265]	0.13 [-0.123,0.383]	0.112 [-0.126,0.35]	-0.027 [-0.248,0.194]	0.074 [-0.089,0.236]	0.035 [-0.445,0.514]
1997	0.365*** [0.147,0.583]	0.435*** [0.173,0.697]	0.446*** [0.205,0.686]	0.271** [0.023,0.518]	0.099 [-0.087,0.284]	0.678*** [0.194,1.163]
1998	0.595*** [0.361,0.829]	0.602*** [0.303,0.9]	0.705*** [0.445,0.966]	0.506*** [0.246,0.767]	0.117 [-0.1,0.334]	0.908*** [0.424,1.392]
1999	0.801*** [0.534,1.068]	0.889*** [0.566,1.212]	0.852*** [0.54,1.164]	0.744*** [0.47,1.019]	0.08 [-0.216,0.375]	1.03*** [0.548,1.512]
2000	0.941*** [0.654,1.229]	1.038*** [0.686,1.39]	1.026*** [0.689,1.364]	0.884*** [0.594,1.174]	-0.013 [-0.288,0.263]	1.215*** [0.7,1.73]
2001	1.091*** [0.796,1.386]	1.117*** [0.76,1.473]	1.125*** [0.788,1.462]	1.101*** [0.786,1.416]	0.044 [-0.266,0.354]	1.338*** [0.811,1.865]
2002	1.12*** [0.823,1.416]	1.228*** [0.875,1.58]	1.15*** [0.821,1.479]	1.041*** [0.711,1.371]	0.072 [-0.256,0.399]	1.363*** [0.844,1.882]
2003	1.111*** [0.8,1.422]	1.282*** [0.911,1.653]	1.094*** [0.737,1.452]	1.04*** [0.718,1.361]	0.092 [-0.254,0.438]	1.314*** [0.782,1.847]
2004	1.074*** [0.752,1.396]	1.32*** [0.933,1.707]	1.066*** [0.697,1.436]	0.903*** [0.565,1.241]	0.027 [-0.361,0.414]	1.242*** [0.698,1.786]
2005	1.071*** [0.738,1.404]	1.268*** [0.889,1.647]	1.057*** [0.67,1.443]	0.972*** [0.627,1.317]	0.032 [-0.334,0.399]	1.197*** [0.666,1.728]
2006	0.979*** [0.647,1.312]	1.177*** [0.813,1.542]	0.943*** [0.563,1.322]	0.915*** [0.562,1.268]	0.062 [-0.321,0.446]	1.04*** [0.506,1.575]
2007	0.999*** [0.671,1.327]	1.175*** [0.793,1.558]	1.014*** [0.641,1.386]	0.927*** [0.577,1.278]	0.21 [-0.197,0.617]	1*** [0.458,1.542]
2008	1.004*** [0.658,1.35]	1.029*** [0.646,1.412]	1.252*** [0.856,1.648]	0.886*** [0.528,1.244]	0.152 [-0.274,0.578]	0.992*** [0.445,1.538]
2009	0.978*** [0.627,1.329]	1.052*** [0.648,1.457]	1.2*** [0.796,1.604]	0.814*** [0.451,1.177]	0.089 [-0.368,0.545]	0.979*** [0.433,1.526]
2010	1.049*** [0.687,1.411]	1.128*** [0.722,1.535]	1.177*** [0.758,1.596]	0.934*** [0.552,1.316]	0.172 [-0.334,0.678]	1.033*** [0.491,1.574]
2011	1.009*** [0.637,1.382]	1.041*** [0.613,1.469]	1.168*** [0.75,1.586]	0.947*** [0.546,1.347]	0.253 [-0.268,0.774]	0.96*** [0.409,1.512]
2012	0.983*** [0.598,1.369]	1.019*** [0.593,1.446]	1.125*** [0.687,1.562]	0.887*** [0.478,1.296]	0.228 [-0.294,0.751]	0.937*** [0.377,1.496]
Nb obs	351/45	351/45	351/45	351/45	351/45	351/45
ctrl/treat						

Notes: Estimates of the fact of being ZFU on $\Delta_{1995} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1995. Estimations are based on propensity score and subclassification (five strata based on the propensity score) using all covariates in Table 4.3 and the number of inhabitants in the area. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

Table 4.12: ZFU effect on changes in plants creation, transfer and stock - alternative zones

	Stock of plant	Plant creation	Stock of plant	Plant creation
1995	-0.011 [-0.049,0.027]		-0.017 [-0.068,0.033]	
1996	-0.02 [-0.069,0.029]	-0.017 [-0.16,0.125]	0.011 [-0.062,0.083]	0.015 [-0.273,0.302]
1997	-0.02 [-0.074,0.033]	0.107 [-0.029,0.243]	0.189*** [0.1,0.277]	0.403*** [0.136,0.67]
1998	-0.002 [-0.057,0.052]	0.105 [-0.042,0.251]	0.33*** [0.226,0.434]	0.595*** [0.302,0.887]
1999	0.02 [-0.04,0.08]	0.183** [0.026,0.341]	0.436*** [0.318,0.554]	0.553*** [0.252,0.855]
2000	0.024 [-0.041,0.09]	0.017 [-0.134,0.168]	0.538*** [0.401,0.675]	0.75*** [0.467,1.033]
2001	0.019 [-0.05,0.088]	0.068 [-0.091,0.227]	0.688*** [0.534,0.842]	0.938*** [0.646,1.23]
2002	0.023 [-0.053,0.099]	-0.01 [-0.175,0.155]	0.673*** [0.514,0.833]	0.431*** [0.108,0.755]
2003	-0.003 [-0.08,0.074]	-0.056 [-0.23,0.118]	0.677*** [0.507,0.847]	0.634*** [0.285,0.982]
2004	-0.012 [-0.092,0.069]	0.009 [-0.152,0.17]	0.666*** [0.489,0.844]	0.541*** [0.208,0.873]
2005	0.025 [-0.062,0.112]	0.1 [-0.061,0.261]	0.7*** [0.52,0.88]	0.612*** [0.317,0.907]
2006	0.039 [-0.044,0.121]	0.024 [-0.138,0.186]	0.704*** [0.532,0.876]	0.765*** [0.448,1.081]
2007	0.07 [-0.014,0.153]	0.141* [-0.005,0.287]	0.722*** [0.544,0.9]	0.588*** [0.317,0.858]
2008	0.033 [-0.048,0.113]	0.038 [-0.125,0.202]	0.77*** [0.588,0.953]	0.609*** [0.283,0.934]
2009	0.028 [-0.057,0.114]	0.003 [-0.165,0.171]	0.763*** [0.566,0.959]	0.639*** [0.294,0.984]
2010	-0.013 [-0.098,0.071]	0.036 [-0.131,0.203]	0.784*** [0.589,0.98]	0.736*** [0.412,1.061]
2011	-0.016 [-0.106,0.075]	0.042 [-0.125,0.209]	0.802*** [0.594,1.011]	0.689*** [0.346,1.031]
2012	0.004 [-0.086,0.093]	0.142 [-0.029,0.313]	0.794*** [0.589,1]	0.649*** [0.338,0.959]
2013		0.077 [-0.094,0.248]		0.695*** [0.368,1.023]
Nb obs ctrl/treat	320/351	320/351	286/45	286/45
Treatment group	all ZUS		1st generation ZFU	
Control group	all ZRU		ZRU w/o 2nd and 3rd generation ZFU	

Notes: Estimate of the fact of being ZFU on $\Delta_{1995} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1995. Estimations based on propensity score and subclassification (four strata based on the propensity score) and using all covariates in Table 4.3 and the number of inhabitants in the area. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

Table 4.13: ZFU effect on changes in stock of flows of plants compared to 1990 level at the municipality level

