CAHIER D'ÉTUDES
WORKING PAPER

N° 167

LOCAL EMPLOYMENT DYNAMICS AND COMMUTING COSTS

JULIEN PASCAL

OCTOBER 2022

BANQUE CENTRALE DU LUXEMBOURG
EUROSYSTÈME

LOCAL EMPLOYMENT DYNAMICS AND COMMUTING COSTS

JULIEN PASCAL

Abstract. I explore the links between commuting costs and local employment dynamics using a spatial discontinuity introduced by a French reform in September 2015. The reform decreased the cost of public transportation in selected areas of the Paris region, but did not affect other areas. In the baseline regression framework, which only includes units that are geographically close to each other, I find that areas benefiting from the reform experienced a 0.25 percentage point decline in the unemployment rate, a 0.60 percentage point increase in the share of employed workers commuting using public transport, and a 1.4% increase in the price of residential real estate. I extend the regression framework to take into account the heterogeneity of treatment introduced by the reform, which allows me to analyze the mechanisms driving the results. I also show that a calibrated spatial search-and-matching model can rationalize the estimated treatment effects.

JEL Codes: E24, J68, R13, R23.

Keywords: Local employment, Commuting Costs, Policy, Search-and-Matching.

September 2022. Julien Pascal: Banque centrale du Luxembourg, Département Économie et Recherche, 2 boulevard Royal, L-2983 Luxembourg (julien.pascal@bcl.lu). This paper is based on the second chapter of my PhD thesis, that was defended in June 2020 at the Institut de Sciences Politiques, Paris (Pascal (2020)). I would like to thank Pablo Garcia-Sanchez, Luca Marchiori, Alban Moura, Olivier Pierrard, Paolo Guarda, as well as several BCL colleagues for valuable comments and suggestions.

1. Résumé non technique

En mars 2020, le Luxembourg est devenu le premier pays au monde à rendre gratuit l'usage des transports publics sur l'ensemble de son territoire. Cette réforme a pu avoir des conséquences sur la décision des travailleurs d'accepter une offre d'embauche, sur la décision des entreprises de créer des emplois, et donc sur la situation de l'emploi en général. Selon les modèles d'économie urbaine, les travailleurs demandent à être compensés pour leurs coûts de transport, ce qui pourrait donc se refléter dans les salaires et dans les décisions d'embauche des entreprises. D'autre part, la baisse du coût d'accès aux transports en commun pourrait inciter les travailleurs à choisir un autre mode de transport (voiture ou transports en commun), avec des conséquences sur les niveaux de pollution et de congestion du réseau routier.

Cette étude offre un nouvel éclairage sur ces questions en utilisant une réforme de la tarification des transports en commun pour la région Île-de-France. Avant septembre 2015, le prix du transport en commun dépendait de manière non linéaire de la distance au centre de Paris. La zone tarifaire centrale (Paris et ses communes limitrophes) bénéficiait du prix minimum. Les usagers venant des autres zones tarifaires, formant des zones concentriques autour de la zone tarifaire centrale, devaient payer des prix plus élevés. La réforme "Forfait Toutes Zones" de septembre 2015 a introduit un prix unique, aligné sur le prix de la zone centrale. Ce scénario permet de comparer l'évolution d'un groupe de contrôle pour lequel le prix n'a pas changé (la zone tarifaire centrale), à celle du groupe de traitement qui a bénéficié d'une baisse du prix (les zones tarifaires éloignées du centre).

L'analyse économétrique met en exergue trois résultats. Premièrement, la baisse du prix des transports en commun se traduit par une hausse de l'usage de ceux-ci pour les travailleurs vivant dans les zones bénéficiant de la réforme. Deuxièmement, l'augmentation de l'usage des transports en commun va de pair avec une diminution du chômage dans ces zones. Troisièmement, on assiste à une augmentation du prix de l'immobilier dans les zones bénéficiant de la réforme. L'approche empirique est complémentée par un modèle spatial du marché du travail, qui est calibré pour reproduire certaines statistiques de la région Île-de-France. Le modèle est capable de bien approximer la baisse du chômage et l'augmentation du prix de l'immobilier observées dans l'analyse empirique. Ce cadre analytique pourra être adapté pour des analyses

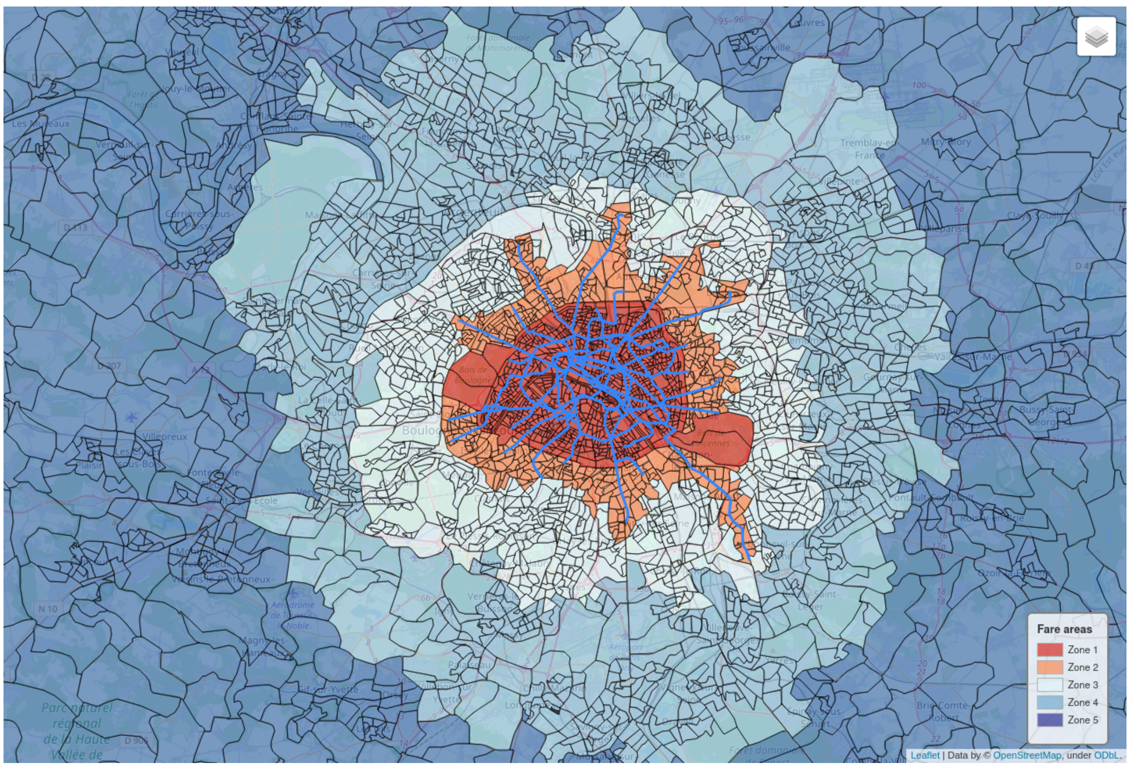
futures de l'impact de réformes au Luxembourg. L'approche analytique publiée dans le Cahier 159 de la BCL, portant sur les choix de migration des travailleurs frontaliers, pourrait permettre de prendre en considération le rôle clé des travailleurs frontaliers dans l'économie luxembourgeoise.

2. Introduction

The main goal of this paper is to provide new empirical evidence on the links between commuting costs and local employment dynamics, using a spatial discontinuity created by a French reform in September 2015 in the Paris metropolitan area. The idea for this discontinuity is simple. Prior to the reform, those who bought a public transport travel pass would pay different fares depending on the zones they crossed during their commute. Residents of the city center and the inner suburbs would pay a single price to travel in zones 1 and 2, including the city center and bordering municipalities. Those living further from the city center (in zones 3 to 5) had to pay a premium to commute to Paris (see Figure 1). In September 2015, the price of travel passes was de-linked from the zones crossed, with the creation of a single price called "Forfait Toutes Zones" (FTZ). The FTZ offered a substantial price discount for residents of zones 3 to 5, while the price for residents of zones 1 and 2 marginally increased. Hence, the removal of fare zones ("dézonage") created a discontinuity in terms of commuting costs for individuals residing on different sides of a former price border. In the baseline regression framework, I exploit this discontinuity to quantify the link between commuting costs and local employment dynamics, by focusing on municipalities close to the former price border and using a difference-in-difference strategy. When comparing the city center and the inner suburbs, the main empirical findings are that the FTZ reform led to a 0.60 percentage point increase in the share of employed workers commuting using public transport, a 0.25 percentage point decrease in the unemployment rate, and a 1.4% increase in the price of residential real estate.

This paper provides evidence that reforms changing the costs of commuting have consequences on local employment dynamics. My analysis is related to the unemployment/inactivity trap literature, which sheds light on the structural barriers that unemployed workers face when searching for a job. In the French context, Anne and L'Horty (2009) show that the complex system of national and local social transfers creates a situation in which minimum wage workers with children are just better off not working. In this paper, I focus on a spatially-located disincentive to work. For those seeking a job within the city of Paris, where most jobs are located, unemployed workers living in the outskirts of Paris (zones 3-5) used to face *more disincentives to work* than unemployed workers living close to the city center (zones 1-2). The FTZ reform reduced disincentives to work in zones 3, 4 and 5, without altering incentives in zones 1 and 2. Some individuals who would have previously rejected offers requiring a long commute are now more likely to accept them. In short, the FTZ reform can be seen as a spatially-located employment premium.

FIGURE 1. Fare zones



Notes: Author's calculations based on data from https://www.vianavigo.com/accueil. This figure displays the fare zones for a trip to the center of Paris (the metro stop Châtelet-les-Halles). Each subdivision represents an IRIS, which is a geographical administrative unit containing approximately 2000 inhabitants. The blue lines indicate the metro lines.

Related work includes Mayer and Trevien (2017) who analyze the arrival of the Regional Express Rail (RER) in the Paris metropolitan area. For municipalities connected to the network, the commuting time to central Paris decreased by about 10%. The authors find that in the newly connected municipalities this change caused an 8.8% increase in employment and a 4.6% increase in the number of firms. The authors use an instrumental variable strategy to identify the causal effect of the new infrastructure. Using a similar methodology, Garcia-López, Hémet, and Viladecans-Marsal (2017) show that improvements in the Parisian transit system created employment clusters in suburban municipalities that had a rail station. Duranton and Turner (2012), also relying on an instrumental variable approach, show that a 10% increase in a provision of buses caused the population to increase by 0.8% in the US. Other related studies also include Chen and Whalley (2012), who find that the opening of the metro system in Taipei reduced the measured concentration of carbon monoxide by 5 to 15%. Baum-Snow and Kahn (2000) show that investments in public transit projects in five major American cities caused an increase in the local value of properties and encouraged switching from driving to public transport.

Instead of using an instrumental variable, the current paper uses a difference-in-difference estimator in conjunction with a quasi-instrumental variation in explanatory variables, as in the seminal contribution by Card and Krueger (1994). In the present setting, a border creates a sharp discontinuity between people receiving the treatment (lower cost of using public transport) and the non-treated (unchanged cost of using public transport). Because I use data at a fine level of spatial granularity, my analysis is more related to literature on *spatial regression discontinuity*. This literature highlights the fact that there exists a trade-off between the need to compare geographical areas close to each other to control for unobservable characteristics and the identification difficulties created by spillovers between neighboring areas (see Neumark and Simpson (2015) for a review). To address this concern, I use two main regression frameworks. In a first part, I only compare the city center and the inner suburbs, which maximizes the geographical proximity of the treated and the non-treated. In a second part, I include observations further away from the city center, which lessens the threat of spillovers between the treated and untreated units, at the cost of increasing the potential impact of unobservable variables on outcome variables. In this second setting, the diversity among the treated units allows me to further explore the mechanisms behind the FTZ reform. In particular, I find that the estimated treatment effects are stronger for units located closer to metro and train stations. I also find evidence that the positive employment effects for units located close to Paris (zone 3) appear to have been driven by more residents accessing the Parisian labor market, while positive employment effects for residents of more distant suburbs (zones 4 and 5) may have been caused by new local firms, rather than more residents commuting to the center of Paris. Related empirical strategies using fine spatial granularity can be found in Kline and Moretti (2013), Einiö and Overman (2016), Hilber, Carozzi, and Xiaolun (2020). This paper also demonstrates that a standard Diamond-Mortensen-Pissarides (DMP) model (Pissarides (2000)) with a spatial dimension can explain empirical findings in the first section. The theoretical part of the paper resonates with the work of Kuhn, Manovskii, and Qiu (2021), who show that the persistence of unemployment across space can be explained by a standard DMP model with utility equalized across space.

Section 3 introduces the key aspects of the FTZ reform. Section 4 describes the data sources and discusses important characteristics of the Paris metropolitan area. Section 5 presents estimation results with a homogeneous treatment and units geographically close to each other. Section 6 discusses estimation with heterogeneous treatments and more geographically dispersed units. Section 7 shows that a calibrated spatial search-and-matching model can explain the empirical findings. The last section concludes.

3. A Brief History of the Reform

Commuting by public transport is widespread in the Paris metropolitan area. In the city of Paris, approximately 78% of employed workers opt for this solution. Many choose to buy one of the several public transport passes available, giving full access to the region Ile-de-France public transport network, which includes metros, buses, tramways, RER[1] and some trains. For regular commuters, the most popular option is to buy a Navigo Travel Card (NTC), valid for a week, a month or a year.[2] Until September 2015, the price of the NTC depended on the fare zones crossed during the travel. Typically, users living in Paris or in cities sharing a border with Paris (fare zones 1 or 2) and working in Paris would buy the NTC valid for zones 1 and 2 only. Users living in the inner suburbs (fare zone 3) and working in Paris would choose the NTC valid for zones 1 to 3, paying

[1]RER (Réseau Express Régional) are express train lines connecting the city of Paris to neighboring cities.

[2]Students have access to the yearly equivalent of the Navigo card, called the ImaginR pass, which comes with a substantial student discount.

a premium for the extra distance traveled (see Figure 1 for the different fare zones in the region Ile-de-France).

