Rival Guests or Defiant Hosts? The Local Economic Impact of Hosting Refugees *

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Abstract

This paper investigates the local economic cost of hosting refugees. Using administrative data in France, we show that the opening of small housing centers for refugees decreases economic activity in hosting municipalities. We demonstrate that this downturn is related to a decline in the population by around two percent due to fewer people moving into hosting municipalities. This avoidance behavior of natives results from prejudices since refugee inflows are not sufficient to generate labor market shocks. We also estimate the aggregate cost of hosting refugees.

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1 Introduction

More than 1,300,000 asylum applications were submitted in Europe in 2015, while less than 200,000 applications were received 30 years earlier. Since the collapse of the Communist bloc in the late 1980s, the rise of refugee inflows to Europe has challenged the existing policies and accommodation schemes. In this context, two phenomena could trigger popular resentment. First, natives may fear the potential economic weight and the cultural threat of a growing number of humanitarian migrants. Second, the spatial concentration of refugees could raise fairness issues for communities who are more exposed. To avoid public discontent, most European governments have tried to "spread" the burden of refugee hospitality (Robinson et al., 2003). One rationale behind this approach is that anti-refugee feelings, native defiance, stem from the competition between refugees and natives over the sharing of scarce resources. We call this competition refugee rivalry. By making the presence of refugees negligible at the local level, refugee dispersal policies reduce the threat they could pose to natives and render hospitality more acceptable. But such policies can also come at a cost. First, they can isolate refugees from more dynamic labor markets and their ethnic networks, potentially adversely affecting their chances of integration (Edin et al., 2004; Clemens et al., 2018; Fasani et al., 2018; Martén et al., 2019). Second, if anti-refugee sentiments are driven not only by fears of competition but also by cultural insecurity or xenophobia, they might not dispel local discontent.

In this paper, we answer the following question: what is the local economic impact of refugee accommodation policies? We focus on how refugee dispersal might dilute the impact of redhumanitarian migrants – asylum seekers, refugees, and beneficiaries of the subsidiary protection – inflows. On the one hand, dispersal may be enough if the impact is driven by refugee rivalry. For instance, if refugees compete with natives over scarce resources, such as jobs, housings, public goods, or even public order. On the other hand, it may not be sufficient if the economic impact is due to native defiance because of cultural insecurity or xenophobia. In particular, we could expect self-segregation as a reaction to refugee inflows, either because of native flight, with natives leaving the area when refugees arrive, or because of native avoidance, with natives avoiding to move to places with refugees. Compared to studies of large refugee shocks (Card, 1990; Hunt, 1992; Balkan et al., 2018), considering refugee dispersal policies is interesting because they are more representative of typical patterns of refugee-hosting (Peri, 2016). Moreover, it allows us to discriminate

between native defiance and refugee rivalry.

We exploit the opening of refugee centers in ninety-eight municipalities in France between 2004 and 2012 to identify the effect of small humanitarian migrant inflows on local communities. We compare the evolution of the population, and other outcomes, in hosting and non-hosting neighboring municipalities up to two years before and after the opening of a refugee center. We focus on the evolution of the local population because it is a relevant indicator of local shocks and shifts in preferences (Tiebout, 1956; Hsieh and Moretti, 2019). In France, the refugee center opening process is centralized, leaving municipalities with very little discretion over the opening. Asylum seekers, subsidiary protection recipients, and refugees do not choose centers they are assigned to. The opening of a refugee center is thus a good experiment to study what happens when a locality is exposed to refugees. To circumvent the lack of precise and regular population census data, we use two population proxies, which are the number of employed residents and the number of fiscal households. The first measure enables us to provide a monthly estimate of population change and the second one allows us to confirm the validity of our results for the non-employed.

We find that, on average, the number of employed residents and fiscal households in hosting municipalities decreases by up to two percent two years after the opening of the refugee center. This effect persists when we account for potential spillovers. This finding also holds when looking at the number of retiree households. We also show that wealthier households react more to the openings. Since the median size of a refugee center relative to the local workforce is 0.3%, this implies that there are three to six fewer people in the municipality for one refugee place in the center. This decrease in population is explained by a reduction in native inflows to hosting municipalities. Finally, for a subsample of openings, we find that the population stops decreasing after three years.

Negative valuation by prospective newcomers of the presence of a refugee center, that is native avoidance, drives the local costs of hosting refugees. Over time, lower inflows of natives to hosting municipalities mean fewer customers and taxpayers. We find that tax collected by hosting municipalities falls by around three percent after opening a refugee center. The economic activity of firms located in the hosting municipalities is also decreasing: their number is declining by 3 percent and the value of their sales by more than 5 percent.