	Stock of plant	Plant creation	Stock of plant	Plant creation	Stock of plant	Plant creation
1990	0.001 [-0.01,0.011]		0.002 [-0.008,0.013]		0.005 [-0.003,0.014]	
1991	-0.001 [-0.015,0.013]	-0.003 [-0.079,0.072]	0 [-0.015,0.015]	-0.008 [-0.091,0.074]	0.003 [-0.009,0.015]	-0.02 [-0.084,0.044]
1992	-0.007 [-0.026,0.012]	-0.018 [-0.088,0.052]	-0.008 [-0.028,0.012]	-0.02 [-0.093,0.052]	-0.001 [-0.017,0.015]	-0.005 [-0.064,0.054]
1993	-0.017 [-0.041,0.006]	-0.013 [-0.097,0.071]	-0.019 [-0.043,0.006]	-0.011 [-0.101,0.079]	-0.003 [-0.023,0.017]	0.009 [-0.062,0.08]
1994	-0.017 [-0.043,0.009]	-0.034 [-0.109,0.042]	-0.018 [-0.045,0.008]	-0.034 [-0.113,0.046]	-0.002 [-0.023,0.02]	0.005 [-0.058,0.069]
1995	-0.016 [-0.045,0.013]	-0.056 [-0.133,0.022]	-0.017 [-0.048,0.013]	-0.064 [-0.146,0.018]	-0.002 [-0.026,0.023]	-0.022 [-0.088,0.043]
1996	-0.016 [-0.048,0.017]	0.013 [-0.073,0.098]	-0.016 [-0.051,0.018]	0.018 [-0.074,0.111]	0 [-0.027,0.028]	0.02 [-0.053,0.092]
1997	0.016 [-0.019,0.051]	0.119*** [0.035,0.203]	0.015 [-0.022,0.052]	0.117** [0.027,0.208]	-0.006 [-0.036,0.024]	-0.029 [-0.101,0.043]
1998	0.036* [-0.002,0.074]	0.115*** [0.031,0.198]	0.036* [-0.004,0.076]	0.121*** [0.032,0.21]	-0.006 [-0.038,0.026]	0.005 [-0.066,0.077]
1999	0.049** [0.009,0.089]	0.108** [0.02,0.195]	0.048** [0.006,0.091]	0.103** [0.01,0.196]	-0.011 [-0.045,0.023]	-0.04 [-0.114,0.035]
2000	0.067*** [0.023,0.111]	0.121** [0.024,0.219]	0.067*** [0.02,0.114]	0.123** [0.016,0.23]	-0.014 [-0.051,0.024]	-0.014 [-0.097,0.069]
2001	0.099*** [0.052,0.146]	0.188*** [0.093,0.283]	0.099*** [0.049,0.149]	0.192*** [0.092,0.293]	-0.018 [-0.059,0.023]	-0.015 [-0.096,0.067]
2002	0.094*** [0.045,0.143]	0.099** [0.011,0.188]	0.094*** [0.042,0.146]	0.102** [0.008,0.196]	-0.017 [-0.06,0.025]	-0.007 [-0.082,0.069]
2003	0.102*** [0.051,0.153]	0.142*** [0.045,0.24]	0.102*** [0.047,0.156]	0.141*** [0.036,0.247]	-0.019 [-0.063,0.025]	-0.028 [-0.111,0.055]
2004	0.105*** [0.053,0.157]	0.09** [0.002,0.178]	0.107*** [0.051,0.163]	0.09* [-0.002,0.182]	-0.011 [-0.056,0.034]	-0.013 [-0.087,0.062]
2005	0.104*** [0.051,0.158]	0.079 [-0.016,0.173]	0.108*** [0.051,0.165]	0.082 [-0.017,0.18]	-0.004 [-0.05,0.042]	-0.001 [-0.081,0.079]
2006	0.107*** [0.052,0.162]	0.078 [-0.017,0.172]	0.113*** [0.054,0.171]	0.085* [-0.014,0.184]	0.003 [-0.044,0.051]	0.016 [-0.064,0.096]
2007	0.113*** [0.055,0.171]	0.074* [-0.012,0.159]	0.122*** [0.06,0.184]	0.085* [-0.002,0.171]	0.015 [-0.035,0.064]	0.031 [-0.041,0.104]
2008	0.114*** [0.054,0.174]	0.04 [-0.047,0.128]	0.125*** [0.061,0.189]	0.049 [-0.042,0.14]	0.023 [-0.029,0.075]	0.027 [-0.047,0.101]
2009	0.116*** [0.054,0.178]	0.092** [0.005,0.178]	0.129*** [0.064,0.195]	0.105** [0.017,0.194]	0.031 [-0.023,0.084]	0.037 [-0.036,0.111]
2010	0.137*** [0.009,0.265]	0.075 [-0.026,0.176]	0.128*** [0.061,0.196]	0.087 [-0.017,0.192]	-0.057 [-0.166,0.051]	0.035 [-0.05,0.12]
2011	0.14** [0.011,0.269]	0.082 [-0.037,0.201]	0.133*** [0.064,0.203]	0.088* [-0.008,0.184]	-0.049 [-0.159,0.06]	0.009 [-0.092,0.109]
2012	0.136** [0.006,0.267]	0.04 [-0.081,0.161]	0.131*** [0.059,0.203]	0.047 [-0.054,0.147]	-0.044 [-0.154,0.067]	0.019 [-0.083,0.121]
2013		0.031 [-0.09,0.151]		0.025 [-0.076,0.126]		-0.027 [-0.129,0.074]
Nb obs ctrl/treat	284/45	284/45	215/45	215/45	260/69	260/69
Treatment group	1st generation ZFU		1st generation ZFU		2nd and 3rd generation ZFU	
Control group	all ZRU		ZRU without 2nd and 3rd generation ZFU			

Notes: Estimates of the fact of being ZFU on $\Delta_{1990} \log(Y_{it} + 1)$, the difference in log outcomes between year t and 1990. Estimations based on a linear regression without controls. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Source: SIRENE and DADS, 1995-2013.

Chapter 5

The Impacts of Housing Subsidies on the Rental Sector: the French Example

This chapter is cowritten with Céline Grislain-Letrémy (Crest and PSL*).

5.1 Introduction

Housing subsidies are a main tool for housing policy in many developed countries. In France, the budget weight of subsidies to tenants reached 16.8 billions of euros in 2014 (CGDD, 2015), namely 0.8% of GDP. These subsidies aim to limit the budget share of housing for tenants and to improve their housing conditions for a given budget share. Despite the rising spending allocated to housing subsidies since the mid-1990s, the proportion of household expenditure that low-income tenants devote to rent has continued to increase, in particular in the private rental sector (Arnault and Crusson, 2012). This suggests a potential incidence of housing subsidies on the level of the rents. Considering that housing supply may be considered as little elastic, at least in the short run, a demand subsidy would lead to a high increase in rents and a small increase in the number and quality of rental dwellings. Under these conditions, housing allowance would be partly captured by landlords.

Several concordant empirical studies have already highlighted and measured the impact of housing subsidies targeting tenants on rents.¹ In the United States, Susin (2002) finds a positive impact of rent vouchers on rents for recipients but also for unsubsidized low-income households. Similarly, Gibbons and Manning (2006) show that a reduction in UK housing benefits decreases rents and that these benefits

¹Subsidies can also target building suppliers. Eriksen and Rosenthal (2010) and Sinai and Waldfoegel (2005) show that the impact of subsidized construction of low-income housing on the housing stock in the United States is limited, because this crowds out equivalent housing that otherwise would have been provided by the private sector.

were massively captured by landlords. For Finland, Kangasharju (2010) yields a similar impact of housing allowances provided to low-income households in the private housing sector. In France, Laferrère and Blanc (2004) and Fack (2006) find a positive effect of housing subsidies on rents in the 1990s.² These two articles use the natural experiment provided by the reform of housing subsidies between 1991 and 1993, which aimed at increasing the number of beneficiaries from housing subsidies. Laferrère and Blanc (2004) highlight that the significant impact of housing subsidies on rents is only slightly explained by an increase in dwelling quality, using the Rents and Charges survey between 1987 and 1999. Fack (2006) determines the impact of housing subsidies on rents for French low-income households. By comparing the evolution of rents for households belonging to the first quartile of standard of living and households belonging to the second one, she finds that the reform of housing subsidies between 1991 and 1993 led to an increase of rents that represented 78% of subsidies. Her results are established by applying a method of difference-in-differences and using the Housing survey between 1973 to 2002.

Our contribution is threefold. First, we offer a new identification strategy to measure the impact of housing allowance on rents. Our study is based on a fuzzy geographic discontinuity in the calculation of housing subsidies. In France, subsidies are approximately 15 to 40 euros per month higher in agglomerations of more than 100,000 inhabitants. This population threshold has not been strictly used to determine the zones with higher subsidies. Some agglomerations with less than 100,000 inhabitants can receive higher subsidies, but they have specific features. Thus, treatment, namely, higher housing subsidies, is endogenous. We use as an instrument a dummy for agglomerations with more than 100,000 inhabitants. We estimate this way a local average treatment effect of housing subsidy on rents in agglomerations relatively close to the discontinuity, that is between 50,000 and 180,000 inhabitants.

Second, we extend the results of Laferrère and Blanc (2004) and Fack (2006), as we measure the potential impact of housing subsidies on rents in France between 1984 and 2012. We find no significant impact in the 1980s, when the expenditure for housing benefit were lower, while we highlight that tenant-based subsidies caused an increase in the rents in the next two decades. This long-lasting impact of housing benefit on the level of the rents suggests that housing supply is inelastic in the long term. This could be the case if local authorities implement restrictive land use policies (Gyourko and Molloy, 2015). Finally, the impact of subsidies on rents is heterogeneous. It is stronger for the dwellings with two rooms or less, for flats and for the recipients of the subsidy.