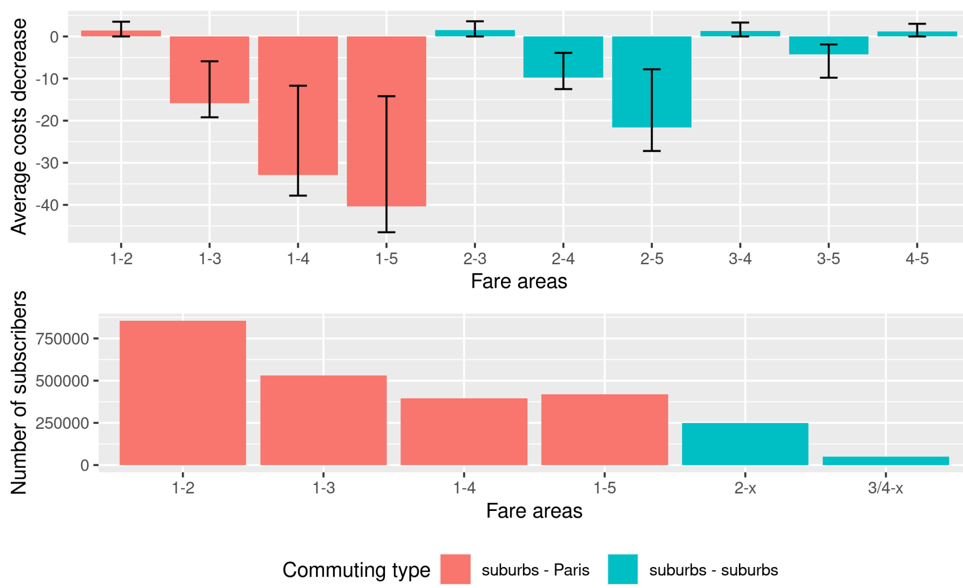
In September 2015, the "dézonage" reform removed the link between the fare zones crossed during commuting and the price of the NTC.[3] A new travel pass named "Forfait Toutes Zones" (FTZ) was created, which replaced the previous multi-price scheme by a *single price*. The FTZ travel pass created a discontinuity in the cost of commuting with public transport. For instance, users of the monthly NTC valid for zones 1 to 3 experienced a 19.2 euro monthly decrease in their commuting costs, while the cost of a monthly NTC valid for zones 1 and 2 did not change. Hence, the reform generated a discontinuity in commuting costs for those living on different sides of the border separating the fare zones 2 and 3. In France in 2015, the net minimum wage of a full-time worker was 1136 euro. So the monthly savings for a worker buying a NTC for zones 1 to 3 amounts to approximately 1.7% of the net minimum wage. This number is likely to be a *lower bound* on the real commuting costs discontinuity, as reports suggest that the reform encouraged users to buy weekly or monthly Navigo passes instead of pricier single-ride tickets, generating additional cost-saving for residents of zone 3 (see OMNIL (2018)). In Figure 2, I report the minimum, average and maximum savings generated by the reform. In particular, I find that zone 3 residents commuting to the city center experienced on average a 15.9 euro decrease in their monthly commuting costs. Note that firms have a legal obligation to reimburse at least 50% of their employees' NTC. Thus, one could also analyze the reform as allowing firms to decrease their costs of hiring. However, in a search-and-matching model with linear utility and zero bargaining power for workers, I show below that the equilibrium does not depend on how commuting costs are shared between employees and firms. Only the total value of commuting costs matters for job creation, not who pay them.

The first part of my empirical strategy relies on the fact along the border between zones 2 and 3, whether a municipality falls on one or the other can be considered a random experiment. Hence, the "dézonage" reform generates a quasi-random variation in commuting costs for the treated units (cities in the fare zone 3 close to the fare zone 2) relative to the untreated units (cities in the fare zones 1 and 2, close to the fare zone 3). In the second section of my empirical strategy, I relax the constraint that the treated

[3]The price of single-ride tickets is still dependent on the fare zones crossed during the travel.

units should belong to the fare zone 3. I also include units from zones 4 and 5 in the treatment group. While this increases the odds of unobserved characteristics driving the results, this setting limits the likelihood of spillovers between treated and non-treated units. It also creates a setting with heterogeneous treatment among the treated, which I use to explore the mechanisms behind the local employment effects of the FTZ reform.

FIGURE 2. Change in monthly commuting costs and number of subscribers by travel pass and fare zone in 2015



Notes: Author's calculations based on data from the STIF. The top panel shows the average impact of the "dézonage" reform on the monthly cost of using a travel pass. For instance, users of a zone 1-2 pass experienced an average 1.4 euro monthly *increase*, while users of a zone 1-3 pass experienced an average 15.9 euro monthly *decrease*. Horizontal lines around the average values represent the minimum and the maximum savings. For instance, users of the monthly zone 1-3 pass experienced a 19.2 euro monthly decrease, which is the maximum savings for zones 1-3 users. The bottom panel displays the number of subscribers by travel pass and fare zone. The label $2-x$ indicates passes valid for the fare zones $2-3$, $2-4$ and $2-5$; the label $3/4-x$ indicates passes valid for the fare zones $3-4$, $3-5$ and $4-5$. Because the focus of this paper is on workers, calculations do not include values for the ImagineR pass, which is a student travel pass.

4. Data and Stylized Facts

This section describes my data sources and discusses key empirical facts for the Parisian metropolitan area. For my empirical analysis, I use a combination of administrative data and observations collected using web scraping techniques.

Regarding data sources, to measure the local employment impact of the reform in the region Ile-de-France, I use the database "Activité des résidents" from INSEE, the French national statistics institute. This database, relying on observations from the national census, includes population characteristics on January 1st of each year at the IRIS level, which is a sub-city unit containing approximately 2000 inhabitants. The "Activité des résidents" database contains detailed information on population structure by age group, sex and employment status. The database also provides information on the different ways employees commute to work. It offers observations at a very fine level of spatial granularity, with observations available at the yearly frequency for the period 2009 - 2018. To complement my analysis, I also use data from the French unemployment agency, Pôle Emploi. The Pôle Emploi dataset is at the monthly frequency for the period 2010-2018. However, observations are grouped at the municipal level, which is a higher level of spatial aggregation.[4] One advantage of the Pôle Emploi dataset is that it offers a detailed view on registered workers. Based on the number of hours they worked in the previous month and on their current availability, workers registered at Pôle Emploi are assigned to one of five categories (A-E). Those who worked zero hours in the previous month and are actively searching for a job are in category A. Those who worked up to 78 hours in the previous month are assigned to category B. Those in categories C-E worked more than 78 hours in the previous month or are not directly available for a job (see Appendix A).

Unfortunately, there is no publicly available map for fare zones at the IRIS level. Therefore, I gathered data on the optimal route to central Paris using the website maintained by Île-de-France Mobilités[5]. For the center of Paris, I use the train-metro station Châtelet-les-Halles. For each IRIS, I drew an address at random, as a starting point for the itinerary. I let the website find the itinerary with shortest time on a typical Monday

[4]In the current setting, municipalities are cities surrounding Paris, as well as the 1st-20th arrondissements of Paris. In the text, I also use the term communes to designate municipalities.

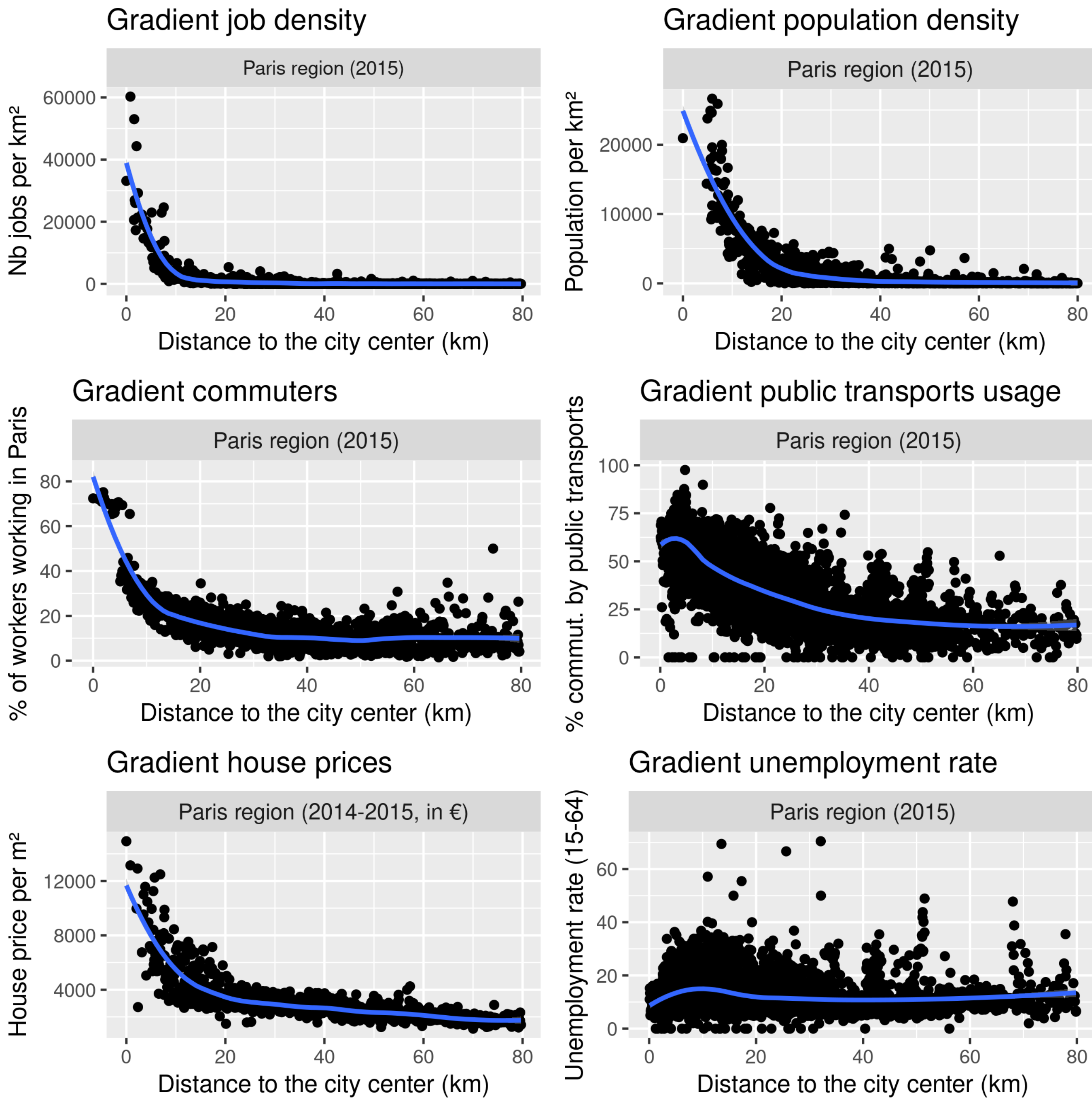
[5]Vianavigo: `https://www.vianavigo.com/accueil`

morning to arrive at Châtelet-les-Halles at 9:00 a.m. The optimal itinerary may combine several public transport modes (bus, metro, train) and walking. Results are presented in Figure 1. For real estate prices, I use the DVF database[6], which contains all private residential property transactions in France, starting from January 2014. Details include the total price paid in each transaction, the geographical situation of the property, as well as some other characteristics (the surface of the property, the number of rooms, etc.).

The Paris region is characterized by five key gradients, illustrated in Figure 3. First, most jobs are located near the city center. Therefore, the Paris metropolitan area can be well approximated by a *monocentric model*, in which all jobs are located in the city center. The population gradient in the top right-panel closely follows the shape of the job gradient. The middle-left panel shows that about 75% Paris residents who are employed work in the city center. This percentage decays to about 10% for those who are residents in cities 40 km away from the center and stabilizes at greater distances. The middle-right panel shows that near the city center, approximately 60% of the workforce uses public transportation to get to work. At greater distances from the center, this share decreases to around 20% of the workforce for cities located more than 60 km from the center. The bottom-left panel displays the house price gradient, around 12000 euros per square meter in the city center, decreasing to approximately 3000 euros per square meter 80 km from the center. Finally, the bottom-right panel indicates the absence of an unemployment gradient. The local unemployment rate is roughly constant except for the small increase around 10 km from the center, which reflects the higher concentration of social housing in some suburbs (see Chapelle, Wasmer, and Bono (2020)).

[6] `https://www.data.gouv.fr/en/datasets/demandes-de-valeurs-foncieres-geolocalisees/`

Figure 3. Key gradients for the Paris metropolitan area



Sources: Author's calculations based on data from the INSEE and the DVF database.

Notes: Distances are measured from the metro station Châtelet. For public transport usage and unemployment, each point represents an IRIS. In the other panels, each point represents a municipality. The blue lines are smoothed conditional means.

5. Difference-in-Difference with homogeneous treatment

5.1. **Commuting and employment outcomes.** In this section, I use a difference-in-difference specification, limiting treatment and control groups to geographical areas that are close to each other. The treatment group includes units located in zone 3, while the control group includes units in zones 1 and 2. To analyze the impact of the FTZ reform on commuting and employment, I use the following baseline specification:

$$y_{it} = \alpha_i + \gamma_t + \beta \times \delta_{i,t} + \eta_i \times t + \varepsilon_{i,t} \quad (1)$$

with $y_{i,t}$ a dependent variable observed in area i at time t; α_i a unit-specific fixed effect capturing permanent differences between areas; γ_t a time fixed effect capturing macro changes common to all units in a given period; $\delta_{i,t}$ an indicator variable equal to 1 if the area is in zone 3 *and* the observation is dated after the reform (1 September 2015); $\varepsilon_{i,t}$ is an area and time-specific *i.i.d* error term. I also control for potential differences in pre-trends by including unit-specific trends (η_i).