We use a spatial general equilibrium model to estimate the aggregate cost of the popu-

lation redistribution generated by self-segregation. The intuition, common with Hsieh and Moretti (2019), is that the population change at the local level is a sufficient statistic for the welfare effects of local shocks at the aggregate level. We find that opening a refugee center reduces aggregate welfare growth by 10^{-4} percent on average.

This paper first contributes to the literature on the impact of refugee inflows on residential preferences. Card et al. (2008) and Aldén et al. (2015) show that natives either leave or settle less in neighborhoods where the proportion of non-European immigrants hits a tipping point. But we show that this segregation occurs with native avoidance even for smaller refugee inflows. In terms of magnitude, our results are close to the one of Boustan (2010). She finds that, after World War 2, every black arrival in northern US cities led to 2.7 white departures. To our knowledge, we are the first to demonstrate a comparable displacement in response to refugees in the current period. According to Balkan et al. (2018), refugee inflows in Turkey have increased demand for housing in native-dominant neighborhoods. In the same vein, Van Vuuren et al. (2019) and Hennig (2018) show a decrease in housing prices following the opening of temporary accommodation for refugees in the vicinity. Our results are consistent with their findings but we can differentiate between native flight and native avoidance, and we study a less urban context in which housing prices may not react quickly enough to compensate defiant potential newcomers.

Our paper also contributes to the literature on the impact of immigration on the local population's preferences. Results are mixed when it comes to the effect of migration on political outcomes as the vote for the extreme right seems to increase with a rise in the migrant population (Halla et al., 2017; Otto and Steinhardt, 2014; Dustmann et al., 2018; Edo et al., 2019), but decreases as a result of a refugees-inflows shock (Vertier and Viskanic, 2019; Steinmayr, 2020) because of the contact theory (Allport et al., 1954). We reconcile these two strands of the literature by showing that natives who are in contact with humanitarian migrants do not leave refugee-hosting municipalities, while it does deter those who are less in contact from moving in the municipality. Furthermore, while natives' preferences towards immigrants have been studied, showing that they may relate to fears about the economic (Mayda 2006; Blanchflower and Shadforth 2009), social (Bansak et al. 2016b; Card et al. 2012b), and cultural (Facchini et al. 2013; Hainmueller and Hopkins 2014) impact of immigration, we add to this literature by studying the local economic consequences of these preferences per se.

Our paper also indirectly contributes to the literature studying the competition be-

tween refugees and natives, what we call refugee rivalry, by demonstrating that part of the economic impact of the refugee arrival is driven by natives themselves. In the literature, evidences of an adverse effect of migration inflows on the labor market (Borjas, 2017; Borjas and Monras, 2017; Card, 1990; Peri and Yasenov, 2018; Mitaritonna et al., 2017; Edo, 2017; Hunt, 1992) and on local amenities (Geay et al., 2013; Ballatore et al., 2018) are mixed. Dustmann et al. (2016) discuss several reasons why this might be the case. One of them is that many studies assume that the labor supply of natives is fixed in the short term. In our paper, we show that this may not be the case. Indeed, as the median capacity of these centers relative to the local population is 0.3%, the refugee rivalry channel is unlikely. Because we cannot differentiate between refugees, subsidiary protected and asylum seekers in the centers, and as they could stay in the same municipality once they exit the center, we still check in the Appendix A.1 that the effect we identify is not due to a labor market shock. We found no consistent links between the size of the potential labor supply shock and the evolution of the workforce.

This article is generally relevant to the literature interested in the dynamics of population at the local level and the reasons for the uneven development of places. We show that amenity shocks are a potentially significant source of divergence between municipalities. Our contribution is that we develop a methodology to study the welfare-cost of local amenity shocks, such as the opening of a refugee center, and the redistribution of population it generates. The general spatial equilibrium model introduced by Rosen (1979) and Roback (1982) relates changes in population in municipalities to changes in resident utility. Hsieh and Moretti (2019) use this framework to show that the evolution of the local population over a defined period is a sufficient statistic for the effect of all local shocks on aggregate utility. In our proposed methodology, we apply this framework, using the identified change of population due to a refugee center opening, to measure the magnitude of its aggregate welfare cost.

To finish with, our results illustrate the need for a careful analysis of the cost of refugee dispersal policies to local economies. Refugee dispersal policies aim to address costs related to refugee rivalry and native defiance by spreading the burden of refugee hospitality across the territory in small centers. Small inflows of refugees at the local level are unlikely to be a credible threat to natives. Still, natives avoid municipalities where a refugee center has opened. Smaller refugee inflow are not sufficient to prevent an adverse impact of hosting refugees because of native defiance. Addressing the negative perception of refugees, as

emphasized in Card et al. (2012a) and in Bansak et al. (2016a), could then be a key objective for policy-makers.