Third, our method makes it possible to assess the effect of housing allowance on the quality and the quantity of housing supply. Higher subsidies seem to have almost no effect on housing quality and no impact on the size of the privately-rented sector. This is consistent with the idea that the housing supply is quite inelastic. To test this hypothesis, we break down our model on rents depending on a proxy of the tightness of housing markets (namely, the past population growth of the agglomeration). We actually find that housing allowance pushed up rents only in fast-growing cities. Finally, we show that the share of one-room flats in the rental

²See Laferrère and le Blanc (2002) and Fack (2005) for companion papers in French of these two works.

stock is significantly higher where housing benefits are more generous. This type of dwelling is actually more likely to be occupied by the beneficiaries of housing assistance, especially students.

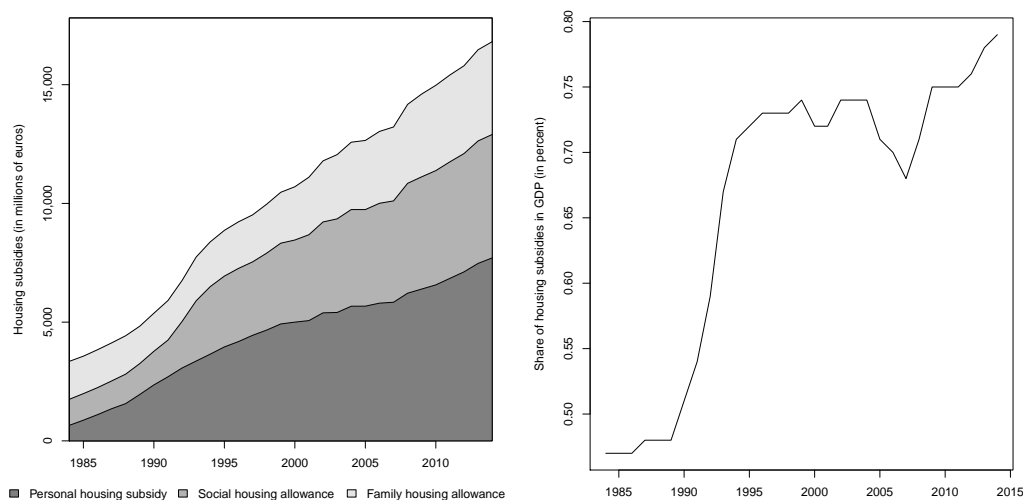
The chapter is organized as follows: section 5.2 describes the French housing benefit policy and especially its spatial heterogeneity; section 5.3 explains the empirical method; section 5.4 presents the data; section 5.5 details the results and section 5.6 finally concludes.

5.2 Housing subsidy in France

In France, public spending for housing aims at easing the burden of housing spending and to improve housing conditions. It targets either housing suppliers or consumers. The share of public spending for housing in the French GDP varies around 2% in the last three decades, namely 40.9 billions of euros in 2014. Housing benefit accounts for half of the spending, 20.5 billions of euros in 2014. Housing subsidies to tenants constitute the most important tool, as they represent 16.2 billions of euros, of which 6.9 billions of euros for the social rental sector (CGDD, 2015).

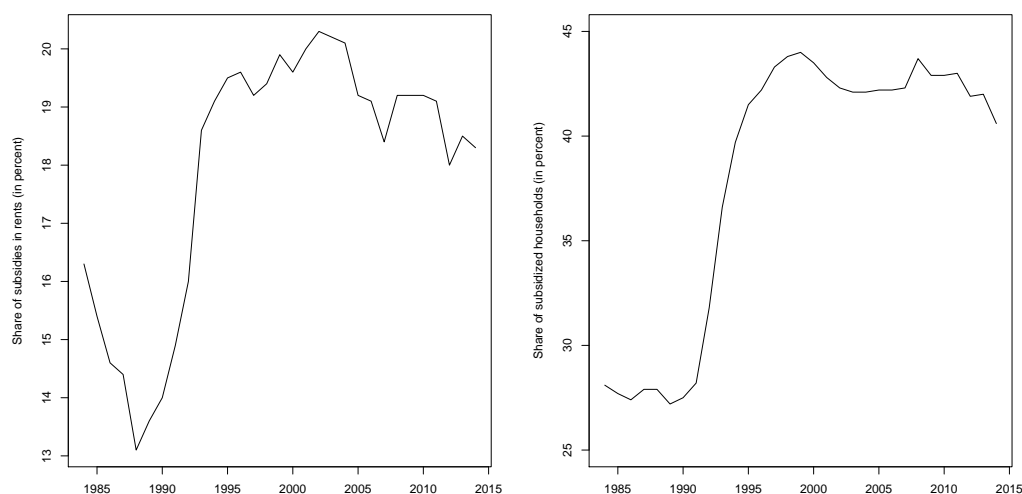
Housing allowance to tenants, which was created in the post-war years, was massively extended since 1977. After this pivotal year in the French housing policy, public finance was directed in the favor of subsidies to households to the expense of building subsidies, which used to prevail. Consequently, the budget weight of subsidies to tenants has kept increasing since 1977 (see figure 5.1). The main factor behind this increase is the growing number of eligible tenants (see figure 5.2). From the 1990s, the whole set of low-income households, including students, could benefit from these subsidies. In the privately-rented sector, the share of subsidized households rose from 27% in 1989 to 44% in 1998. Since the 2000s, the number of beneficiaries is almost constant.

Figure 5.1: Housing subsidies targeting tenants – amounts and weight in the GDP



Source: CGDD, Housing accounts, 2014

Figure 5.2: Share of housing subsidies in rents and share of subsidized households in the private rental sector



Source: CGDD, Housing accounts, 2014

Three main housing subsidies target tenants: personal housing subsidy (“*aide personnalisée au logement*”, APL), mainly for social housing tenants; family housing allowance (“*allocation de logement familiale*”, ALF); and social housing allowance (“*allocation de logement sociale*”, ALS), given to students, childless couples, young, old or disabled people. Even though each subsidy is dedicated to some households or to some dwellings, their method of calculation is common since 2001. These subsidies benefit to the tenants of social or private dwellings, to some homeowners with outstanding loans. The subsidies can be paid to the tenant or directly to the landlord. The calculation of the amount of housing subsidies, which is quite complex (Ministère de l’égalité des territoires et du Logement (2012) and Trannoy and Wasmer (2013), Box 12 pp. 51-52), takes into account household characteristics and resources, as well as, in a very limited extent, on the rent level.

The amount of subsidy also depends on the location in one of three zones. Zone I comprises Paris agglomeration and the new towns in Paris region. Zone II comprises agglomerations of more than 100,000 inhabitants, fringes of Paris region and some agglomerations with a specific housing market (especially tight, such as coastal or border zones, or especially depressed, such as areas affected by industrial decline). Zone III corresponds to the rest of the country (Figure 5.3). The amount of housing subsidies is higher in zone II than in zone III, all other things being equal. In zone I, the amount is even higher. This zoning was determined in 1977 and has been little modified since then, mainly for budgetary reasons. The difference in subsidies between the three zones is not constant and depends on the characteristics of the household. It amounts approximately from 15 to 40 euros per month between zone II and zone III (see Table 5.1 for an example).

Figure 5.3: Housing subsidy zones in France

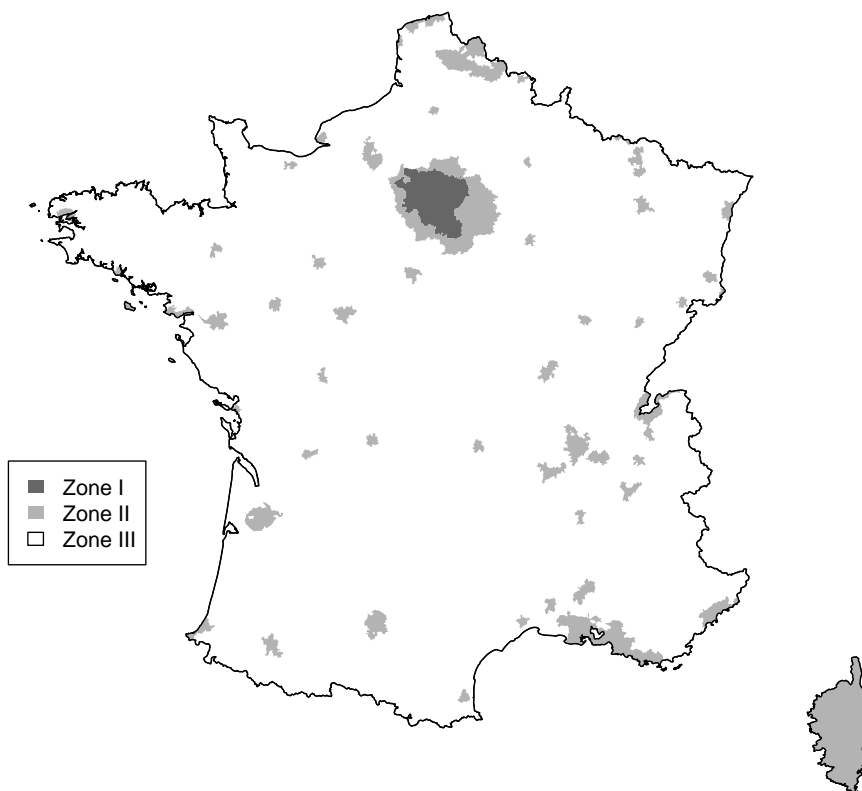


Table 5.1: Example of housing subsidy amounts depending on location and income

Monthly disposable income	0	500	1000	1500	2000	2500
Monthly subsidy in zone II	425	425	340	196	51	0
Monthly subsidy in zone III	398	398	314	173	31	0
Difference in monthly subsidy	27	27	26	23	20	0

Note: housing subsidy amount for a single-parent family with two children, for a monthly rent of 500 euros, according to the 2012 scheme. Authors' calculations.