The main identifying assumption for β to provide an unbiased estimate of the treatment effect is the *conditional common trend assumption*: in the absence of treatment and after controlling for confounding factors, treated and untreated units would have evolved along the same path. In the present setting, the common trend assumption is likely to hold for two reasons. First, by restricting the sample to units that are close to each other, I control for the impact of potential unobservable characteristics on local employment dynamics. Second, by including unit-specific time trends I control for potential differences in pre-trends before the reform that would lead to spurious results. The common trend assumption makes it possible to use the path of untreated units to build the counterfactual path that treated units would have followed in the absence of treatment. For this approach to be valid, the treatment should affect treated units only. As illustrated in Figure 3, a very large share of zones 1 and 2 residents work in Paris (approximately 75% of employed Paris residents work in the city). For these individuals, the costs of commuting remained constant. Thus, to a first order approximation, we can assume that they were not affected by the reform.[7]

The above discussion suggests one limitation of the current framework. In the current setting it is hard to disentangle the growth effect from the reorganization effect of the reform (Redding and Turner, 2015). In particular, when considering employment effects, one cannot rule out that the estimated effects result from the shifts of jobs from one group to another, in a zero-sum fashion. This is why the empirical section of this paper make claims about *local* employment effects of the FTZ reform, not about the employment effects for the entire region Ile-de-France.[8]

Table 1 presents descriptive statistics for the control group (zones 1 and 2) and the treatment group (zone 3). There is little difference in terms of population (15-64), which reflects the fact that IRIS are designed to target 2000 residents per unit. Control and treatment groups also have similar population structures, both in terms of age categories and gender. The unemployment rate, defined as the number of unemployed workers divided by the active workforce, is one percentage point higher in the treatment group than in the control group. Workers in the treatment group are also slightly more likely to be salaried and employed with a permanent contract. In terms of commuting modes, individuals in the treatment group are less likely to commute using public transport (46.1% vs 59.3%) and more likely to use their car to go to work (38.2% vs 17.8%). Importantly, Table 1 indicates that units in the treatment group are evenly distributed in space, as average latitude and longitude are almost equal for the two groups.

[7] Some zone 1 and 2 residents working in zones 3-5 benefited from the reform. They may have accepted more distant jobs than they would have considered without the reform. This creates a bias, reducing the estimated impact on local unemployment.

[8] However, note that the model presented below rationalizes the empirical findings with a global employment effect.

Table 1. Descriptive statistics for treatment group (zones 1-2) and control group (zone 3)

	Control (N=14483)	Treatment (N=8523)		Control (N=14483)	Treatment (N=8523)
Population 15-64			**Salaried temporary position/Active Population 15-64**		
Mean (SD)	1740 (629)	1670 (509)	Mean (SD)	0.0840 (0.0322)	0.0671 (0.0222)
Median [Min, Max]	1680 [19.0, 7690]	1590 [20.0, 5170]	Median [Min, Max]	0.0814 [0, 0.762]	0.0647 [0, 0.264]
Population 15-24/Population 15-64			**Part-time workers/Active Population 15-64**		
Mean (SD)	0.188 (0.0491)	0.191 (0.0442)	Mean (SD)	0.143 (0.0327)	0.126 (0.0266)
Median [Min, Max]	0.182 [0.0319, 0.796]	0.187 [0, 0.606]	Median [Min, Max]	0.141 [0, 0.475]	0.125 [0, 0.284]
Population 25-54/Population 15-64			**Salaried subsidized job/Active Population**		
Mean (SD)	0.651 (0.0632)	0.643 (0.0522)	Mean (SD)	0.00317 (0.00392)	0.00366 (0.00514)
Median [Min, Max]	0.655 [0.152, 0.906]	0.643 [0.315, 0.897]	Median [Min, Max]	0.00221 [0, 0.0541]	0.00269 [0, 0.152]
Population 55-64/Population 15-64			**Walking to work/Employed Workers**		
Mean (SD)	0.161 (0.0410)	0.166 (0.0367)	Mean (SD)	0.103 (0.0455)	0.0739 (0.0386)
Median [Min, Max]	0.158 [0, 0.724]	0.165 [0.00476, 0.424]	Median [Min, Max]	0.0947 [0, 0.522]	0.0673 [0, 0.512]
Men 15-64/Population 15-64			**Biking to work/Employed Workers**		
Mean (SD)	0.481 (0.0382)	0.487 (0.0346)	Mean (SD)	0.0746 (0.0349)	0.0506 (0.0298)
Median [Min, Max]	0.478 [0.190, 1.00]	0.484 [0.346, 0.896]	Median [Min, Max]	0.0730 [0, 0.826]	0.0469 [0, 0.584]
Women 15-64/Population 15-64			**Commuting by car/Employed Workers**		
Mean (SD)	0.519 (0.0382)	0.513 (0.0346)	Mean (SD)	0.178 (0.0958)	0.382 (0.103)
Median [Min, Max]	0.522 [0, 0.810]	0.516 [0.104, 0.654]	Median [Min, Max]	0.156 [0, 0.840]	0.374 [0, 0.758]
Active Population 15-64/Population 15-64			**Commuting by public transport/Employed Workers**		
Mean (SD)	0.771 (0.0603)	0.756 (0.0617)	Mean (SD)	0.593 (0.110)	0.461 (0.104)
Median [Min, Max]	0.778 [0.254, 1.00]	0.763 [0.0526, 0.928]	Median [Min, Max]	0.609 [0, 0.899]	0.466 [0, 0.852]
Unemployed workers 15-64/Active Population 15-64			**Commuting time to city center (mn)**		
Mean (SD)	0.124 (0.0485)	0.134 (0.0626)	Mean (SD)	24.2 (9.45)	41.1 (8.56)
Median [Min, Max]	0.113 [0.0110, 0.315]	0.118 [0.0112, 0.316]	Median [Min, Max]	24.0 [3.00, 118]	41.0 [20.0, 78.0]
Unemployed men 15-64/Active Population 15-64			**Distance nearest RER-train station (km)**		
Mean (SD)	0.0607 (0.0283)	0.0676 (0.0345)	Mean (SD)	1.11 (0.632)	1.05 (0.676)
Median [Min, Max]	0.0547 [0, 0.238]	0.0596 [0, 0.308]	Median [Min, Max]	1.00 [0.0599, 3.86]	0.868 [0.0302, 3.66]
Unemployed women 15-64/Active Population 15-64			**Geodesic distance to city center (km)**		
Mean (SD)	0.0635 (0.0249)	0.0665 (0.0318)	Mean (SD)	4.72 (2.15)	10.2 (1.78)
Median [Min, Max]	0.0590 [0, 0.303]	0.0589 [0, 0.217]	Median [Min, Max]	4.47 [0.104, 13.0]	10.2 [6.23, 15.5]
Salaried workers 15-64/Active Population 15-64			**Latitude**		
Mean (SD)	0.763 (0.0572)	0.793 (0.0552)	Mean (SD)	48.9 (0.0315)	48.9 (0.0612)
Median [Min, Max]	0.766 [0.160, 1.06]	0.798 [0.308, 1.01]	Median [Min, Max]	48.9 [48.8, 48.9]	48.9 [48.7, 49.0]
Salaried permanent position/Active Population 15-64			**Longitude**		
Mean (SD)	0.643 (0.0655)	0.692 (0.0696)	Mean (SD)	2.34 (0.0519)	2.35 (0.106)
Median [Min, Max]	0.643 [0.0238, 0.957]	0.700 [0.249, 0.947]	Median [Min, Max]	2.35 [2.23, 2.47]	2.34 [2.16, 2.52]

Sources: Author's calculations based on data from the INSEE.

Notes: The city center for Paris is represented by the metro station Châtelet. Observations are at the year-IRIS level for the period 2009-2018. The control group includes units belonging to zones 1 and 2, while the treatment group includes units from the zone 3.

Table 2 displays the results from regression 1, where the dependent variable is the percentage of employed workers using public transport. To justify the inclusion of IRIS-specific linear pre-trends, I first run a regression including a Placebo variable, which is a dummy variable equal to 1 one period before the reform.[9] The first column omits IRIS-specific pre-trends ($\eta_i = 0$). Without IRIS-specific pre-trends, the conditional pre-trend assumption does not hold, as confirmed by the positive and statistically significant coefficient estimated for the Placebo variable. The second column shows that controlling for IRIS-specific linear pre-trends restores the conditional trend assumption, as indicated

[9]Observations are available at yearly frequency, with each observation indicating the value in January. The dummy variable Placebo is equal to 1 in January 2015 and 0 otherwise. A significant value for the Placebo coefficient suggests differences in pre-trends between the treatment and control groups.

by the small and statistically insignificant coefficient on the Placebo dummy. The third column reports the baseline estimate for the impact of the FTZ reform. I find that the reform led to a 0.60 percentage point increase in the share of employed workers commuting by public transport in zone 3, with the coefficient significant at the 1% level. In columns 4-7, I restrict the sample to geographical units within a certain distance of the border separating zones 2 and 3. For a maximum distance of 5 km to the border, the coefficient indicates that the reform caused a 0.58 percentage point increase in public transport usage, with the coefficient still significant at the 1% level. For a maximum distance of 2 km to the border, the coefficient suggests that the reform caused a 0.44 percentage point increase in commuting by public transport, with the coefficient only significant at the 11% level. The reduction in statistical significance could be driven by the reduction in sample size: there are 52% fewer observations in column 7 than in column 3. In columns 8-14, I cluster standard errors at the commune level, to take into account the potential spatial correlation of observations. This higher level of spatial aggregation barely affects the estimated standard errors.

Regarding the impact of the FTZ reform on local unemployment, estimation results are presented in Table 3. Columns 1-2 justify the inclusion of IRIS-specific pre-trends. The third column reports my preferred estimate of the impact of the FTZ reform on local unemployment. This indicates that the reform led to 0.25 percentage point decrease in unemployment rate for geographical units in zone 3. The coefficient is statistically significant at the 4% level. Restricting the sample to geographical units within a certain distance to the border separating zones 2 and 3 barely affects the estimated coefficient (see columns 4-6), except in column 7. As in Table 2, column 7 contains 52% fewer observations than column 1-3, which may explain the reduction in statistical significance. Also, the closer units are to the border, the more likely are spillovers between treated and untreated. For example, residents in zone 3 may walk to the nearest metro station in zone 2 to benefit from the lower fare. Also note that clustering observations at the commune level results in less significant estimates. For instance, the p-value of my preferred estimate goes from 4% to 15%.

To further assess the degree to which the FTZ reform caused a reduction in local unemployment, I now turn to the Pôle Emploi dataset, with the results presented in Table 4. While the regressions above involve observations at the IRIS-year frequency, the Pôle Emploi dataset is at the commune-month frequency. This dataset offers more observations along the time dimension, but fewer observations along the spatial dimension, since there are many IRIS with each. I calculate the fare zone at the commune level by averaging across the corresponding IRIS. Each commune with a value above 2.5 is assigned to the treatment group. While IRIS were designed to have a population size of 2000 inhabitants, population per commune is not homogeneous. To take that factor into account, I weight observations by population size. I also consider the different categories used at Pôle Emploi. In columns 1-3, the dependent variable includes all categories. Columns 1-2 justify the need for the inclusion of commune-specific pre-trends. According to estimates in the third column, the reform caused a 1.6% decrease in the number of workers registered at Pôle Emploi for communes in the treatment group. However, when I restrict the sample to individuals who worked zero hours in the previous month and who are actively searching for a job (category A), the coefficient indicates a 2.7% decrease for this specific category. This coefficient is large and strongly significant, which I interpret as additional evidence that the reform caused a decrease in unemployment for municipalities in the treatment group. In the Pôle Emploi classification, those in categories B-E typically worked at least 78 hours the preceding month, or are not actively searching for a job. A regression that only excludes category A, suggests that the reform led to a 1.3% increase in the number of individuals registered in these categories. To reconcile the discrepancies between columns 3, 6 and 9, one may posit that the reform caused a 2.7% decrease in the number of unemployed workers who worked zero hours in the previous month (category A), with some transitioning from category A to part-time jobs (categories B-E).

Table 2. Regression 1: Public transport usage

	Dependent variable: Share of employed workers commuting using public transport													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	1.17***	0.65***	0.60***	0.58***	0.46**	0.41*	0.44	1.17***	0.65***	0.60**	0.58**	0.46*	0.41	0.44
	(0.00)	(0.00)	(0.00)	(0.01)	(0.03)	(0.08)	(0.11)	(0.00)	(0.00)	(0.01)	(0.02)	(0.06)	(0.11)	(0.13)
Placebo	0.34**	0.10						0.34**	0.10					
	(0.01)	(0.24)						(0.05)	(0.34)					
Num.Obs.	23 006	23 006	23 006	21 655	19 279	15 547	10 939	23 006	23 006	23 006	21 655	19 279	15 547	10 939
R2	0.939	0.969	0.969	0.969	0.970	0.966	0.954	0.939	0.969	0.969	0.969	0.970	0.966	0.954
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 include all geographical units in zones 1-3. Columns 4-3 and 11-14 restrict the sample to units within a certain distance of the border between zones 2 and 3.

TABLE 3. Regression 1: Unemployment rate

	Dependent variable: Unemployment Rate													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.46***	−0.20*	−0.25**	−0.22*	−0.16	−0.20	−0.09	0.46***	−0.20	−0.25	−0.22	−0.16	−0.20	−0.09
	(0.00)	(0.08)	(0.04)	(0.08)	(0.21)	(0.16)	(0.62)	(0.00)	(0.21)	(0.15)	(0.21)	(0.36)	(0.31)	(0.73)
Placebo	0.41***	0.10*						0.41***	0.10*					
	(0.00)	(0.05)						(0.00)	(0.06)					
Num.Obs.	23 006	23 006	23 006	21 655	19 279	15 547	10 939	23 006	23 006	23 006	21 655	19 279	15 547	10 939
R2	0.892	0.945	0.945	0.946	0.948	0.950	0.952	0.892	0.945	0.945	0.946	0.948	0.950	0.952
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 include all units in zones 1-3. Columns 4-3 and 11-14 restrict the sample to units within a certain distance of the border between zones 2 and 3.