The paper is structured as follows. Section 2 presents our context and data. Section 3 details our identification strategy. Section 4 investigates the effect of refugee center openings on local economies. Section 5 aims to estimate the aggregate welfare cost of refugee hosting. We conclude with Section 6.

2 Context and Data

2.1 Humanitarian migrants and housing centers in France

This paper considers housing centers for humanitarian migrants, namely refugees, subsidiary protection recipients, and asylum seekers. Asylum seekers are people who have applied for refugee status and are awaiting the *Office Francais de Protection des Refugiés et Apatrides*'s (OFPRA, French Office for the Protection of Refugees and Stateless Persons) decision to determine their right to stay or not. Importantly, asylum seekers have no right to work since 1991. Temporary work permits can be issued after nine months, but this rarely happens.

Asylum seekers who acquire the refugee status obtain a ten-year renewable residence permit and, except for voting, have the same rights as natives. Asylum seekers who are not eligible for refugee status but who cannot return to their origin country receive a subsidiary protection status. Subsidiary protection recipients obtain a one-year renewable residence permit and the right to work. In 2019, 17% of asylum seekers were eventually granted the refugee status, while 10% received the subsidiary protection.

The *Office Francais pour l'Integration des Immigrés* (OFII, French Office for the Integration of Immigrants) is the institution that assigns a place in a housing center to asylum seekers. If they accept the housing solution, they will receive a living allowance, the *Allocation pour Demandeurs d'Asile* (ADA, Asylum Seekers Allowance). Asylum seekers who then obtain a protection status can stay in their center for several months, go to centers dedicated to refugees, or exit the national hosting scheme. Once humanitarian migrants exit the centers, they could either stay in the original municipality or leave for other areas.

Due to the increase in asylum applications, it is frequently necessary to open new centers. Between 2004 and 2012, as can be seen in Figure 13 in Appendix C.2, 98 housing centers were opened for the first time in a municipality. As shown in Figure 1, there are

about 1,000 refugee centers homogeneously spread across the French metropolitan area. The average capacity of a housing center is of 66 humanitarian migrants.

The opening of housing centers is consistent with the government's willingness to "spread the burden" of refugee-hosting across the territory. The opening process is as follows: frequent regional project calls are launched at the initiative of the French Ministry of the Interior. NGOs and national housing landlords[1] apply to open and run housing centers. We know from discussions with French asylum actors that the selection of a project mainly depends on the architectural plan, the quality of the project and of the operator, and the financing modalities. Centers are opening in varieties of buildings – former hotels, housing centers for other types of population, police or fireman stations, blocks of private apartments – that can be either found in the existing lot of buildings owned by the center operator or rented for at least 15 years. It is only since 2015 that municipalities have to be consulted before the opening of a housing center for refugees. Focusing on centers that opened between 2004 and 2012, we avoid endogeneity concerns related to the involvement of local political forces. In Section C.3 of the Appendix, we use press data to show that there was little public interest over refugee centers in our period of analysis.

There is a high turnover of refugees and asylum seekers in housing centers. In centers for asylum seekers, those who obtained the refugee status will have to leave in the next three months and a new asylum seeker will take his place. Because asylum seekers do not choose where they are allocated, the composition of refugees at the opening of a refugee housing center is determined primarily by the national refugee inflow. Controlling for national trends will then allow us to control for the change in the refugee composition during the period of analysis.

2.2 Data

2.2.1 Housing centers

[Figure 1 here]

Our database of centers for humanitarian migrants was obtained from the French Ministry of the Interior. It includes all centers for asylum seekers (HUDA, CADA, AT-SA[2])

[1] The so-called "Bailleurs sociaux" in France. They are semi-public bodies that rent social housing for low, means-tested household rent. They are also responsible for the social housing of certain vulnerable populations.

[2] (HUDA: Emergency Accommodation for Asylum Seekers. CADA: Reception Centres for Asylum Seekers. AT-SA: Temporary Reception Asylum Service.

and all centers for refugees and beneficiaries of the subsidiary protection (CPH[3]) as of April 2018. This distribution of humanitarian migrants by legal status is not airtight as centers for asylum seekers can keep hosting refugees or beneficiaries of the subsidiary protection several months after they obtain the protection status. These centers are intended to be there for the long run, usually for at least 15 years. We double-checked the quality of our data with the FINESS[4] database for already closed centers. Our housing center database provides information on the exact location of the housing center, the name of the NGO or social housing landlord running the center, its opening date, and its capacity.