Table 5.2: Population trend, and shares of private and social housing in zones II and III – average value by municipality

	Zone II	Zone III
Gross rate of agglomeration pop _{1982–09}	8.1%	8.7%
Share of private rental housing ₁₉₈₂	26.1%	28.5%
Share of private rental housing ₂₀₀₉	24.2%	26.2%
Share of social rental housing ₁₉₈₂	18%	21.6%
Share of social rental housing ₂₀₀₉	18.8%	21.5%

Sources: Census 1982 and 1999, agglomerations between 50,000 and 180,000 inhabitants.

5.3 Evaluation method

5.3.1 Evaluation strategy

We apply a method of instrumental variable that relies on the link between the subsidy amount and the dwelling’s location detailed in the previous section. We only use the discontinuity between the two last zones. In fact, zone I includes the Paris region which is too specific to be compared with agglomerations of the other zones. On the contrary, we argue that there are very comparable agglomerations in zones II and III, that mainly differ by the amount of received subsidies. These comparable agglomerations are the ones of which population is just below or just above the population limit between these two zones, i.e. 100,000 inhabitants. Besides, the zoning for other housing subsidies, such as landlord subsidies for rental investment, does not match with this housing subsidies zoning (Table 5.10 in appendix 5.A). Other housing policies could not bias our estimation.

Comparing these agglomerations makes it possible to determine the impact of the payment of housing subsidies on the level of rents. A similar method is used by Bono and Trannoy (2012) to estimate the impact of a rental investment subsidy scheme (the *Scellier* program) on building land prices. However, they compare the evolution of building land prices for bordering municipalities between which real estate markets are potentially interdependent. When comparing here agglomerations and not municipalities across the border, this dependency effect is likely negligible.

The population limit of 100,000 inhabitants between zones II and III has not been strictly used to determine the outlines of the two zones, as some less populated agglomerations were included in zone II (see Table 5.3). In this framework, being located on one side or on the other side of the threshold modifies the probability to be assigned to zone II or III (and so to receive or not higher housing subsidies), without fully determining this assignment.

The treatment effect estimator δ is computed by using the rents of dwellings located in the agglomerations between 50,000 and 180,000 inhabitants (Figure 5.4). This window can be considered as wide but reducing it would lead to keep too few agglomerations in the estimations, and descriptive statistics support the idea of great similarity from both sides of the discontinuity. Even if the average rent per square meter is always higher in the treatment group, its level does not increase with population in both groups for agglomerations under 180,000 inhabitants, which

Table 5.3: Frequency and average rent in function of the agglomeration population

Agglomeration population in 1975	Number of agglomerations		Rent per square meter	
	untreated (zone III)	treated (zone II)	untreated (zone III)	treated (zone II)
20000-40000	48	3	7.2	8.6
40000-60000	27	7	7.8	8.6
60000-80000	21	3	7.5	8.8
80000-100000	9	1	7.1	9.6
100000-120000	0	9	.	8
120000-140000	0	7	.	8.5
140000-160000	0	4	.	8.9
160000-180000	0	1	.	7.2
180000-200000	0	4	.	9.7
200000-220000	0	6	.	9.7
220000-250000	0	2	.	10.4

Source: Rents and Charges survey between 2005 and 2012.

suggests that there is no population trend in the rent level here (Table 5.3). In addition, these agglomerations have a similar population trend, and comparable shares of private and social housing (Table 5.2).

Zone delineation has not been modified much in the forty last years; in our sample, no zoning modification was performed after 1991. Consequently, treatment assignment does not rely on the present population but on the population in the 1970s. It thus can be considered as independent of recent demographic changes in the population of the metropolitan areas. However, agglomerations in which the housing subsidy zoning was modified between 1977 and 1991 are excluded; they represent 4% of the observations. Besides, agglomerations in border areas also are excluded, because they often belong to a wider international metropolitan area, about which we have no information.³

We use the instrumental variable method in a standard linear hedonic model. We regress the logarithm of the rent per square meter R on the treatment T and the characteristics X of the dwelling.⁴ X comprises characteristics that are intrinsic to the dwelling (living area, completion year, etc.) and relative to its location (past growth of agglomeration, median fiscal income of the municipality, share of open space in the municipality). We also add year fixed effects. Finally, we instrument the treatment T with the threshold \underline{P} of 100,000 inhabitants, using a two stage least squares model.

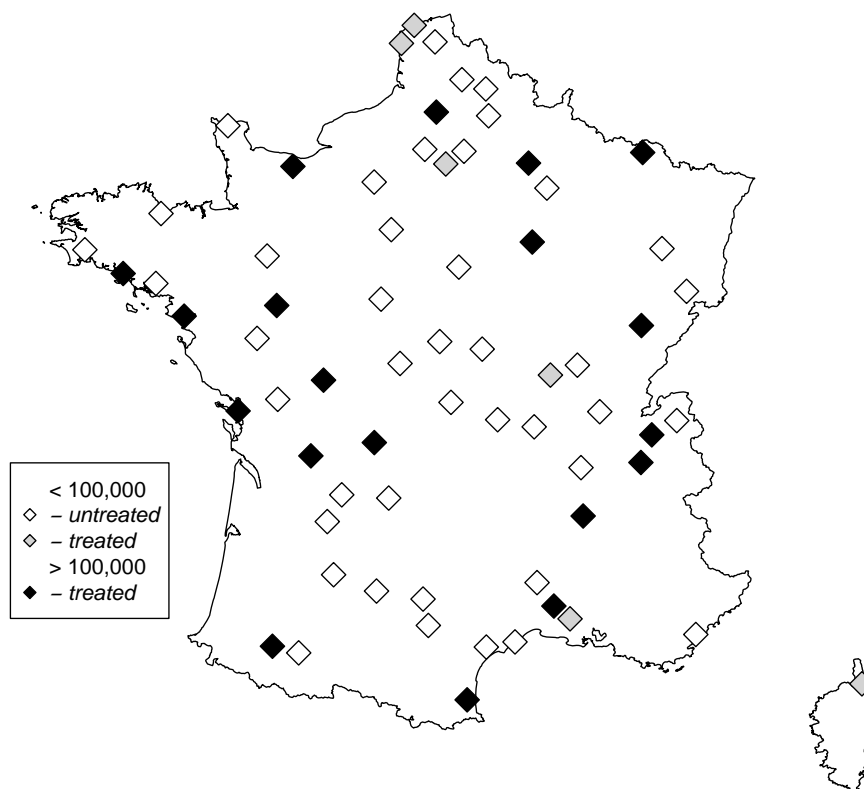
$$\begin{cases} T &= \eta \underline{P} + \gamma X + \nu \\ R &= \delta T + \beta X + \epsilon \end{cases}$$

³For example, Annemasse (Haute-Savoie) is part of the metropolitan area of Geneva.

⁴Results are robust when regressing the total rent.

The threshold of 100,000 inhabitants is relative to agglomeration size. As in our data observations are dwellings, residuals are clustered by agglomeration to take into account spatial autocorrelation of rents.