Table 4. Regression 1: Registered unemployment

	Dependent variable: Log number registered unemployed								
	Full Sample: zones 1-3								
	All categories			Category A			All categories excluding A		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
β	0.070***	−0.011**	−0.016***	0.057***	−0.024***	−0.027***	0.101***	0.021**	0.013
	(0.000)	(0.026)	(0.004)	(0.000)	(0.000)	(0.000)	(0.000)	(0.013)	(0.131)
Placebo	0.043***	0.009***		0.039***	0.005		0.047***	0.013***	
	(0.000)	(0.002)		(0.000)	(0.102)		(0.000)	(0.003)	
Num.Obs.	11 340	11 340	11 340	11 340	11 340	11 340	11 340	11 340	11 340
R2	0.998	1.000	1.000	0.998	0.999	0.999	0.997	0.999	0.999
Cluster SE	COM	COM	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X	X
COM trends		X	X		X	X		X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from Pôle Emploi.

Notes: Standard errors are clustered at the commune level (COM). Placebo is an indicator variable equal to 1 in the year before the reform. For the first three columns, the dependent variable is the number of individuals registered at Pôle Emploi. For columns 4-6, the dependent variable only includes those in category A. For columns 4-6, the dependent variable includes all those except category A.

5.2. **Impact on house prices.** I now turn my attention to the impact of the FTZ reform on house prices. I have to adapt equation (1) because property transactions are not in repeated cross section. Instead, each property is identified by its own latitude and longitude. Observations are reported for each day of the year between 2014 and 2018. I use the following specification:

$$p_{jt} = \alpha_{c(j)} + \gamma_{k(t)} + \beta \times \delta_{j,t} + \eta_c(j) \times t + \Omega X_{j,t} + \varepsilon_{j,t} \tag{2}$$

where the dependent variable p_{jt} is the logarithm of the price per square meter of transaction j at time t. The function $c(j)$ maps observation to their municipality. Thus, the variable $\alpha_{c(j)}$ is a municipality fixed effect. The variable $\gamma_{k(t)}$ is date fixed effect, where the function $k(t)$ maps the day t to the nearest date in a year-month format. The variable $\delta_{j,t}$ is an indicator variable equal to 1 if the property j is in zone 3 and if the observation occurs after the reform. As in the previous section, I control for potential violations of parallel pre-trends by including municipality-specific trends ($\eta_c(j)$). I also control for quality differences across properties by including a cubic polynomial of the distance to the center of Paris, the property type (house or apartment) and the number of rooms ($X_{j,t}$).

Results are presented in Table 5. The first two columns justify the need for commune-specific trends to control for differences in pre-trends between the treatment and control groups. The third column reports my preferred estimate. Holding the observable characteristics of properties constant, the estimated coefficient suggests that the FTZ reform caused a 1.4% increase residential real estate prices per square meter in the treatment group. Restricting the sample to sales located closer to the border separating zones 2 and 3 (columns 4-7) results in even larger and more statistically significant estimates. In columns 8-14, instead of clustering standard errors at the IRIS level, I cluster standard errors at the commune level. With this higher level of spatial aggregation, estimates are no longer statistically significant at conventional levels. However, the arbitrary borders represented by municipalities may not provide the most accurate spatial aggregation to measure spatial correlation in house prices.

Table 5. Regression 2: House prices

	Dependent variable: Log price per square meter													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	−0.032***	0.015*	0.014*	0.015*	0.017**	0.017*	0.020**	−0.032	0.015	0.014	0.015	0.017	0.017	0.020
	(0.000)	(0.074)	(0.076)	(0.051)	(0.040)	(0.057)	(0.038)	(0.126)	(0.380)	(0.377)	(0.329)	(0.305)	(0.357)	(0.307)
Placebo	0.015	0.009						0.015	0.009					
	(0.102)	(0.167)						(0.453)	(0.481)					
Num.Obs.	330 384	330 384	330 384	309 256	273 601	212 048	145 861	330 384	330 384	330 384	309 256	273 601	212 048	145 861
R2	0.514	0.546	0.546	0.548	0.548	0.538	0.508	0.514	0.546	0.546	0.548	0.548	0.538	0.508
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the DVF database.

Notes: Placebo is an indicator variable equal to 1 in the year before the reform. Controls includes a cubic polynomial for the distance to the center of Paris, the type of property (house or apartment) and the number of rooms. For columns 1-7, errors are clustered at the IRIS level. For columns 8-14, errors are clustered at the commune (i.e. municipality) level.

In Appendix B, I report additional robustness checks. First, I exclude observations in zone 3 that are within a certain distance to the border (500 meters). This robustness check controls for the risk of potential spillovers between the treatment and control groups, since some residents near the border may simply have walked to the nearest metro station in zone 2. Overall, I do not find significant changes in the coefficient values, but the standard errors are sometimes larger, which could be due to the smaller number of observations in the treatment group. Second, I run a Granger causality test to estimate how a future treatment dummy affects the dependent variables in the pre-treatment period. I find no clear evidence of differences in pre-trends between the treatment and control groups.

6. Difference-in-Difference with heterogeneous treatment

6.1. Commuting and employment outcomes. I now extend the scope of the analysis to include geographical units from zones 4 and 5. Residents in these areas using public transport to commute to jobs in the city center saved 32 to 40 euros a month on average in the post-treatment period (see Table 2). The control group still includes units in zones 1 and 2. However, now the treatment group is constituted of units in zones 3 - 5. In this section, the baseline specification is:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \sum_{i=3}^{5} \beta^{(i)} \times \delta_{i,t}^{(i)} + \eta_i \times t \\ + \beta_d \times d \times \delta_{i,t} + \beta_R \times d_R \times \delta_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{3}$$

where $\delta^{(i)}$ is an indicator variable equal to 1 for zone i in the post-treatment period. α_i and γ_t are still unit-specific and period-specific fixed effects. I still control for potential differences in pre-trends by including unit-specific trends with the parameters η_i. The parameters $\{\beta^{(i)}\}_{i=3}^{5}$ capture the zone-specific treatment effects of the reform. While commuting costs in zones 4 and 5 declined more than in zone 3, residents in zones 4 and 5 are also less likely to work in the city center. To control for the degree of attachment to labor market in the city center, I interact the treatment dummy ($\delta_{i,t}$) with the geodesic distance to the center of Paris (d). I also interact the treatment dummy with the shortest distance to a RER-train station (d_R). After controlling for different declines in commuting costs, individuals who live further away from train stations should be less affected by the reform.

For the regression to provide a valid estimate of the treatment effect, the path of untreated units (zones 1-2) must generate a meaningful counterfactual path for treated units (zones 3-5). Because units in the zones 4-5 are further from the city center, the risk of unobservable variables driving the results is higher compared to the setting described in section 5. However, the larger spatial distance between units in the treatment and control groups also diminishes the likelihood of spillovers between treated and untreated units. Furthermore, including observations from zones 4-5 generates substantial variation in covariates among the treated units, which I use to analyze the mechanisms behind the reform. In that sense, the results in this section are complementary to those in the previous section.

Table 6 presents descriptive statistics for the control group (zones 1 and 2) and the treatment group (zone 3-5). As in the previous section, there are no major differences in terms of population characteristics. However, there are large differences in commuting modes. Workers in the treatment group are less likely to use using public transport

(33.8% vs 59.3%) and more likely to use their car (53.8% vs 17.8%). Average values for latitude and longitude are similar between treatment and control groups, which indicates that units are evenly distributed across space.

TABLE 6. Descriptive statistics for the treatment and control groups (IRIS): zones 1-2 vs zones 3-5

	Control (N=14491)	**Treatment (N=34147)**		**Control (N=14491)**	**Treatment (N=34147)**
Population 15-64			**Salaried temporary position/Active Population 15-64**		
Mean (SD)	1740 (629)	1540 (772)	Mean (SD)	0.0839 (0.0319)	0.0613 (0.0225)
Median [Min, Max]	1680 [19.0, 7690]	1490 [10.0, 9740]	Median [Min, Max]	0.0814 [0, 0.762]	0.0578 [0, 0.535]
Population 15-24/Population 15-64			**Part-time workers/Active Population 15-64**		
Mean (SD)	0.188 (0.0491)	0.194 (0.0426)	Mean (SD)	0.143 (0.0327)	0.127 (0.0273)
Median [Min, Max]	0.182 [0.0319, 0.796]	0.189 [0, 0.794]	Median [Min, Max]	0.141 [0, 0.475]	0.125 [0, 0.504]
Population 25-54/Population 15-64			**Salaried subsidized job/Active Population**		
Mean (SD)	0.651 (0.0632)	0.628 (0.0505)	Mean (SD)	0.00317 (0.00392)	0.00390 (0.00579)
Median [Min, Max]	0.655 [0.152, 0.906]	0.628 [0.154, 1.00]	Median [Min, Max]	0.00221 [0, 0.0541]	0.00287 [0, 0.425]
Population 55-64/Population 15-64			**Walking to work/Employed Workers**		
Mean (SD)	0.161 (0.0410)	0.178 (0.0430)	Mean (SD)	0.103 (0.0455)	0.0562 (0.0388)
Median [Min, Max]	0.158 [0, 0.724]	0.176 [0, 0.692]	Median [Min, Max]	0.0947 [0, 0.522]	0.0500 [0, 0.600]
Men 15-64/Population 15-64			**Biking to work/Employed Workers**		
Mean (SD)	0.481 (0.0382)	0.491 (0.0300)	Mean (SD)	0.0745 (0.0349)	0.0333 (0.0247)
Median [Min, Max]	0.478 [0.190, 1.00]	0.490 [0.298, 0.953]	Median [Min, Max]	0.0729 [0, 0.826]	0.0286 [0, 0.584]
Women 15-64/Population 15-64			**Commuting by car/Employed Workers**		
Mean (SD)	0.519 (0.0382)	0.509 (0.0300)	Mean (SD)	0.178 (0.0959)	0.538 (0.172)
Median [Min, Max]	0.522 [0, 0.810]	0.510 [0.0474, 0.731]	Median [Min, Max]	0.156 [0, 0.840]	0.528 [0, 1.30]
Active Population 15-64/Population 15-64			**Commuting by public transport/Employed Workers**		
Mean (SD)	0.770 (0.0603)	0.753 (0.0578)	Mean (SD)	0.593 (0.110)	0.338 (0.153)
Median [Min, Max]	0.778 [0.254, 1.00]	0.760 [0.0518, 1.00]	Median [Min, Max]	0.609 [0, 0.899]	0.343 [0, 1.00]
Unemployed workers 15-64/Active Population 15-64			**Commuting time to city center (mn)**		
Mean (SD)	0.124 (0.0488)	0.118 (0.0578)	Mean (SD)	24.2 (9.46)	60.3 (21.8)
Median [Min, Max]	0.113 [0.0164, 0.333]	0.100 [0.0150, 0.334]	Median [Min, Max]	24.0 [3.00, 118]	55.0 [20.0, 163]
Unemployed men 15-64/Active Population 15-64			**Distance nearest RER-train station (km)**		
Mean (SD)	0.0609 (0.0285)	0.0589 (0.0312)	Mean (SD)	1.11 (0.632)	1.94 (2.26)
Median [Min, Max]	0.0547 [0, 0.238]	0.0502 [0, 0.333]	Median [Min, Max]	1.00 [0.0599, 3.86]	1.20 [0.0180, 21.4]
Unemployed women 15-64/Active Population 15-64			**Geodesic distance to city center (km)**		
Mean (SD)	0.0636 (0.0250)	0.0591 (0.0303)	Mean (SD)	4.72 (2.15)	24.6 (16.3)
Median [Min, Max]	0.0591 [0, 0.303]	0.0508 [0, 0.246]	Median [Min, Max]	4.47 [0.104, 13.0]	18.8 [6.23, 91.4]
Salaried workers 15-64/Active Population 15-64			**Latitude**		
Mean (SD)	0.762 (0.0573)	0.805 (0.0532)	Mean (SD)	48.9 (0.0315)	48.8 (0.168)
Median [Min, Max]	0.766 [0.160, 1.06]	0.811 [0.308, 1.23]	Median [Min, Max]	48.9 [48.8, 48.9]	48.9 [48.1, 49.2]
Salaried permanent position/Active Population 15-64			**Longitude**		
Mean (SD)	0.643 (0.0655)	0.706 (0.0667)	Mean (SD)	2.34 (0.0519)	2.36 (0.307)
Median [Min, Max]	0.643 [0.0238, 0.957]	0.717 [0.128, 1.23]	Median [Min, Max]	2.35 [2.23, 2.47]	2.35 [1.48, 3.48]

Sources: Author's calculations based on data from the INSEE.

Notes: Paris city center is represented by metro station Châtelet. Observations are at the year-IRIS level for the period 2009-2018. The control group includes units belonging to zones 1 and 2, while the treatment group includes units from the zones 3-5.

Table 7 displays the results from regression 3, using the percentage of employed workers commuting using public transport as dependent variable. The third column indicates that the reform caused a 0.28 percentage point increase in the share of employed workers commuting by public transport. The fourth column indicates that effects of the reform on commuting habits are stronger in zone 3 (0.58 ppt), milder in zone 4 (0.33 ppt) and negligible in zone 5 (0.08 ppt, but not statistically significant). The coefficients for β_R and β_d are not statistically significant. Given that the reform generated larger savings in zone 3 than in zone 5, the expected result would have been $\beta^{(5)} > \beta^{(3)}$. I show below that this unexpected result can be explained by the emergence of new local employment opportunities in zones 4 and 5.