In our analysis, we focus on municipalities that have opened a housing center for humanitarian migrants for the first time. Second-time openings may not have the same impact on the labor market or on the hearts and minds of natives. Figure 1 displays the commuting zones where, between 2004 and 2012, a municipality, or several, opened for the first time a housing center. Figure 13 shows the timing of the ninety-eight openings we study in our paper. Table 14 in Appendix E compares hosting municipalities in our sample with the average municipality in 2002. We see that hosting municipalities were on average more populated than the typical municipality, less rural, with a higher unemployment rate and fewer unskilled workers.

2.2.2 Municipality data

This study uses the DADS ("Declaration Annuelle des Donnees Sociales"), a database linking employees and employers. It covers approximately 85% of French employees in 2008[5]. The DADS uses forms sent by private companies for the payment of employer contributions. Firms report periods of employment, the corresponding wage, and most importantly the municipality of residence (where the employee lives) and the municipality of employment (where the employee works) for each position held. We are also able to monthlyize the annual data given that we know when the job starts and ends during the year. We use the data available from 2002 to 2014 to create a panel of all French municipalities.

As a result, we have a monthly panel of more than 34,000 municipalities between 2002 and 2014, with the number and average wage of people *working* in the town and the

[3]CPH:Provisional Accommodation Centre.

[4]*Fichier National des Etablissements Sanitaires et Sociaux* database. We did not use this database for our analysis because it does not provide the capacity of housing centers and because the information on the date of the opening is sometimes inaccurate.

[5]Public sector, and self-employed workers are not included.

number and average wage of people *living* in the town. According to Coudene and Levy (2016), this distinction is important since only 36% of active residents lived and worked in the same municipalities in 2013 in France. With the DADS data, we can use both the workplace and the residence to analyze the location decisions of natives. Though we cannot consistently differentiate foreigners and natives in the DADS, we explain in Appendix B why the population response is likely to be driven by natives.

The number of workers in the municipality informs us about whether people change their valuation of their *living* environment and local amenities. Natives may expect refugees to have a detrimental impact on the quality of local amenities, which could lead them to avoid or leave the area. We use the number of working residents as a measure of population as the annual municipal population statistics in the census are only collected every five years in municipalities of less than nine thousand inhabitants. Because the resident working population does not encompass some categories of the population such as retirees or people outside employment, we also use the IRCOM database from the *Direction Generale des Impots*, the French government agency in charge of taxable revenues. It provides information on the total number of fiscal households, of retiree households, of households that pay the income tax, on the total taxable revenues, and other variables at the municipal level. A fiscal household is the family unit composed of either a single person, or two partners and their children or other dependents who pay the French income tax as a group. Natives may also expect refugees to compete with them in local labor markets and to affect their wages and the number of available jobs. We also look at whether people change their valuation of their working environment by looking at workers in the municipality, instead of the number of employed residents.

To finish with, we use the so-called FICUS (until 2007) and FARE (since 2008) datasets that contains the financial and fiscal accounts sent by all French firms to the fiscal authority. We aggregate them to retrace at a yearly frequency the evolution of the number of firms, the total value-added, and the value of sales for the municipalities in our panel.

3 Methodology

3.1 Econometric model

Our methodology is as follows: we compare hosting municipalities with other municipalities within their commuting zone two years before and after the opening of a refugee center.

We call an "event group" the group made up of one hosting municipality and its control municipalities. In our study, we observe 98 event groups for 49 months. We evaluate the average effect of the opening of refugee centers by estimating the following OLS model:

$$Log(Y_{igt}) = \beta.Opening_{it} + \mu_{gt} + \omega_i + \delta_t + \epsilon_{igt} \tag{1}$$

where $Opening_{it}$ is a dummy variable equal to one if a refugee center has opened in the municipality i at time t in the event group g. μ_{gt} capture time-varying shocks in the event group g. We only include observations that are less than 24 months away from the opening in a given event group g. δ_t, ω_i are calendar time and municipality fixed effects. Under conventional identification hypothesis, the OLS estimated coefficient of β measures the average deviation of hosting municipalities relative to their commuting zone trends (μ_{gt}) after the opening.