Figure 5.4: Agglomerations used for estimations



5.3.2 Sample selection

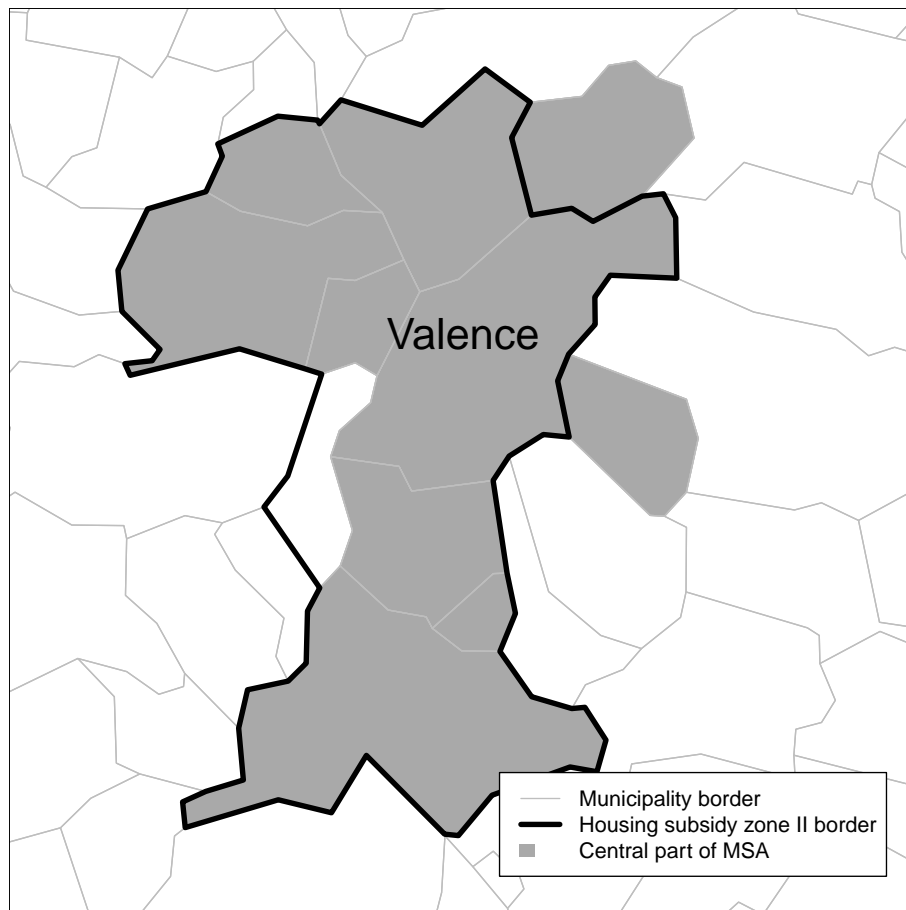
The simplest way to compute the estimation would be to compare all dwellings located in agglomerations inside our window. This solution is inadequate, because the treatment is not homogenous within an agglomeration. While, in treated agglomerations, the central part is classified in zone II, the outskirts are classified in zone III and the subsidies are the same than in untreated agglomerations. Thus, comparing the whole agglomerations would not provide the treatment effect.

To our knowledge, the delineation of targeted areas refers neither to existing administrative nor to statistical zoning. Thus, we observe the exact border of the central part of the agglomeration only for the treatment group. We need to assess what this central zone would have been in the control group to compare similar treated and untreated municipalities and to provide unbiased estimates.

The French National Institute of Statistics and Economics Studies (INSEE) provides a delineation of agglomeration called *urban areas* (“*aires urbaines*”) that are

similar to the metropolitan statistical areas in the US. These *urban areas* are divided into a central part and a peripheral part. We notice that the central part of *urban areas* often coincides with the zone II of housing subsidy. In fact, in treated agglomerations, the central part of the *urban areas* correctly predicts the treatment assignment for 96% of dwellings of our sample.⁵ Figure 5.5 provides an example for the Valence agglomeration. Consequently, we use the central part of the *urban areas* as defined by INSEE in 2010 for the central zone in the control group.⁶ All population variables at the agglomeration level, including the 100,000 inhabitants threshold, are computed according to this zoning.

Figure 5.5: Coincidence of the central part of *urban areas* with the zone II of housing subsidy: the example for Valence agglomeration



⁵In our data, in treated agglomerations, 89% of dwellings are located in both the central treatment zone and the central part of the *urban areas*; 7% of dwellings are located outside the two groups; 3% of dwellings are located in the central part of the *urban areas* but are not treated; 1% are treated but located in the outskirts of the *urban areas*.

⁶In treated agglomerations, we use the central part of the agglomeration as defined by housing policy makers (i.e., the part of the agglomeration where housing subsidies are higher).

5.4 Data

Given that we estimate our model on a small sample of agglomerations, we use two different surveys to obtain a sufficient number of observations. First, we use the Rents and Charges survey between 1987 and 2012.⁷ About 5,000 households are questioned during five consecutive quarters and answer about their dwelling features, their renting conditions and the amount of their rents and charges. We also use the Housing survey from 1984 to 2006.⁸ Each four to six years, more than 40,000 households are interviewed accurately about their own characteristics and the characteristics of their housing. Given that we use two different datasets, we do not include survey weights in our regressions. However, we do not find significant differences between weighted and unweighted estimation using only the Rents and Charges survey. To measure the dwelling quality, we use the following variables provided by both surveys: the area, the number of rooms, the number of dwellings in the building and the presence of a bathroom, toilets, a bath, a garden, a balcony, a garage or safety device (alarms, reinforced doors).

The characteristics of the housing market at the municipality level are given by the population Census between 1982 and 2011:⁹ the tenancy status, the type of dwelling (house or apartment), the number of rooms and the presence of a bathroom.

These data are supplemented with other variables relative to municipalities: the zoning for housing subsidies, the dispersion of income in the municipality, the agglomeration population in 1975 and the population trends between 1975 and 2009, the share of open space in the land cover, as a proxy for natural amenities, and the average housing price. The aim of including those geographic control variables is to fully take into account the differences in local housing markets.

5.5 Results

5.5.1 Impact of housing subsidy zoning on rents

Housing subsidy zoning has a significant and positive impact on rents in the private sector.¹⁰ Location in zone II, where housing subsidies are higher, significantly increases the level of rents (see Table 5.4).

Adding variables that control for the dwelling quality reduces this impact, from 9% to 6%, suggesting a positive link between the location in zone II and the housing quality. It might mean that a part of the gap in rents between zone II and III is used to increase the quality of the dwelling or that there are preexisting differences in the housing characteristics between the two zones. However, taking the housing

⁷In France, there is no comprehensive recording of rents (contrary to dwellings sales, which are recorded by solicitors). Except for Paris region, available sources are heterogeneous. Harmonization of data collection is ongoing in order to enable some rent control. This comprehensive and homogenous data set will be available only in a few years.

⁸More precisely, the 1984, 1988, 1992, 1996, 2002 and 2006 Housing Surveys.

⁹More precisely, the 1982, 1990, 1999, 2006 and 2011 Census. It is not possible to include previous census releases as social and private rental sectors are not distinguished before 1982.

¹⁰Many characteristics of municipalities are added as control variables; the regressions here presented include the significant ones only.

characteristics separately, we do not find any significant impact of location in zone II on the dwelling quality (section 5.5.3). Finally, the effect of the treatment drops from 6% to 4% when we include geographic control variables. It is probably the sign that the instrumental method does not fully control for disparities between zones, which is expected when comparing cities of different size. Finally, the estimate is almost stable when we add the average housing price variable, this means that the model seems to appropriately take into account the geographic features that could influence housing market at the municipality level.

The impact of the housing subsidy zoning on rents is of important magnitude, as location in zone II increases the rents by 4%. Given that the average rent is 475 euros, it means the zoning increases the rent by 20 euros. This impact had already been evidenced in the context of a rapid increase in the total amount of aid during the 1990s (Fack, 2006, Laferrère and Blanc, 2004). Our results show that it holds in the long run (between 2005 and 2012), suggesting that housing supply remains quite inelastic.

The instrumental variable method relies on a first stage equation, which explains the treatment (being located in zone II for housing subsidies) with respect to the location in an agglomeration of more than 100,000 inhabitants. The threshold of 100,000 inhabitants significantly explains the treatment (Table 5.11 in Appendix 5.B); indeed, it is the main predictor for location in zone II for housing subsidies. Besides, the F-test of joint nullity of coefficients in this first step equals 43, which guarantees that the threshold of 100,000 inhabitants is not a weak instrument.

5.5.2 Treatment heterogeneity

The impact of location in zone II on rents is heterogeneous, depending on the housing characteristics (Table 5.5). When restricting the sample to the dwellings with two rooms or less, this impact is higher (5%); it is smaller and less significant (3%) when estimated for the dwellings with three rooms or more. Besides, the treatment is significant for flats, while it is not for home. These findings suggest that the increase in rents caused by housing subsidies is stronger on the segments of the housing market that are dedicated to low-income households.