Table 8 shows the estimated effects of the FTZ reform on local unemployment rates. The third column suggests that the reform caused a 0.17 percentage point decline in the unemployment rate for the entire treatment group. The fourth column indicates a larger decrease in the local unemployment rate in zone 3 (0.25 ppt) than in zones 4 and 5 (0.13 and 0.15 ppt). The fifth column reports a positive and statistically significant coefficient on the distance to the nearest RER-metro station ($\beta_R \approx 0.04$). This indicates that employment effects are stronger for geographical units closer to a metro-train station. I interpret this finding as further evidence that changes in unemployment rates are caused by changes in the costs of commuting by public transport. Controlling for the distance to the center of Paris (those living further away should be less affected by the reform, because they are less likely to work in the city center in the first place) and for the distance to the nearest metro-train station, I find that $\beta^{(5)} > \beta^{(3)}$ (see column 7). If changes in local unemployment rates are proportional to changes in commuting costs, one should expect such a result because savings on commuting costs are substantially larger in zone 5 than in zone 3. Columns 8-14 report standard errors clustered at the commune level, which is one way of controlling for the potential spatial correlation of variables. Overall, the standard errors are larger, but the coefficient β_R is still statistically significant at the 10% level.

To further assess the robustness of the empirical findings related to local employment, I use the number of category A workers registered at Pôle Emploi as an alternative measurement of unemployment. Table 9 is similar to Table 8: the estimated decrease in the number of unemployed workers is stronger in zone 3 (2.7%) than in zones 4 and 5 (1.2% and 0.9%). However, coefficient β_R is no longer statistically significant. This could be due to the difficulty of properly measuring distance to the nearest metro-train station at the municipality level.[10]

Overall, Tables 7 and 8 reveal a tension between the previously reported empirical findings. Controlling for the attachment to the Parisian labor market, Table 8 finds large employment effects in zones 4 and 5 (between 0.26 and 0.46 ppt decrease in the unemployment rate according to column 6). However, Table 7 reports small or no effects on commuting mode in zones 4 and 5. To investigate what is causing this discrepancy, I run an additional regression with the number of newly created companies at the municipality level as a dependent variable. Table 10 shows that after controlling for the distance to Paris, municipalities in zones 4 and 5 experienced a 5.2 and 7.4% increase in the number of newly created firms (see column 6). This result recalls the findings of Mayer and Trevien (2017) and Garcia-López, Hémet, and Viladecans-Marsal (2017), who find that the introduction of the RER in the Paris metropolitan area encouraged the creation of new firms in the suburban municipalities. Overall, Table 10 offers a possible explanation to the tension previously mentioned: the decrease in unemployment rate in zone 3 is primarily due to more residents accepting to commute to jobs in Paris city center, while employment effects in zone 4 and 5 reflect the creation of more firms in suburban municipalities, generating new opportunities for local employment.

[10]Each municipality includes several IRIS. To measure the distance to nearest metro-train station at the municipality level, I took the average of distances at the IRIS level, which may result in a loss of accuracy.

Table 7. Regression 3: Public transport usage

	Dependent variable: Share of employed workers using public transport													
	Full Sample: zones 1-5													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.06	0.30**	0.28*					0.06	0.30**	0.28*				
	(0.68)	(0.04)	(0.06)					(0.81)	(0.04)	(0.06)				
Placebo	−0.06	0.04						−0.06	0.04					
	(0.54)	(0.54)						(0.62)	(0.60)					
$\beta^{(3)}$				0.58***	0.54***	0.58***	0.57***				0.58**	0.54**	0.58**	0.57**
				(0.00)	(0.01)	(0.01)	(0.01)				(0.01)	(0.02)	(0.02)	(0.02)
$\beta^{(4)}$				0.33*	0.29	0.32	0.34				0.33	0.29	0.32	0.34
				(0.08)	(0.15)	(0.19)	(0.16)				(0.11)	(0.18)	(0.22)	(0.19)
$\beta^{(5)}$				0.08	−0.03	0.05	0.09				0.08	−0.03	0.05	0.09
				(0.67)	(0.89)	(0.90)	(0.81)				(0.68)	(0.90)	(0.91)	(0.83)
β_R					0.04		0.05					0.04		0.05
					(0.44)		(0.34)					(0.48)		(0.38)
β_d						0.00	0.00						0.00	0.00
						(0.93)	(0.68)						(0.94)	(0.71)
Num.Obs.	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618	48 618
R2	0.970	0.986	0.986	0.986	0.986	0.986	0.986	0.970	0.986	0.986	0.986	0.986	0.986	0.986
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Coefficient β measures the average treatment effect for all the treated units. Coefficients $\{\beta^{(i)}\}_{i=3}^{5}$ capture zone-specific treatment effects. Coefficient β_R measures the impact of the distance to the nearest RER-train station. Coefficient β_d captures the effect of the geodesic distance to the center of Paris.

Table 8. Regression 3: Unemployment rate

	Dependent variable: Unemployment Rate													
	Full Sample: zones 1-5													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.67***	−0.12	−0.17**					0.67***	−0.12	−0.17				
	(0.00)	(0.12)	(0.04)					(0.00)	(0.23)	(0.12)				
Placebo	0.47***	0.10***						0.47***	0.10**					
	(0.00)	(0.00)						(0.00)	(0.02)					
$\beta^{(3)}$				−0.25**	−0.29**	−0.34***	−0.34***				−0.25	−0.29*	−0.34*	−0.34*
				(0.03)	(0.01)	(0.01)	(0.01)				(0.14)	(0.09)	(0.06)	(0.06)
$\beta^{(4)}$				−0.13	−0.17	−0.26**	−0.25**				−0.13	−0.17	−0.26	−0.25
				(0.22)	(0.10)	(0.04)	(0.05)				(0.35)	(0.21)	(0.11)	(0.13)
$\beta^{(5)}$				−0.15	−0.26**	−0.46***	−0.44**				−0.15	−0.26*	−0.46**	−0.44**
				(0.11)	(0.02)	(0.01)	(0.01)				(0.21)	(0.07)	(0.04)	(0.05)
β_R					0.04**		0.02					0.04*		0.02
					(0.03)		(0.27)					(0.07)		(0.37)
β_d						0.01**	0.01						0.01	0.01
						(0.05)	(0.23)						(0.10)	(0.32)
Num.Obs.	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638	48 638
R2	0.903	0.951	0.951	0.951	0.951	0.951	0.951	0.903	0.951	0.951	0.951	0.951	0.951	0.951
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Coefficient β measures the average treatment effect for all the treated units. Coefficients $\{\beta^{(i)}\}_{i=3}^{5}$ capture zone-specific treatment effects. Coefficient β_R measures the impact of the distance to the nearest RER-train station. Coefficient β_d captures the effect of the geodesic distance to the center of Paris.

Table 9. Regression 3: Registered unemployment (category A)

	Dependent variable: Log number registered unemployed as category A						
	Full Sample: zones 1-5						
	Category A						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β	0.066***	−0.012**	−0.015***				
	(0.000)	(0.027)	(0.010)				
Placebo	0.037***	0.005**					
	(0.000)	(0.041)					
$\beta^{(3)}$				−0.027***	−0.027***	−0.021***	−0.022***
				(0.000)	(0.000)	(0.005)	(0.004)
$\beta^{(4)}$				−0.012*	−0.012*	−0.003	−0.003
				(0.061)	(0.093)	(0.708)	(0.690)
$\beta^{(5)}$				−0.009	−0.008	0.011	0.011
				(0.170)	(0.285)	(0.349)	(0.351)
β_R					−0.001		0.001
					(0.697)		(0.740)
β_d						−0.001**	−0.001**
						(0.039)	(0.047)
Num.Obs.	100 932	100 932	100 932	100 932	100 932	100 932	100 932
R2	0.998	0.999	0.999	0.999	0.999	0.999	0.999
Cluster SE	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from Pôle Emploi.

Notes: Standard errors are clustered at the commune level (COM). Placebo is an indicator variable equal to 1 in the year before the reform. The dependent variable only includes individuals registered at Pôle Emploi in category A. Coefficient β measures the average treatment effect for all the treated units. Coefficients $\{\beta^{(i)}\}_{i=3}^{5}$ capture zone-specific treatment effects. Coefficient β_R measures the impact of the distance to the nearest RER-train station. Coefficient β_d captures the effect of the geodesic distance to the center of Paris.

Table 10. Regression 3: Newly created companies

	Dependent variable: Log number of newly created companies at the municipality level						
	Full Sample: zones 1-5						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β	0.087***	0.011	0.007				
	(0.002)	(0.477)	(0.641)				
Placebo	0.041***	0.008					
	(0.006)	(0.476)					
$\beta^{(3)}$				0.021	0.021	0.046	0.044
				(0.448)	(0.464)	(0.120)	(0.147)
$\beta^{(4)}$				0.013	0.013	0.052**	0.050*
				(0.514)	(0.547)	(0.044)	(0.058)
$\beta^{(5)}$				-0.007	-0.007	0.074*	0.071*
				(0.711)	(0.775)	(0.073)	(0.085)
β_R					0.000		0.003
					(0.986)		(0.652)
β_d						-0.002**	-0.003**
						(0.023)	(0.020)
Num.Obs.	6416	6416	6416	6416	6416	6416	6416
R2	0.996	0.997	0.997	0.997	0.997	0.997	0.997
Cluster SE	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from INSEE (Sirene).

Notes: Standard errors are clustered at the commune level (COM). Placebo is an indicator variable equal to 1 in the year before the reform. The dependent variable is the (log) number of newly created companies, excluding self-employed workers (removing "auto-entreprises" and "auto-entrepreneurs"). Coefficient β measures the average treatment effect for all the treated units. Coefficients $\{\beta^{(i)}\}_{i=3}^{5}$ capture zone-specific treatment effects. Coefficient β_R measures the impact of the distance to the nearest RER-train station. Coefficient β_d captures the effect of the geodesic distance to the center of Paris.

6.2. **Impact on house prices.** To analyze the impact of the FTZ reform on residential real estate prices, I use a variant of equation (2):

$$\begin{aligned} p_{jt} = \alpha_{c(j)} + \gamma_{k(t)} + \sum_{i=3}^{5} \beta^{(i)} \times \delta^{(i)}_{j,t} + \eta_c(j) \times t \\ + \Omega X_{j,t} + \beta_R \times d_R \times \delta_{j,t} + \varepsilon_{j,t} \end{aligned} \tag{4}$$

where the dependent variable p_{jt} is the logarithm of the price per square meter of transaction j that occurred at time t. The function $c(j)$ maps observation indexes to the municipality in which they are located. Thus, the variable $\alpha_{c(j)}$ is a municipality fixed effect. The variable $\gamma_{k(t)}$ is date fixed effect, where the function $k(t)$ maps the period t to the nearest date in a year-month format. The variable $\delta^{(i)}_{j,t}$ is an indicator variable equal to 1 if the property j is in zone i and if the transaction occurred after the reform. As in the previous section, I control for potential violations of parallel pre-trends by including municipality-specific trends ($\eta_c(j)$). I also control for quality differences across properties by including a cubic polynomial of the distance to the center of Paris, the property type (house or apartment) and the number of rooms ($X_{j,t}$). I interact the treatment dummy with the shortest distance to a RER-train station (d_R).[11]

Results are presented in Table 11. The third column indicates that the reform caused a 1.3% increase in the price per square meter for properties in the treatment group (zones 3-5). Column 4 shows that the positive impact of the reform is stronger in zones 4 and 5. Interestingly, column 5 finds that the positive price impact is stronger for properties located closer to a metro-train station, as indicated by the negative value for the coefficients β_R, which is strongly statistically significant ($\beta_R \approx 1.6\%$). The coefficient β_R remains statistically significant, even after clustering observations at the commune level. I interpret this result as further evidence that the reform caused a change in local employment, because classical urban models predict that changes in unemployment rates should be negatively correlated with changes in rents. That is, if a location becomes relatively more attractive, here through cheaper commuting costs and better employment outcomes, spatial equilibrium dictates that real estate prices must rise for utility to remain constant across space.

[11] Because I already control for a cubic polynomial of the distance to the center of Paris, I do not add an interaction of distance and treatment.

TABLE 11. Regression 3: Log price per square meter

	Dependent variable: Log price per square meter									
	Full Sample: zones 1-5									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
β	−0.044***	0.010	0.013**			−0.044***	0.010	0.013		
	(0.000)	(0.118)	(0.023)			(0.002)	(0.474)	(0.275)		
Placebo	0.016**	−0.014***				0.016	−0.014			
	(0.010)	(0.004)				(0.237)	(0.153)			
$\beta^{(3)}$				0.007	0.024***				0.007	0.024
				(0.345)	(0.002)				(0.668)	(0.147)
$\beta^{(4)}$				0.009	0.029***				0.009	0.029**
				(0.151)	(0.000)				(0.496)	(0.039)
$\beta^{(5)}$				0.026***	0.060***				0.026***	0.060***
				(0.000)	(0.000)				(0.002)	(0.000)
β_R					−0.016***					−0.016***
					(0.000)					(0.000)
Num.Obs.	664 715	664 715	664 715	664 715	664 715	664 715	664 715	664 715	664 715	664 715
R2	0.670	0.700	0.700	0.700	0.700	0.670	0.700	0.700	0.700	0.700
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X	X
COM trends		X	X	X	X		X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the DVF database.

Notes: Placebo is an indicator variable equal to 1 in the year before the reform. Controls includes a cubic polynomial for the distance to the center of Paris, the type of property (house or apartment) and the number of rooms. For columns 1-5, errors are clustered at the IRIS level. For columns 6-10, errors are clustered at the commune (i.e. municipality) level.

7. MODEL

I now introduce a spatial search-and-matching model to rationalize the empirical findings discussed in the previous sections. My focus is on explaining the finding of section 5, that FTZ reform led to a 0.60 percentage point increase in the share of employed workers commuting using public transport, a 0.25 percentage point decrease in the unemployment rate, and a 1.4% increase in the price of residential real estate. The findings of section 6 are more subtle, since they suggest the emergence of new employment sub-centers in the periphery and would require a more sophisticated approach, beyond the scope of this paper. The model is a simplified version of a Diamond-Mortensen-Pissarides (DMP) model (Pissarides (2000)), where the spatial dimension enters through the condition that utility should be equalized across space, as in Rosen (1979) and Roback (1982).