Table 5.6 confirms this partial segmentation of the housing market. We find that the raise in rents is stronger and more significant among housing subsidy recipients (5%) than among households who do not receive the allowance (3%). This result is important for two reasons. First, this backs our identification strategy because it is consistent with the idea that landlords increase rents charged to tenants, provided that they receive housing subsidies. Second, housing subsidies also impact the rents paid by unsubsidized households, which is in line with the finding of Susin (2002) in the United-States. Indeed, the French private rental sector is quite competitive and the rent could be set without legal constraint at the tenant's arrival, until 2012. However, the annual rent increase is controlled, once the tenant moved in. Substantial rises in rents should thus occur at the start of the tenancy. Considering that more of 40 percent of private sector tenants are subsidized (see figure 5.2), a landlord looking for a tenant is likely to receive applications from subsidized households. Thus, he might demand for a rent taking into account the level of the housing

Table 5.4: Effect of housing subsidy zoning on rents

Intercept	2.035*** (0.0449)	4.665*** (0.0771)	2.287** (1.153)	2.279** (1.082)
Zone II for housing subsidies	0.0934** (0.0428)	0.0609** (0.0271)	0.0421** (0.0164)	0.0404*** (0.0156)
log(size)		-0.676*** (0.0239)	-0.657*** (0.0216)	-0.654*** (0.0216)
Number of rooms		0.0561*** (0.00943)	0.0521*** (0.00950)	0.0510*** (0.00941)
Length of the tenancy		-0.0122*** (0.00110)	-0.0121*** (0.00108)	-0.0122*** (0.00109)
Completion year < 1914		-0.131*** (0.0265)	-0.0972*** (0.0269)	-0.0980*** (0.0266)
Completion year 1915-1948		-0.134*** (0.0209)	-0.101*** (0.0214)	-0.101*** (0.0212)
Completion year 1949-1967		-0.133*** (0.0168)	-0.106*** (0.0169)	-0.105*** (0.0167)
Completion year 1968-1990		-0.0969*** (0.0162)	-0.0803*** (0.0169)	-0.0821*** (0.0167)
Completion year > 1990		ref.	ref.	ref.
Bathroom		0.0482* (0.0258)	0.0490* (0.0252)	0.0510** (0.0252)
Bath		0.0696*** (0.0174)	0.0597*** (0.0161)	0.0609*** (0.0160)
Garden		0.0568*** (0.0142)	0.0563*** (0.0155)	0.0597*** (0.0155)
Balcony		0.0377*** (0.0131)	0.0323*** (0.0113)	0.0289*** (0.0111)
Home security device		0.0294** (0.0135)	0.0274* (0.0140)	0.0274** (0.0136)
House		0.0453** (0.0180)	0.0371** (0.0162)	0.0354** (0.0164)
Share of open space ₂₀₀₀			0.182*** (0.0590)	0.151*** (0.0582)
Share of rental housing ₁₉₉₉			-0.402*** (0.0943)	-0.403*** (0.0943)
log(population density ₁₉₉₉)			0.0252 (0.0191)	0.0260 (0.0188)
log(median city income ₂₀₀₁)			0.199* (0.109)	0.177* (0.101)
IQR city income ₂₀₀₁			0.114*** (0.0312)	0.103*** (0.0287)
Δ MSA pop ₁₉₇₅₋₉₉			0.451*** (0.0716)	0.416*** (0.0727)
Average housing price ₂₀₀₄				0.0343*** (0.0117)
Observations	2159	2159	2159	2159

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes privately rented dwellings located agglomeration in with a population between 50,000 and 180,000 inhabitants. The time period extends from 2005 to 2012. IQR stands for interquartile range.

Sources: Rents and Charges survey, Housing survey.

Table 5.5: Effect of housing subsidy zoning on rents – Treatment heterogeneity depending on housing type

Intercept	2.467** (1.230)	2.425 (1.770)	1.901 (1.391)	1.972 (1.600)
Zone II for housing subsidies	0.0327* (0.0187)	0.0540*** (0.0186)	0.0479*** (0.0166)	0.0161 (0.0290)
Housing characteristics	✓	✓	✓	✓
Share of open space ₂₀₀₀	0.187*** (0.0609)	0.204** (0.0832)	0.192*** (0.0549)	0.109 (0.0897)
Share of rental housing ₁₉₉₉	−0.266** (0.121)	−0.596*** (0.136)	−0.628*** (0.102)	0.130 (0.199)
log(population density ₁₉₉₉)	0.0332* (0.0172)	0.0155 (0.0241)	0.0374* (0.0197)	−0.00875 (0.0141)
log(median city income ₂₀₀₁)	0.175 (0.117)	0.204 (0.173)	0.239* (0.133)	0.246 (0.157)
IQR city income ₂₀₀₁	0.105*** (0.0361)	0.107** (0.0465)	0.116*** (0.0363)	0.146** (0.0681)
Δ MSA pop _{1975–99}	0.468*** (0.0702)	0.452*** (0.100)	0.483*** (0.0756)	0.260** (0.107)
Observations	1260	899	1658	501
Subsample	More than 3 rooms	Less than 2 rooms	House	Flat

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes privately rented dwellings located agglomeration in with a population between 50,000 and 180,000 inhabitants. The time period extends from 2005 to 2012.

Sources: Rents and Charges survey, Housing survey.

subsidy, before knowing whether the tenant benefits from housing allowance. All in all, this policy increases the willingness to pay of a large part of tenants and might consequently increase the equilibrium rent of all dwellings, including those that are not occupied by subsidy recipients.

We also find that the impact of housing allowance is only significant in fast growing cities, where the population growth between 1975 and 1999 exceeds 5%. We interpret this result as an additional evidence of the link between housing supply elasticity and the upward impact of housing subsidies. Indeed, housing market should be tighter when population grows because the housing supply adjusts slowly. It implies that landlords are in a stronger position to impose increases of rents. Similarly, for the United States, Hilber and Turner (2013) show that mortgage interest deduction raises the share of home-owners where land use regulation is lax while it is capitalized in housing prices where land use regulation is strict.

Table 5.6: Effect of housing subsidy zoning on rents – Treatment heterogeneity depending on housing market characteristics

Intercept	3.765** (1.563)	1.720 (1.435)	3.237*** (1.156)	0.965 (1.556)
Zone II for housing subsidies	0.0174 (0.0294)	0.0690*** (0.0193)	0.0497*** (0.0163)	0.0330* (0.0195)
Housing characteristics	✓	✓	✓	✓
Share of open space ₂₀₀₀	0.141 (0.0967)	0.235*** (0.0477)	0.250*** (0.0583)	0.0689 (0.0760)
Share of rental housing ₁₉₉₉	−0.324*** (0.121)	−0.367** (0.150)	−0.317*** (0.112)	−0.494*** (0.127)
log(population density ₁₉₉₉)	0.00809 (0.0187)	0.0415*** (0.0116)	0.0255* (0.0137)	0.0186 (0.0267)
log(median city income ₂₀₀₁)	0.0620 (0.147)	0.236* (0.143)	0.116 (0.113)	0.257* (0.147)
IQR city income ₂₀₀₁	0.169*** (0.0624)	0.0710** (0.0346)	0.0772** (0.0350)	0.153*** (0.0407)
Δ MSA pop _{1975–99}	0.679*** (0.240)	0.498*** (0.0892)	0.422*** (0.0773)	0.528*** (0.0992)
Observations	920	1239	1258	901
Housing subsidy recipient	All	All	Yes	No
MSA pop. growth 1975–1999	<5%	≥5%	All	All

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes privately rented dwellings located agglomeration in with a population between 50,000 and 180,000 inhabitants. The time period extends from 2005 to 2012.

Sources: Rents and Charges survey, Housing survey.

5.5.3 Almost no significant impact on housing quality or quantity

A demand subsidy should lead not only to an increase in rents but also to an increase in the quality of dwellings or in the number of rental dwellings, unless housing supply is fully inelastic. Results show that location in zone II, where housing subsidies are higher, has no impact on housing quality, as measured by some intrinsic characteristics of the dwelling (number of housing in the building, number of rooms, presence of a bathroom, and the size) (Table 5.7). However, these characteristics cannot be easily improved by the landlord.¹¹

Table 5.7: Effect of housing subsidy zoning on housing quality

	Number of housing in the building	Number of rooms	Presence of a bathroom	Living area
Intercept	1183.4 (3046.5)	13.31*** (4.959)	1.120 (0.821)	9.268*** (2.125)
Zone II for housing subsidies	11.92 (28.63)	-0.0669 (0.0681)	0.0123 (0.0119)	-0.0145 (0.0321)
Δ MSA pop ₁₉₇₅₋₉₉	186.4 (165.7)	0.104 (0.249)	-0.0385 (0.0461)	-0.0536 (0.120)
Share of open space ₂₀₀₀	-93.89 (60.96)	0.0740 (0.210)	-0.117** (0.0481)	0.0319 (0.0857)
Share of rental housing ₁₉₉₉	-80.20 (167.6)	-1.955*** (0.572)	-0.0680 (0.0951)	-0.330 (0.269)
log(population density ₁₉₉₉)	18.52* (10.15)	-0.0754 (0.0574)	0.00389 (0.00769)	-0.0698*** (0.0247)
log(median city income ₂₀₀₁)	-116.9 (307.6)	-0.955** (0.484)	-0.0165 (0.0806)	-0.459** (0.206)
IQR city income ₂₀₀₁	-67.90 (67.12)	-0.154 (0.152)	0.0237 (0.0319)	-0.0992 (0.0724)
Observations	2159	2159	2159	2159

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The sample includes privately rented dwellings located agglomeration in with a population between 50,000 and 180,000 inhabitants. The time period extends from 2005 to 2012.

Sources: Rents and Charges survey, Housing survey.

Similarly, when using data at the municipality level, results show that the housing subsidy zoning has no impact on the proportion of rental housing in the total housing stock (Table 5.8).¹² This result holds when distinguishing between fast- and slow-growing agglomerations. These two findings suggest that housing supply remains inelastic in the long run. They also confirm that the dwellings below and above the threshold are indeed comparable, which validates our approach.

We do not find either any significant treatment impact on the share of privately-rented home or furnished rentals (see Table 5.9). However, we highlight an increase

¹¹Contrary to other proxies for quality, such as the painting or the presence of a fully fitted kitchen, such housing characteristics are not present in the rent and charges survey. The sampling design of the housing survey does not allow to use only this dataset for the estimation.

¹²These results hold when considering the variation of the number of rentals.

Table 5.8: Effect of housing subsidy zoning on the private rental sector in 2011 at the municipality level

Intercept	−0.419 (0.460)	−1.882** (0.770)	0.476 (0.444)
Zone II for housing subsidies	0.00320 (0.0143)	−0.0156 (0.0206)	0.0165 (0.0154)
Share of open space ₂₀₀₆	0.00139 (0.0254)	−0.0514 (0.0388)	0.0315 (0.0308)
log(population density ₂₀₁₁)	0.00980** (0.00479)	0.00443 (0.00727)	0.0127* (0.00687)
log(median city income ₂₀₁₁)	0.0322 (0.0435)	0.176** (0.0727)	−0.0531 (0.0406)
IQR city income ₂₀₁₁	0.0378 (0.0236)	0.0876** (0.0375)	−0.00497 (0.0242)
Δ MSA pop _{1990–2011}	0.222*** (0.0566)	0.323** (0.148)	0.198** (0.0769)
Average housing price ₂₀₁₀	0.00255 (0.00178)	0.00702* (0.00415)	0.00108 (0.00164)
Zone A for landlord subsidies	−0.0633*** (0.0196)		−0.0590*** (0.0196)
Zone B1 for landlord subsidies	−0.00744 (0.0220)		−0.00537 (0.0249)
Zone B2 for landlord subsidies	0.0177 (0.0113)	−0.0179 (0.0141)	0.0160 (0.0151)
% rental housing ₁₉₈₂	0.574*** (0.0642)	0.604*** (0.0756)	0.637*** (0.0647)
Observations	310	116	194
MSA pop. growth 1975-1999	All	<5%	≥5%

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the share of rental housing in the total housing stock in 2011. The sample includes municipalities located agglomeration in with a population between 50,000 and 180,000 inhabitants.

Sources: Population Census.

in the share of a one-room rentals in the privately rented housing stock. It suggests that local housing markets in zone II experienced a demand shift toward one-room apartments. Laferrère and le Blanc (2004) show that the extension of housing allowance to student in the 1990s, regardless of their parents' income, allowed some of them to move out of the parental home. Our finding could be due to the entry of a greater number of students in housing markets belonging to zone II.

5.5.4 Robustness checks

Results are provided for a sample including dwellings located in agglomerations between 50,000 and 180,000 inhabitants and rented between 2005 and 2012 (Tables 5.4 to 5.8): this sample includes 2,159 dwellings located in 63 agglomerations. Results are robust when using different windows or different study periods (see Table 5.13 in the appendix). A wider window of 30,000-250,000 inhabitants (4,081 dwellings located in 101 agglomerations) provides a slightly larger and positive impact of lo-

Table 5.9: Effect of housing subsidy zoning on the private rental sector in 2011 at the municipality level – Treatment heterogeneity depending on housing type

Dependent variables	Proportion in the privately rented housing stock in 2011				
	rental home	furnished rentals	1 room rentals	2-4 rooms rentals	≥5 rooms rentals
Intercept	1.243 (0.819)	−0.499 (0.399)	−1.154*** (0.309)	1.437 (1.001)	0.369 (0.587)
Zone II for housing subsidies	−0.0156 (0.0209)	0.00991 (0.00885)	0.0216** (0.00863)	0.00541 (0.0259)	−0.00835 (0.00930)
Share of open space ₂₀₀₆	−0.101*** (0.0373)	0.0292 (0.0195)	0.0689*** (0.0206)	−0.111** (0.0433)	0.00520 (0.0246)
log(population density ₂₀₁₁)	−0.0290** (0.0121)	0.00501 (0.00366)	0.0248*** (0.00452)	−0.00349 (0.0115)	−0.00270 (0.00589)
log(median city income ₂₀₁₁)	−0.0623 (0.0763)	0.0411 (0.0380)	0.0881*** (0.0301)	−0.102 (0.0902)	−0.00697 (0.0569)
IQR city income ₂₀₁₁	0.0000243 (0.0397)	0.0120 (0.0208)	0.0305* (0.0171)	0.0129 (0.0414)	−0.00784 (0.0241)
Δ MSA pop _{1990–2011}	−0.191** (0.0902)	−0.0384 (0.0430)	−0.0524 (0.0501)	−0.190* (0.108)	−0.113** (0.0567)
Average housing price ₂₀₁₀	−0.00505 (0.00720)	0.00163 (0.00112)	−0.000642 (0.00180)	0.00292 (0.00459)	0.0000291 (0.00270)
Zone A for landlord subsidies	0.0369 (0.0338)	0.0411*** (0.00859)	0.0502*** (0.0120)	0.214*** (0.0449)	−0.0613*** (0.0193)
Zone B1 for landlord subsidies	−0.0139 (0.0403)	0.0610*** (0.00965)	0.0103 (0.0175)	0.0277 (0.0504)	−0.0581*** (0.0208)
Zone B2 for landlord subsidies	−0.0290 (0.0254)	0.0115* (0.00606)	0.0105 (0.00976)	−0.0362 (0.0407)	−0.0354* (0.0197)
% rental home ₁₉₈₂	0.459*** (0.0457)				
% furnished rentals ₁₉₈₂		0.0728 (0.0771)			
% 1 room rentals ₁₉₈₂			0.332*** (0.0681)		
% 2-4 rooms rentals ₁₉₈₂				0.302* (0.167)	
% ≥5 rooms rentals ₁₉₈₂					0.402*** (0.0607)
Observations	310	310	310	310	310

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The sample includes municipalities located agglomeration in with a population between 50,000 and 180,000 inhabitants.

Sources: Population Census.

cation in zone II on rents (5%). Reducing the window to 75,000-140,000 gives a less significant impact, but very similar (4%), as this new sample contains 888 dwellings located in 21 agglomerations only. The two estimations validate our identification strategy as the estimated impact is very stable and does not consequently depend on the population size of the agglomeration on either side of the discontinuity.

Performing the estimation on previous periods (1987-1991 or 1992-2004, instead of 2005-2012) confirms the impact of location in zone II on rents. Indeed, before the rapid increase of housing subsidies in the early 1990s (see figures 5.1 and 5.2), the effect of the treatment is found to be positive (3%) but not significant. This result allows to reject the hypothesis that the impact we find is only due to the preexisting differences between zone II and III housing markets. Besides, the impact of higher housing subsidies is significant in the 1992-2004 period and the treatment coefficient is higher than during the subsequent period (7%). This larger effect could reveal a small adjustment of housing supply in the 2000s, after the period of the rapid increase in housing subsidies in the 1990s. This could also be explained by a reduction of the share of housing subsidies in the rents in the 2000s (see figure 5.2).¹³

When considering social housing only, the interpretation of the impact is difficult, because the housing subsidy zoning matches with the rent threshold zoning (Table 5.14). Thus, the impacts of the two different zonings cannot be disentangled: the impact of the location in zone II on rents may correspond to the ability for the lessor to fix a higher rent. Results are robust when adding the zoning for landlord subsidies, meaning that this housing policy does not explain the increase of rents.

Placebo tests do not reveal any unexpected impact (Table 5.15). Other discontinuities at 50,000 or 200,000 inhabitants are non significant and point estimates are close to zero. When restricting the sample to the untreated agglomerations of less than 100,000 inhabitants, the threshold of 50,000 has a non significant impact on rents. Similarly, when restricting the sample to the treated agglomerations of more than 100,000 inhabitants, the threshold of 200,000 is non significant. Reassuringly, the point estimate of the 100,000-inhabitant dummy variable is very similar to the treatment effect. Finally, the population trend (here the log of the agglomeration size) is non significant but *negative*, which suggest a collinearity issue with the 100,000 inhabitant threshold. All these four tests confirm that the estimated treatment effect is not due to a non modeled population trend effect.