7.1. **Setting and value functions.** Time is discrete and infinite. There is a positive mass of ex-ante identical, risk-neutral and infinitely lived workers. Workers can live in two locations, denoted by the index $l \in \{l_{1,2}, l_3\}$ where $l_{1,2}$ represents living in fare zone 1-2, and l_3 represents living in fare zone 3. Every worker, whether employed or unemployed, consumes one unit of housing. All unemployed workers enjoy a flow of utility $b(l)$, capturing the value of home production and leisure, which depends on location to capture differences in amenities between cities, in the spirit of Brueckner, Thisse, and Zenou (1999). When employed, they receive a wage, which may depend on workers' residence, and they produce an output of value py. The variable p is a random variable with cdf G and pdf g. One can think of p as measuring the dispersion of worker productivity in the economy, or as an idiosyncratic match-specific productivity shock (the quality of a match between similar workers and firms). The value of p is unknown to workers until meeting with firms, when it is fully revealed. The variable y measures the value of the production good, which I choose to be the numeraire ($y = 1$).

At the beginning of each period, unemployed workers search for a job and can move at no cost from one location to another. Firms post vacancies at a per-period cost κ. The number of contacts between firms and workers is determined by a constant-returns-to-scale matching function in each local labor market $M(u(l), v(l))$, where $u(l)$ denotes the measure of unemployed workers and $v(l)$ denotes the total number of vacancies in the labor market l. Every period, firms meet with a worker with a probability $q(\theta(l)) = M(1, \theta(l))$, with $\theta(l) = \frac{v(l)}{u(l)}$ denoting local labor market tightness. The per-period probability that a worker meets with a given firm is $f(\theta(l)) = M(\theta(l)^{-1}, 1) = \theta(l)q(\theta(l))$. As is common in the literature, I assume that $q'(\theta(l)) < 0$ and $f'(\theta(l)) > 0$. If the worker is sufficiently productive (p above a certain level, as explained below), production begins after which the job is destroyed with an exogenous probability δ. Future flows of utility are discounted at the risk-free rate r.

I maintain the monocentric assumption, meaning that all jobs are located in the city center. However, I assume that there are two types of firms in the city center: one type only hires residents from the city center $l_{1,2}$, while the other type hires residents from the city periphery l_3. Assuming that the labor market is segmented in this way greatly simplifies the analysis, because it allows one to solve for the equilibrium without having to know the distribution of workers across space.[12] Employed workers commute to the city center every weekday to work, while unemployed workers travel to the city center for job interviews. For employees resident in l commuting costs are $c(l)$. The cost of going to occasional interviews for unemployed workers is denoted by $\mu c(l)$. Because job interviews are relatively infrequent, unemployed workers have lower travel costs than employed workers residing in the same location: $\mu \in [0, 1)$.[13] All residents in location l pay a rent $\gamma(l)$. The value function for an unemployed worker residing in location l, denoted by $B(l)$, is:

$$B(l) = b(l) - \mu c(l) - \gamma(l) + \frac{f(\theta(l))}{1+r} E_p\Big(\max\{W(l,p), B(l)\}\Big) + \frac{1 - f(\theta(l))}{1+r} B(l) \qquad (5)$$

where $W(l, p)$ is the value function of an employed worker residing in l, with productivity parameter p. I assume that unemployed workers have zero bargaining power, as in Postel-Vinay and Robin (2002). As a result, newly employed workers obtain a wage making them indifferent between accepting the job or remaining unemployed: $B(l) = W(l, p)$. This assumption simplifies the analysis, but is also empirically relevant in the present context. Cahuc, Postel-Vinay, and Robin (2006) estimate that the bargaining power of low-skilled workers is between 0 and 20% using French administrative data. So the present model is tailored to low skilled (income) workers, who are more likely to be affected by the FTZ reform. Using the zero-bargaining power assumption, equation (5) is:

$$B(l) = \frac{1+r}{r}\Big(b(l) - \mu c(l) - \gamma(l)\Big) \qquad (6)$$

[12] For a spatial search-and-matching model in which the distribution of unemployed workers across space matters, see for instance Marchiori, Pascal, and Pierrard (2022).

[13] Commuting costs include the monetary cost of the commuting (the cost of the travel pass), as well as the value of time spent commuting.

Assuming that employed workers pay only $\alpha \in [0,1]$ of commuting costs once they are employed, the rest being paid by the employer[14], the value function for an employed worker of type p residing in location l is:

$$W(l,p) = w(l,p) - \alpha c(l) - \gamma(l) + \frac{\delta}{1+r} B(l) + \frac{1-\delta}{1+r} W(l,p) \tag{7}$$

where $w(l,p)$ is the per-period wage paid to a worker of type p. Using the zero-bargaining power assumption to simplify leads to the following expression for wages:

$$w(l,p) = w(l) = b(l) + (\alpha - \mu)c(l) \tag{8}$$

Equation (8) states that employed workers are compensated for the value of home production and for the extra cost they incur by commuting to work in the city center.[15] Because workers have no bargaining power, the wage is independent of the idiosyncratic productivity variable p. However, the value of p alters the feasibility of jobs, as explained below. The value of a filled vacancy for a firm hiring workers resident in location l has to cover the value of output (p), minus the wage and the share of commuting costs paid by the employer:

$$V^E(l,p) = p - w(l) - (1-\alpha)c(l) + \frac{1-\delta}{1+r} V^E(l,p) \tag{9}$$

Using equation (8) and simplifying leads to:

$$V^E(l,p) = p - \tilde{w}(l) + \frac{1-\delta}{1+r} V^E(l,p) \tag{10}$$

with $\tilde{w}(l)$ denoting the wage that would be paid to a worker paying 100% of the commuting costs (no reimbursement by firms, which corresponds to $\alpha = 1$):

$$\tilde{w}(l) = b(l) + (1-\mu)c(l) \tag{11}$$

While firms prefer workers with a higher productivity parameter p, they will accept any worker generating positive per-period profits. Thus, equation (10) shows that the

[14] As explained in section 3, French firms in the Parisian metropolitan area are required by law to reimburse at least 50% of the Navigo Travel Card (NTC) paid by their employees. More than 95% of firms reimburse exactly 50% of the NTC (STIF, 2010). This situation corresponds to $\alpha = 0.5$.

[15] For employed workers to be compensated for their additional commuting costs, it must be that $\alpha > \mu$, which is verified empirically, as explained below.

optimal strategy for a firm is to hire workers with $p \geq \tilde{w}(l)$ and to reject workers otherwise.

Because the wage $\tilde{w}(l)$ is independent of α, a corollary is that *job feasibility does not depend on the share of commuting costs reimbursed by employers.* On the one hand, any increase in the share of commuting costs paid by workers will have to be compensated by a corresponding increase in the wage. On the other hand, any increase in the share of commuting costs paid by firms is an extra cost for them, which decreases the wage paid to workers by the same amount. With the assumption of linear utility (risk neutrality) and zero bargaining power for workers, the two effects cancel out and the distribution of commuting costs between employed workers and firms does not matter for the quantity of jobs created.

For a firm, the value of an unfilled vacancy must reflect the per-period cost κ, as well the expected value of a filled vacancy:

$$V^U(l) = -\kappa + \frac{1}{1+r} q(\theta(l)) \Big(1 - G\big(\tilde{w}(l)\big)\Big) E_p \Big[V^E(l,p) | p \geq \tilde{w}(l)\Big] \tag{12}$$

The probability of meeting a worker in period t is given by $q(\theta(l))$, but the vacancy is only filled if $p \geq \tilde{w}(l)$, which happens with probability $1 - G\big(\tilde{w}(l)\big)$. If we assume free entry for firms, at equilibrium the value of an unfilled vacancy converges to zero $V^U(l) = 0$. Combining the free-entry condition with equations (10) and (12) leads to the following job creation condition (13):

$$E[p | p \geq \tilde{w}(l)] - \tilde{w}(l) - \frac{(r+\delta)\kappa}{q(\theta(l))\Big(1 - G\big(\tilde{w}(l)\big)\Big)} = 0 \tag{13}$$

To close the model, note that the change in the number of unemployed workers in location l will reflect the inflow from exogenous job destruction and the outflow from successful job matches:

$$u_{t+1}(l) - u_t(l) = \delta(1 - u_t(l)) - \theta_t(l) q(\theta_t(l)) \Big(1 - G\big(\tilde{w}(l)\big)\Big) u_t(l) \tag{14}$$

At equilibrium, both the number of unemployed workers and the number of vacancies in location l are constant, which yields the Beveridge curve equation:

$$u(l) = \frac{\delta}{\delta + \theta(l)q(\theta(l))\Big(1 - G\big(\tilde{w}(l)\big)\Big)} \tag{15}$$

7.2. **Equilibrium.** An equilibrium consists in two quadruplets $\Big(\tilde{w}(l), \theta(l), u(l), \gamma(l)\Big)_{l\in\{l_{1,2},l_3\}}$ jointly satisfying the reservation wage equation (11), the job creation condition (13), the Beveridge curve equation (15), such that utility is equalized across space $B(l_{1,2}) = B(l_3)$ with $B(l)$ solving equation (6).

There exists a unique equilibrium. Equation (11) uniquely pins down the reservation wage $\tilde{w}(l)$. For a fixed reservation wage $\tilde{w}(l)$, the job creation condition (13) uniquely determines labor market tightness $\theta(l)$. Having found the labor market tightness, one can determine the equilibrium level of unemployment $u(l)$ using the Beveridge curve equation (15). Normalizing one of the two rents (for instance $\gamma(l_{1,2})$), the value of the other rent adjusts so that the utility across space is constant according to equation (6).

7.3. **Calibration.** I now calibrate the model to reproduce key features of the Paris region. I set the time period to one month and use a yearly discount factor of 4%, which implies a monthly discount rate of $r = 0.33\%$. I use a Cobb-Douglas matching function $M(u(l), v(l)) = \chi u(l)^{\eta} v(l)^{\eta-1}$. Cahuc, Carcillo, and Le Barbanchon (2019) find that the value of home production is 94% the value of output based on French administrative data. I set the value of home production in zone 1-2 to be 90% the value of output ($b(l_{1,2}) = 0.9y = 0.9$). Using the density of amenities across space (such as parks, theaters, monuments), there are approximately half as many amenities in zone 3 as in zones 1-2 (see Marchiori, Pascal, and Pierrard (2022)). To the best of my knowledge, there are no empirical studies quantifying the contribution of amenities and unemployment benefits to the total value of home production. Assuming that 25% of the value of home production derives from leisure and the rest comes from unemployment benefits, the latter being constant across space, one finds that $b(l_3) = 0.825$.

More than 95% of firms reimburse exactly 50% of commuting costs incurred by their workers (STIF, 2010). Thus, I set $\alpha = 0.5$. For the relative frequency at which unemployed workers commute to the center of Paris, I use $\mu = 1/5$, as in Wasmer and Zenou (2002). Regarding commuting costs $c(l)$, Table 1 shows that the average commuting time for a worker in fare zone 3 is 40 minutes. Assuming one return trip a day, 230 days worked in a year, and valuing time spent commuting at the net minimum wage in 2015 (7.53 euros per hour), time spent commuting for a worker residing in zone 3 is approximately 192 euros per month. With the FTZ reform, the cost of the monthly travel card went from 89.20 to 70 euros. Summing the value of time and the cost of the travel card, the reform led to a 6.8% decline in total commuting costs for workers living in fare zone 3. Using the same methodology, total monthly commuting cost in $l_{1,2}$ is approximately equal to $115 + 70 = 185$ euros.

According to INSEE, in 2015 the median net monthly wage was approximately 1800 euros for a full-time worker, so the wage-to-commuting-costs ratio was approximately $\frac{1800}{185} \approx 9.72$. I use equation (8) and the empirical value for this ratio to find $c(l_{1,2}) = 0.0956$. Using previously calculated values for commuting costs in zone 3, I find that $c(l_3) = 0.1453$ before the reform, and $c(l_3) = 0.1354$ after the reform. The rent-to-earnings ratio is approximately 37% in the city of Paris. I fix the value $\gamma(l_{1,2})$ to match this ratio, which yields $\gamma(l_{1,2}) = \frac{37}{100} w(l_{1,2}) = 0.3436$. The values $\gamma(l_3)$ before and after the reform are calculated such that utility is constant across space. Regarding the distribution of the idiosyncratic productivity parameter p, I use a truncated normal distribution with mean $\mu_p = 0$ and standard deviation $\sigma_p = 1$. I set the monthly job destruction rate to $\delta = 1.2\%$, which corresponds to the value calculated by Hairault, Le Barbanchon, and Sopraseuth (2015) using French labor force surveys.

I jointly calibrate the three remaining parameters: the matching efficiency parameter χ, the elasticity of the matching function with respect to the measure of unemployed workers η and the cost of posting vacancies κ. I choose these parameters in order to reproduce the monthly job finding rate of $f = 7.5\%$ estimated by Hairault, Le Barbanchon, and Sopraseuth (2015), the monthly vacancy filling rate of $q = 1/2$ estimated by Cahuc, Carcillo, and Le Barbanchon (2019), and the unemployment rate in location $l_{1,2}$ as of January 2015. Calibrated parameters are presented in Table 12.