5.6 Conclusion

We measure the impact of housing subsidies on the private rental sector. To do so, we use an instrumental variable method based on a spatial discontinuity in the subsidy scheme. We show that housing subsidies has a positive impact on rents. This impact is stronger for the low-income households and the dwellings with two rooms or less. Besides, higher subsidies seem to have almost no effect on housing quality and to have no impact on the number of offered rental dwellings, except on the share of one-room dwellings in the privately rented housing stock.

¹³This is due to the fact that the annual allowance revaluation is indexed to inflation, which was lower than rent growth.

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5.A Comparison of housing subsidies zones crossed with the landlord subsidies zones

Table 5.10: Number of dwellings in the housing subsidies zones (zones II and III) crossed with the landlord subsidies zones

	Zone II	Zone III
Landlord sub. zone A	0	43,252
Landlord sub. zone B1	177,584	55,784
Landlord sub. zone B2	1,208,007	1,201,865
Landlord sub. zone C	29,297	90,245

Source: Population Census.

5.B Supplementary estimates

Table 5.11: First stage of the instrumental variable method – regression on dwellings

Intercept	16.33*** (4.735)
1 (MSA pop < 100000)	0.917*** (0.0479)
log(size)	-0.0106 (0.0190)
Number of rooms	0.00110 (0.00603)
Length of the tenancy	-0.000199 (0.00116)
Completion year < 1914	0.00353 (0.0262)
Completion year 1915-1948	-0.0308 (0.0248)
Completion year 1949-1967	0.0325 (0.0232)
Completion year 1968-1990	0.00853 (0.0204)
Completion year > 1990	ref.
Bathroom	-0.0292 (0.0221)
Bath	0.00888 (0.0168)
Garden	-0.0464* (0.0260)
Balcony	0.0215 (0.0241)
Home security device	-0.0247 (0.0185)
House	0.0355* (0.0210)
Share of open space ₂₀₀₀	0.311 (0.294)
Share of rental housing ₁₉₉₉	-0.548 (0.371)
log(population density ₁₉₉₉)	-0.0518 (0.0654)
log(median city income ₂₀₀₁)	-1.630*** (0.472)
IQR city income ₂₀₀₁	-0.106 (0.106)
Δ MSA pop ₁₉₇₅₋₉₉	0.190 (0.239)
Observations	2159

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%.

Sources: Rents and Charges survey, Housing survey.

Table 5.12: First stage of the instrumental variable method – regression on municipalities

Intercept	1.291 (1.256)
1 (MSA pop < 100000)	0.904*** (0.0475)
Share of open space ₂₀₀₆	0.0490 (0.167)
log(population density ₂₀₁₁)	-0.0305 (0.0400)
log(median city income ₂₀₁₁)	-0.144 (0.116)
IQR city income ₂₀₁₁	0.256** (0.120)
Δ MSA pop _{1990–2011}	-0.163 (0.427)
Observations	401

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%.
Sources: Population Census by INSEE.

Table 5.13: Effect of housing subsidy zoning on rents – Robustness checks: window and study period

Intercept	1.193 (1.362)	1.473 (0.954)	−5.913 (4.042)	−1.884 (1.274)
Zone II for housing subsidies	0.0413* (0.0234)	0.0500*** (0.0156)	0.0302 (0.0395)	0.0723*** (0.0236)
Housing characteristics	✓	✓	✓	✓
Share of open space ₂₀₀₀	0.0926 (0.0664)	0.208*** (0.0526)		
Share of rental housing ₁₉₉₉	−0.181 (0.159)	−0.223** (0.101)		
log(population density ₁₉₉₉)	0.0317** (0.0142)	0.0335** (0.0147)		
log(median city income ₂₀₀₁)	0.259* (0.142)	0.271*** (0.0933)	0.953** (0.396)	0.627*** (0.126)
IQR city income ₂₀₀₁	0.140*** (0.0333)	0.0967*** (0.0300)	0.332*** (0.121)	0.237*** (0.0436)
Δ MSA pop _{1975–99}	0.746*** (0.124)	0.452*** (0.0646)		
Share of open space ₁₉₉₀			−0.130 (0.133)	−0.0206 (0.0688)
Share of rental housing ₁₉₉₀			−0.0157 (0.202)	
log(population density ₁₉₉₀)			0.0152 (0.0255)	
Δ MSA pop _{1968–90}			0.578*** (0.174)	
Share of rental housing ₁₉₈₂				−0.259** (0.132)
log(population density ₁₉₈₂)				0.00451 (0.0160)
Δ MSA pop _{1962–82}				0.201** (0.0806)
Observations	888	4081	1098	3256
Period	2005-2012	2005-2012	1984-1991	1992-2004
Agglomeration population	75,000- 140,000	30,000- 250,000	50,000- 180,000	50,000- 180,000

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using the IV method and year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes only privately rented dwellings.

Sources: Rents and Charges survey, Housing Survey.

Table 5.14: Effect of housing subsidy zoning on rents. Robustness checks: OLS, social housing, and other housing policies

Intercept	2.105* (1.168)	2.870** (1.375)	1.160 (1.108)
Zone II for housing subsidies	0.0565*** (0.0174)	0.0349** (0.0170)	0.0455*** (0.0147)
Housing characteristics	✓	✓	✓
Share of open space ₂₀₀₀	0.177*** (0.0597)	0.00974 (0.0566)	0.0976* (0.0541)
Share of rental housing ₁₉₉₉	-0.391*** (0.0995)	-0.267** (0.117)	-0.424*** (0.0886)
log(population density ₁₉₉₉)	0.0244 (0.0190)	0.0145 (0.0100)	0.0221 (0.0186)
log(median city income ₂₀₀₁)	0.203* (0.112)	0.188 (0.136)	0.323*** (0.104)
IQR city income ₂₀₀₁	0.109*** (0.0332)	0.0331 (0.0453)	0.135*** (0.0280)
Δ MSA pop ₁₉₇₅₋₉₉	0.449*** (0.0735)	0.0157 (0.0847)	0.311*** (0.0650)
Zone A for landlord subsidies			0.228*** (0.0329)
Zone B1 for landlord subsidies			0.0757** (0.0362)
Zone B2 for landlord subsidies			-0.0248 (0.0235)
Observations	2159	2686	2159
Estimator	OLS	IV	IV
Sample	private sec.	social housing	private sec.

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes dwellings located agglomeration in with a population between 50,000 and 180,000 inhabitants. The time period extends from 2005 to 2012.

Sources: Rents and Charges survey, Housing Survey.

Table 5.15: Effect of housing subsidy zoning on rents – Placebo tests

Intercept	1.585 (1.564)	2.799** (1.188)	1.000 (0.766)	1.636* (0.980)
Housing characteristics	✓	✓	✓	✓
Share of open space ₂₀₀₀	0.133*** (0.0506)	0.195*** (0.0643)	0.128** (0.0546)	0.142** (0.0600)
Share of rental housing ₁₉₉₉	-0.468*** (0.120)	-0.425*** (0.0941)	-0.224 (0.140)	-0.383*** (0.0988)
log(population density ₁₉₉₉)	0.0541*** (0.0106)	0.0231 (0.0220)	0.0511*** (0.0129)	0.0334** (0.0138)
log(median city income ₂₀₀₁)	0.241 (0.153)	0.131 (0.111)	0.309*** (0.0761)	0.376*** (0.113)
IQR city income ₂₀₀₁	0.136*** (0.0487)	0.110*** (0.0335)	0.0516 (0.0315)	0.145*** (0.0311)
Δ MSA pop _{1975–99}	0.444*** (0.0545)	0.459*** (0.0725)	0.534*** (0.0943)	0.353*** (0.0836)
1 (MSA pop < 50000)	0.00802 (0.0175)			
1 (MSA pop < 100000)		0.0386** (0.0152)		
1 (MSA pop < 200000)			0.00324 (0.0166)	
Zone II for housing subsidies				0.115*** (0.0283)
log(MSA pop ₁₉₇₅)				-0.105*** (0.0402)
Observations	1953	2159	2646	2159
Estimator	OLS	OLS	OLS	IV
Subsample	private sec. untreated	private sec.	private sec. treated	private sec.
Agglomeration population	30,000- 100,000	50,000- 180,000	100,000- 300,000	50,000- 180,000

Notes: standard errors are in parentheses; significance levels: *** 1%, ** 5%, * 10%. All regressions are run using year fixed effects; standard errors are clustered by agglomeration. The dependent variable is the logarithm of the rent per square meter. The sample includes privately rented dwellings. The time period extends from 2005 to 2012.

Sources: Rents and Charges survey, Housing Survey.