TABLE 12. Calibrated parameters

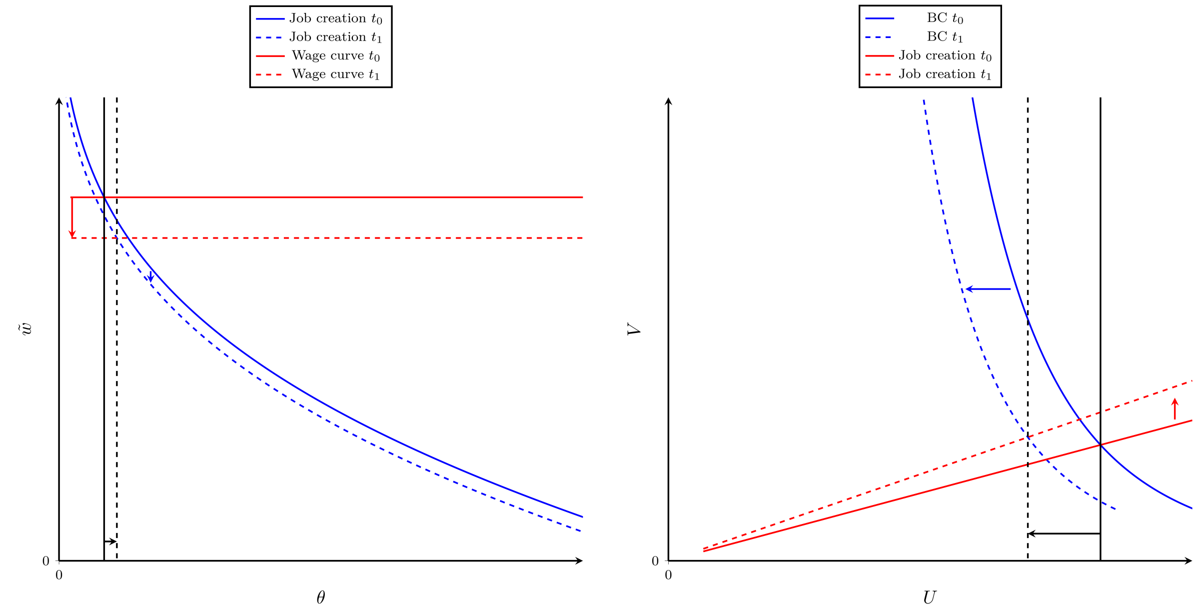
Symbol	y	r	δ	α	μ	$b(l_{1,2})$	$b(l_3)$	$c(l_{1,2})$	$c_{t_0}(l_3)$	$c_{t_1}(l_3)$	$\gamma(l_{1,2})$	$\gamma_{t_0}(l_3)$	$\gamma_{t_1}(l_3)$	μ_p	σ_p	χ	κ	η
Value	1	0.003	0.012	1/2	1/5	0.9	0.825	0.0956	0.0.1453	0.1354	0.3436	0.2586	0.2606	0	1	0.7297	17.3446	0.7740

Notes: The index t_0 refers to the pre-reform equilibrium, while the the index t_1 refers to the post-reform equilibrium. If the index is omitted, it means that the value is not affected by the reform.

To gain insights on the FTZ reform, note that the different mechanisms can be represented visually (see Figure 4). A decline in the commuting costs in l_3 leads to a drop in the reservation wage $\tilde{w}(l_3)$: in the $\tilde{w}$-θ space, the wage curve shifts down. Simultaneously, the job creation curve shifts down, because more low productivity jobs are feasible, which decreases the expected idiosyncratic productivity of workers $E[p|p \geq \tilde{w}(l_3)]$. In the calibrated model, the downward shift of the reservation wage curve dominates, which raises labor market tightness θ. In the vacancy-unemployment space, the reduction in waiting time to find an employable worker rotates the job creation condition anti-clockwise. The job finding rate for workers also increases, which shifts the Beveridge curve to the left. As a result, the unemployment rate in l_3 decreases. Because travel is cheaper for unemployed workers, the value of unemployment goes up in l_3. However, workers in $l_{1,2}$ are unaffected by the FTZ reform. To restore the spatial equilibrium condition ($B(l_3) = B(l_{1,2})$), rents in l_3 must rise until they exactly offset the savings in commuting costs: $\Delta\mu c(l) = \Delta\gamma(l)$.

Table 13 compares the model to the data. The fit is very food for the vacancy filling rate, the job finding rate and the unemployment rate in $l_{1,2}$. However, the unemployment rate in the model is slightly higher in $l_{1,2}$ than in l_3. This reflects the higher level of amenities, which pushes up the reservation wage. The 6.8% decline in total commuting costs in l_3 generates a 0.172 percentage point decline in the unemployment rate in l_3 and a 0.76% increase in rents. Despite its simplicity, the calibrated model is able to explain approximately 68.9 % and 54.7 % of the change in unemployment and rents estimated from the empirical section. The rest could be explained by channels that are not present in the model, such as the creation of new employment sub-centers in the post-reform period, as suggested by the empirical results of section 6, and by the work of Garcia-López, Hémet, and Viladecans-Marsal (2017). Alternatively, the calibration may overestimate the monetary value of time spent commuting, underestimating the savings generated by the reform. An alternative parametrization in which each hour commuting is valued at 4.08 euros (54.1% of the net minimum wage) leads to a 9.92% decrease in commuting costs, which in turn implies a 0.25 percentage point decrease in the unemployment rate and a 1.11 % increase in rents. With this alternative parametrization, the model explains 100% of the change in unemployment (by construction) and 79.2% of the increase in rents.

Figure 4. Model



Notes: This figure shows the FTZ reform in the reservation wage-labor market tightness space ($\tilde{w}$-θ) and in the vacancy-unemployment space ($V - U$). The FTZ reform consists in a 6.8% decline in commuting costs for workers living in zone 3. The job creation condition refers to equation (13). The reservation wage curve curve is given by (11). The Beveridge curve refers to equation (15).

Table 13. Model and data

Variable	f	q	$u(l_{1,2})$	$u_{t_0}(l_3)$	$\Delta u(l_3)$	% $\Delta r(l_3)$	$w(l_{1,2})$	$w_{t_0}(l_3)$	$w_{t_1}(l_3)$	$w(l_{1,2})$	$w_{t_0}(l_3)$	$w_{t_1}(l_3)$	$E[p\|p \geq \tilde{w}(l_{1,2})]$	$E_{t_0}[p\|p \geq \tilde{w}(l_3)]$	$E_{t_1}[p\|p \geq \tilde{w}(l_3)]$
Model	0.079	0.5	0.131	0.123	-0.172	0.76%	0.928	0.868	0.865	0.976	0.941	0.933	1.506	1.478	1.471
Data	0.075	0.5	0.124	0.134	-0.25	1.4%	-	-	-	-	-	-	-	-	-

Notes: This table shows key values for the calibrated model, as well as changes in value caused by the FTZ reform, which leads to a 6.8% decline in commuting costs for workers living in zone 3. The index t_0 refers to the pre-reform equilibrium, while the index t_1 refers to the post-reform equilibrium. The index is omitted where the value is not affected by the reform. $\Delta u(l_3)$ refers to the percentage point change in the unemployment rate in l_3, while % $\Delta r(l_3)$ refers to the percentage change in rents in l_3.

8. Conclusion

The introduction of the public transport travel pass "Forfait Toutes Zones" in the Paris metropolitan area provides a quasi-natural experiment to measure how a decrease in commuting costs affects local employment dynamics. The cost of using public transport decreased in the periphery (zones 3-5), while it remained constant for central Paris and the inner suburbs (zones 1-2). Comparing the periphery to central Paris and the inner suburbs, the FTZ reform led to 0.60 percentage point increase in the share of employed workers commuting by public transport, a 0.25 percentage point decrease in the unemployment rate, and a 1.4% increase in the price of residential real estate. I show that these empirical findings can be explained by a standard calibrated search-and-matching model. Despite its simplicity, the calibrated model can explain a large fraction of the estimated effects of the reform. According to the model, the FTZ reform increases local employment by reducing the reservation wage of workers. Spatial equilibrium requires that utility is constant across space, which causes the price of residential real estate to appreciate.

When extending the empirical analysis to include geographical units further away from Paris (zones 4-5), I find that the reform also boosted local employment and residential real estate prices in these areas. For geographical units in zones 4 and 5, the empirical evidence suggests that the positive local employment effects may have been driven by the emergence of new employment sub-centers, as in Mayer and Trevien (2017) and Garcia-López, Hémet, and Viladecans-Marsal (2017). Further research could relax the monocentric assumption to study the joint distribution of firms and workers across space. Adapting this model to the Luxembourg setting would also require considering the large share of cross-border workers, who have the additional outside option of working in their home country, as in Marchiori, Pascal, and Pierrard (2022). Additional research could also analyze the welfare effects of the FTZ reform. Given that it was financed through taxation, one may wonder whether the distortions created by additional taxes offset the estimated gains. A thorough welfare analysis should also allow for the potential reduction in CO_2 equivalent and carbon monoxide following the reform.

References

Anne, D., and Y. L'Horty (2009): "Aides sociales locales, revenu de Solidarité active (RSA) et gains du retour à l'emploi," *Économie et Statistique*, 429(1), 129–157.

Baum-Snow, N., and M. E. Kahn (2000): "The effects of new public projects to expand urban rail transit," *Journal of Public Economics*, 77(2), 241–263.

Brueckner, J. K., J.-F. Thisse, and Y. Zenou (1999): "Why is central Paris rich and downtown Detroit poor?: An amenity-based theory," *European Economic Review*, 43(1), 91–107.

Cahuc, P., S. Carcillo, and T. Le Barbanchon (2019): "The effectiveness of hiring credits," *The Review of Economic Studies*, 86(2), 593–626.

Cahuc, P., F. Postel-Vinay, and J.-M. Robin (2006): "Wage bargaining with on-the-job search: Theory and evidence," *Econometrica*, 74(2), 323–364.

Card, D., and A. B. Krueger (1994): "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *The American Economic Review*, 84(4), 772–793.

Chapelle, G., E. Wasmer, and P. H. Bono (2020): "An urban labor market with frictional housing markets: Theory and an application to the Paris urban area," *Journal of Economic Geography*.

Chen, Y., and A. Whalley (2012): "Green infrastructure: The effects of urban rail transit on air quality," *American Economic Journal: Economic Policy*, 4(1), 58–97.

Duranton, G., and M. A. Turner (2012): "Urban growth and transportation," *Review of Economic Studies*, 79(4), 1407–1440.

Einiö, E., and H. Overman (2016): "The (Displacement) Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI," *C.E.P.R. Discussion Papers*, (11112).

Garcia-López, M.-À., C. Hémet, and E. Viladecans-Marsal (2017): "Next train to the polycentric city: The effect of railroads on subcenter formation," *Regional Science and Urban Economics*, 67, 50–63.

Hairault, J.-O., T. Le Barbanchon, and T. Sopraseuth (2015): "The cyclicality of the separation and job finding rates in France," *European Economic Review*, 76, 60–84.

Hilber, C. A., F. Carozzi, and Y. Xiaolun (2020): "On the Economic Impacts of Mortgage Credit Expansion Policies: Evidence from Help to Buy," *CEPR Discussion Paper*, (1681).

Kline, P., and E. Moretti (2013): "Local economic development, agglomeration economies, and the big push: 100 years of evidence from the Tennessee Valley Authority," *The Quarterly Journal of Economics*, 129(1), 275–331.

Kuhn, M., I. Manovskii, and X. Qiu (2021): "The Geography of Job Creation and Job Destruction," Discussion paper, National Bureau of Economic Research.

Marchiori, L., J. Pascal, and O. Pierrard (2022): "(In)efficient commuting and migration choices: theory and policy in an urban search model," *BCL Working Papers*, (159).

Mayer, T., and C. Trevien (2017): "The impact of urban public transportation evidence from the Paris region," *Journal of Urban Economics*, 102, 1–21.

Neumark, D., and H. Simpson (2015): "Place-based policies," in *Handbook of Regional and Urban Economics*, vol. 5, pp. 1197–1287. Elsevier.

OMNIL (2018): "Note Repères 10 Les Forfaits Toutes Zones, quels impacts deux ans après leur mise en place ?," *Online, accessed 3 June 2022,* https://www.omnil.fr/IMG/pdf/note_reperes_n10_tarif_unique_vf.pdf.

Pascal, J. (2020): "Search, matching and heterogeneity," Ph.D. thesis, Institut d'études politiques de Paris - Sciences Po.

Pissarides, C. A. (2000): *Equilibrium Unemployment Theory*. MIT press.

Postel-Vinay, F., and J.-M. Robin (2002): "Equilibrium wage dispersion with worker and employer heterogeneity," *Econometrica*, 70(6), 2295–2350.

Redding, S. J., and M. A. Turner (2015): "Transportation costs and the spatial organization of economic activity," *Handbook of Regional and Urban Economics*, 5, 1339–1398.

Roback, J. (1982): "Wages, rents, and the quality of life," *Journal of Political Economy*, 90(6), 1257–1278.

Rosen, S. (1979): "Wage-based indexes of urban quality of life," *Current Issues in Urban Economics*, pp. 74–104.

STIF (2010): "Le remboursement des titres de transport par les employeurs en Ile-de-France," *Online, accessed 3 June 2022,* https://www.iledefrance-mobilites.fr/medias/portail-idfm/2eeac66d-fc06-469c-95f1-12bb2f6f32c7_ae1_2012_10_18_article_remboursement_employeur_vf.pdf.

Wasmer, E., and Y. Zenou (2002): "Does city structure affect job search and welfare?," *Journal of Urban Economics*, 51(3), 515–541.

Appendix A. Pôle Emploi categories

Workers registered to the French unemployment agency (Pôle Emploi) are assigned to one of the 5 existing categories (A-E):[16]

- category A includes jobless jobseekers obliged to actively seek a job
- category B includes jobseekers having performed a short-term reduced activity and obliged to actively seek a job (i.e. 78 hours or less in the course of the month)
- category C includes jobseekers having performed a long-term reduced activity and obliged to actively seek a job (i.e. more than 78 hours in the course of the month)
- category D includes jobless jobseekers not obliged to actively seek a job (because of an internship, a training course, an illness, etc) Including the job-seekers in agreement of personalised reclassifying (CRP), in contract of professional transition (CTP) and in professional safeguard contract (CSP)
- category E includes employed jobseekers not obliged to actively seek a job (for example: beneficiaries of subsidised contracts)

Appendix B. Robustness checks

B.1. **Buffer area in the treatment group.** This sub-section presents the regressions from section 5, where the observations from the treatment group (zone 3) that are close to the border separating the fare zones 2 and 3 have been removed. I use a threshold distance to the border of 500 m.

[16] Source: https://www.insee.fr/en/metadonnees/definition/c2010

TABLE 14. Regression 1 with buffer in zone 3: public transport usage

	Dependent variable: Share of employed workers using public transport													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	1.06***	0.74***	0.70***	0.67***	0.56**	0.49*	0.60*	1.06***	0.74***	0.70**	0.67**	0.56*	0.49	0.60*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)	(0.05)	(0.06)	(0.00)	(0.01)	(0.01)	(0.02)	(0.07)	(0.12)	(0.10)
Placebo	0.22	0.07						0.22	0.07					
	(0.13)	(0.46)						(0.25)	(0.57)					
Num.Obs.	21 055	21 055	21 055	19 704	17 328	13 596	8988	21 055	21 055	21 055	19 704	17 328	13 596	8988
R2	0.941	0.970	0.970	0.970	0.971	0.968	0.955	0.941	0.970	0.970	0.970	0.971	0.968	0.955
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X
Buffer zone 3	X	X	X	X	X	X	X	X	X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 includes all geographical units in zones 1-3, excluding observations from the treatment group (zone 3) that are at a distance of less than 500 meters from the border separating zones 2 and 3. Columns 4-3 and 11-14 further restrict the sample to units within a certain distance of the border between zones 2 and 3.

Table 15. Regression 1 with buffer in zone 3: unemployment rate

	Dependent variable: Unemployment Rate													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.59***	−0.14	−0.20	−0.17	−0.12	−0.17	−0.06	0.59***	−0.14	−0.20	−0.17	−0.12	−0.17	−0.06
	(0.00)	(0.25)	(0.11)	(0.19)	(0.37)	(0.28)	(0.77)	(0.00)	(0.38)	(0.23)	(0.33)	(0.49)	(0.41)	(0.82)
Placebo	0.47***	0.13**						0.47***	0.13**					
	(0.00)	(0.01)						(0.00)	(0.02)					
Num.Obs.	21 055	21 055	21 055	19 704	17 328	13 596	8988	21 055	21 055	21 055	19 704	17 328	13 596	8988
R2	0.889	0.944	0.944	0.945	0.947	0.950	0.953	0.889	0.944	0.944	0.945	0.947	0.950	0.953
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X
Buffer zone 3	X	X	X	X	X	X	X	X	X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 includes all geographical units in zones 1-3, excluding observations from the treatment group (zone 3) that are at a distance of less than 500 meters from the border separating zones 2 and 3. Columns 4-3 and 11-14 further restrict the sample to units within a certain distance of the border between zones 2 and 3.

Table 16. Regression 1 with buffer in zone 3: Log price per square meter

	Dependent variable: Log price per square meter													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	−0.040***	0.010	0.009	0.011	0.012	0.013	0.021	−0.040	0.010	0.009	0.011	0.012	0.013	0.021
	(0.000)	(0.320)	(0.337)	(0.258)	(0.210)	(0.229)	(0.108)	(0.116)	(0.660)	(0.663)	(0.608)	(0.569)	(0.597)	(0.473)
Placebo	0.012	0.007						0.012	0.007					
	(0.277)	(0.362)						(0.637)	(0.669)					
Num.Obs.	304 421	304 421	304 421	283 293	247 638	186 085	119 898	304 421	304 421	304 421	283 293	247 638	186 085	119 898
R2	0.509	0.539	0.539	0.543	0.546	0.540	0.516	0.509	0.539	0.539	0.543	0.546	0.540	0.516
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X		X	X	X	X	X	X
Buffer zone 3	X	X	X	X	X	X	X	X	X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's calculations based on data from the DVF database.

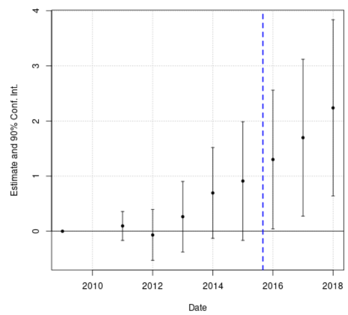
Notes: Placebo is an indicator variable equal to 1 in the year before the reform. Controls includes a cubic polynomial for the distance to the center of Paris, the type of property (house or apartment) and the number of rooms. For columns 1-7, errors are clustered at the IRIS level. For columns 8-14, errors are clustered at the commune (i.e. municipality) level. Columns 1-3 and 9-11 includes all geographical units in zones 1-3, excluding observations from the treatment group (zone 3) that are at a distance of less than 500 meters from the border separating zones 2 and 3. Columns 4-3 and 11-14 further restrict the sample to units within a certain distance of the border between zones 2 and 3.

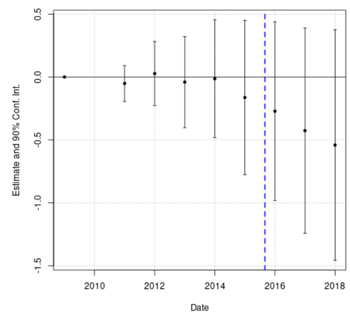
B.2. **Granger causality.** Conditional on the year and IRIS fixed effects and after removing differences in pre-trends, the treatment should have no impact on the treated IRIS before the reform. To quantify the dynamic impact of the reform, I estimate a year-dependent model, were the dummy $\delta_{i,t}$ is interacted with a year-specific dummy δ_y equal to 1 when the underlying year is equal to y:

$$y_{it} = \alpha_i + \gamma_t + \sum_{y=2011}^{2018} \beta_y \times \delta_{i,t} \times \delta_y + \eta_i \times t + \varepsilon_{i,t} \tag{16}$$

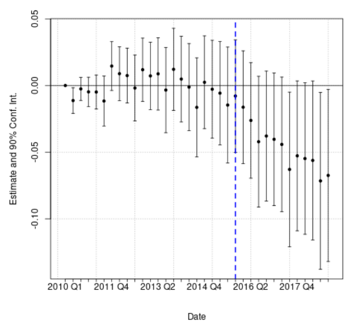
The parameter β_y measures the average treatment effect during year y. Results are presented visually in Figure 5. Overall, the 90% confidence intervals around the estimated values show no clear signal that the treatment has an impact on the treated before the treatment. The percentage of workers commuting by public transport seem to experience a small positive pre-trend, but the it is not significant at the 90% confidence level. I also estimate a variation of regression where the dependent variable is the (log) number of workers registered to the French unemployment agency (Pole Emploi). While the data is available at monthly frequency in this case, I estimate quarterly effects to smooth out volatility in the estimates. I weight observations by the share of total population that each municipality represents in the sample. As for the model with yearly observations, I drop the first two observations of the sample (2010-Q1 and 2010-Q2), which then constitute the reference periods for the estimation. As illustrated panel C of Figure 5, no significant pre-trend emerges for this alternative measurement of local unemployment. A negative trends emerges only in the post-treatment period.

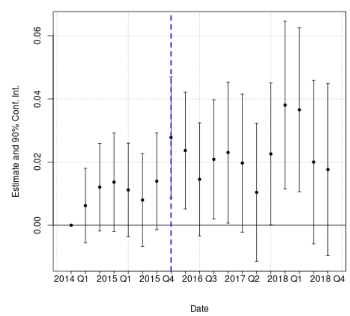
FIGURE 5. Granger causality checks





(a) % workers commuting using public transport (b) Unemployment rate





(c) Log number of workers registered to Pole Emploi (A) (d) Log price per square meter

Sources: Author's calculations based on data from the INSEE, Pole Emploi and DVF.
Notes: Vertical bars represent 90% confidence intervals based on standard errors clustered at the IRIS level for panel A, B and D. For panel C, errors are clustered at the municipality level. The vertical blue line indicate the implementation of the reform (September 2015). The control group is constituted of units in zones 1 and 2, while the treatment group is constituted of units in zone 3.

B.3. **Impact on population.** This sub-section uses the regression framework from section 5, where the dependent variable is the logarithm of the population between 15 and 64 within an IRIS. Table 17 reports a 0.9% increase in population in the treatment group (see the third column). Restricting the sample to observations within a certain distance to the border (5km to 2km) results in a coefficient that ranges between 1.0% and 0.5% (columns 4-7). Table 18 show the results from the same regression, with the additional restriction that the observations from the treatment group (zone 3) that are close to the border separating the fare zones 2 and 3 have been removed. Table 18 indicates that the reform caused a 1.0-0.5% increase in population in the treatment group.

Table 17. Regression 1: Log population 15-64

	Dependent variable: Log population 15-64													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.031***	0.008*	0.009*	0.010*	0.006	0.008	0.006	0.031***	0.008	0.009*	0.010*	0.006	0.008	0.006
	(0.000)	(0.088)	(0.059)	(0.052)	(0.293)	(0.201)	(0.387)	(0.000)	(0.126)	(0.089)	(0.079)	(0.402)	(0.262)	(0.391)
Placebo	0.008**	−0.003						0.008*	−0.003					
	(0.035)	(0.241)						(0.090)	(0.198)					
Num.Obs.	23 006	23 006	23 006	21 655	19 279	15 547	10 939	23 006	23 006	23 006	21 655	19 279	15 547	10 939
R2	0.958	0.983	0.983	0.981	0.978	0.977	0.977	0.958	0.983	0.983	0.981	0.978	0.977	0.977
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 includes all geographical units in zones 1-3. Columns 4-3 and 11-14 restrict the sample to units within a certain distance of the border between zones 2 and 3.

Table 18. Regression 1 with buffer in zone 3: Log population 15-64

	Dependent variable: Log population 15-64													
	Full Sample: zones 1-3			Restricted Sample: zones 1-3				Full Sample: zones 1-3			Restricted Sample: zones 1-3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
β	0.033***	0.009*	0.010*	0.010*	0.006	0.008	0.005	0.033***	0.009	0.010*	0.010*	0.006	0.008	0.005
	(0.000)	(0.083)	(0.073)	(0.064)	(0.302)	(0.209)	(0.562)	(0.000)	(0.113)	(0.097)	(0.084)	(0.392)	(0.259)	(0.575)
Placebo	0.009**	−0.002						0.009**	−0.002					
	(0.014)	(0.505)						(0.041)	(0.399)					
Num.Obs.	21 055	21 055	21 055	19 704	17 328	13 596	8988	21 055	21 055	21 055	19 704	17 328	13 596	8988
R2	0.959	0.983	0.983	0.982	0.978	0.977	0.977	0.959	0.983	0.983	0.982	0.978	0.977	0.977
Max Dist. Border (km)				5	4	3	2				5	4	3	2
Cluster SE	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	IRIS	COM	COM	COM	COM	COM	COM	COM
IRIS FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IRIS trends		X	X	X	X	X	X		X	X	X	X	X	X
Buffer zone 3	X	X	X	X	X	X	X	X	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from the INSEE.

Notes: Standard errors are clustered at the IRIS level (columns 1-7), or at the commune level (columns 8-14). Placebo is an indicator variable equal to 1 in the year before the reform. Columns 1-3 and 9-11 includes all geographical units in zones 1-3, excluding observations from the treatment group (zone 3) that are at a distance of less than 500 meters from the border separating zones 2 and 3. Columns 4-3 and 11-14 further restrict the sample to units within a certain distance of the border between zones 2 and 3.

Appendix C. Robustness checks with heterogeneous treatment

Table 19. Regression 3: Registered unemployment (Pôle Emploi, all categories)

	Dependent variable: Log number of workers registered at Pôle Emploi						
	Full Sample: zones 1-5						
	All categories						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β	0.077***	−0.003	−0.007*				
	(0.000)	(0.332)	(0.080)				
Placebo	0.040***	0.006***					
	(0.000)	(0.003)					
$\beta^{(3)}$				−0.016***	−0.015***	−0.013**	−0.013**
				(0.003)	(0.007)	(0.038)	(0.038)
$\beta^{(4)}$				−0.006	−0.005	0.000	0.000
				(0.217)	(0.323)	(0.989)	(0.991)
$\beta^{(5)}$				−0.002	−0.001	0.010	0.010
				(0.651)	(0.910)	(0.305)	(0.302)
β_R					−0.001		0.000
					(0.568)		(0.972)
β_d						0.000	0.000
						(0.136)	(0.183)
Num.Obs.	100 932	100 932	100 932	100 932	100 932	100 932	100 932
R2	0.999	1.000	1.000	1.000	1.000	1.000	1.000
Cluster SE	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X
date FE	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from Pôle Emploi.

Notes: Standard errors are clustered at the commune level (COM). The dependent variable includes all workers registered at Pôle Emploi (categories A - E). Placebo is an indicator variable equal to 1 in the year before the reform.

Table 20. Regression 3: Registered unemployment (Pôle Emploi, all except category A)

	Dependent variable: Log number of workers registered at Pôle Emploi						
	Full Sample: zones 1-5						
	All except category A						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β	0.098***	0.012*	0.008				
	(0.000)	(0.084)	(0.268)				
Placebo	0.044***	0.008**					
	(0.000)	(0.022)					
$\beta^{(3)}$				0.013	0.015*	0.015*	0.016*
				(0.127)	(0.081)	(0.095)	(0.076)
$\beta^{(4)}$				0.012	0.015*	0.015	0.016*
				(0.156)	(0.089)	(0.115)	(0.098)
$\beta^{(5)}$				0.001	0.006	0.009	0.009
				(0.878)	(0.481)	(0.516)	(0.496)
β_R					−0.002		−0.002
					(0.221)		(0.318)
β_d						0.000	0.000
						(0.477)	(0.754)
Num.Obs.	100 932	100 932	100 932	100 932	100 932	100 932	100 932
R2	0.997	0.999	0.999	0.999	0.999	0.999	0.999
Cluster SE	COM	COM	COM	COM	COM	COM	COM
COM FE	X	X	X	X	X	X	X
date FE	X	X	X	X	X	X	X
COM trends		X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sources: Author's calculations based on data from Pôle Emploi.

Notes: Standard errors are clustered at the commune level (COM). The dependent variable includes all workers registered at Pôle Emploi, except those in category A.

BANQUE CENTRALE DU LUXEMBOURG

EUROSYSTÈME

2, boulevard Royal
L-2983 Luxembourg

Tél.: +352 4774-1
Fax: +352 4774 4910

www.bcl.lu • info@bcl.lu