The separation by event group g makes our approach similar to a "stacked" difference-in-differences approach where we estimate a separate difference-in-differences model in every event group g. There is only one hosting municipality per event group g, but if there are several openings within a commuting zone, the commuting zone can be present several times. Hosting municipalities are never included in any control group. It is then equivalent to estimate separately the following model[6] in each event group two years before and after the opening:

$$Log(Y_{it}) = \beta_g.Opening_{it} + \omega_i + \delta_t + \epsilon_{it} \tag{2}$$

We can then aggregate all the coefficients β_g to get the average treatment on the treated β. Furthermore, we estimate the following model to look at the dynamic effect of openings:

$$Log(Y_{igt}) = \sum_{k=-24}^{24} \beta_l.\mathbb{1}\{l = k\} \times T_i + \mu_{gt} + \omega_i + \delta_t + \epsilon_{igt} \tag{3}$$

Where T_i is a dummy variable equal to 1 for refugee-hosting municipalities and l the time relative to the opening (refugee centers open when $l > 0$ and $l \in [-24, 24]$). Under conventional identification hypothesis, the OLS estimated coefficients β_l measures the causal impact of a housing center opening l months away from the center opening on Y_{igt}.

[6] This approach is not equivalent to the two way fixed effect model with a dummy treatment variable that is traditionally used for difference-in-differences. Recent literature, De Chaisemartin and D'Haultfoeuille (2019), Goodman-Bacon (2018) or even Abraham and Sun (2018), has shown that when treatment effects are heterogeneous across cohorts; the two-way fixed effect estimator recovers a linear combination of cohort-specific average treatment effects on the treated where some weights can be negative, mostly because early and late cohorts are not observed on an interval of time of the same length. In this paper we adopt the elegant solution proposed in Appendix D of Cengiz et al. (2019): a "stacked" difference in difference approach which solves the issue by aligning event groups by event time and not calendar time: all cohorts are observed for the same number of time before and after the opening.

Standard errors are clustered at the municipality level.

3.2 Identification Hypothesis

Our approach is similar to a difference-in-differences as shown in equation (2) and Section K of the Appendix. There are two hypothesis under which we estimate a causal effect: the parallel trend and the stable unit treatment value assumptions.

3.2.1 Parallel trend hypothesis: do refugee centers open in places with worse demographic prospects ?

[Figure 2 here]

The first hypothesis is that outcome trends in control municipalities are an appropriate estimate of what would have occurred in hosting municipalities without a refugee center opening. We test whether hosting and non-hosting municipalities within the same commuting zone follow a similar trend before a refugee center opens. If they have the same pre-trends, it is more likely that both groups would have evolved in the same way in the absence of a housing center opening in hosting municipalities.

Figure 2 shows that, on average, the opening date coincides with a significant and gradual decrease in the number of employed residents in hosting municipalities, whereas we see no change in other municipalities of the commuting zone. The divergence between the two groups could be overstated— e.g. all outflows from hosting municipalities could go to the control group after opening. Overall, the solid (hosting municipalities) and dashed (control municipalities) lines follow a similar trend before the opening but start to diverge at $l = 0$. This tends to support the parallel trend assumption as well as to indicate a negative impact of openings of refugee centers on population. Because hosting and non-hosting municipalities do not seem to diverge before the opening, this supports the hypothesis that refugee centers do not open in areas with worse demographic and economic prospects. The only drawback with Figure 2 is that it does not take into account potential spillover effects.

3.2.2 Stable unit treatment value assumption

The second hypothesis is that the opening of a refugee center in one municipality does not impact its control municipalities' potential outcome (stable unit treatment value assumption or SUTVA). In our case, we could face a breach of the SUTVA hypothesis as the

impact of a refugee center opening may spill over neighboring towns. In particular, natives may move to neighboring towns in response to the opening of the refugee center. Another possibility is that the presence of a refugee housing center in the neighboring towns may also influence the decision to move to the municipality.

We use two different strategies to control for potential spillover effects. First, we replicate our analysis using an alternative control group. We match hosting municipalities with credibly unexposed municipalities outside the hosting commuting zone thanks to a propensity score. We match on the characteristics of localities two years before the opening of the center. The municipality characteristics come from the French Censuses: the population, whether the municipality is rural or not, the share of men, the share of unskilled workers, the number of migrants, the number of active workers, and the number of empty buildings. We bootstrap standard errors using the wild-bootstrap procedure and 500 repetitions (see Cameron et al. (2008)). For cities outside the commuting zone, the assumption that there is no spillover effect is more palatable.

[Figure 3 here]

We assess the quality of the matching procedure in Appendix E. More importantly, we also check whether hosting and matched control municipalities share similar economic prospects before the opening of the refugee shelter. We compare the evolution of the number of working residents in hosting municipalities in a four-year window around the opening of the refugee center with the corresponding trend in the matched control group. In Figure 3, we see that hosting and matching control municipalities follow a similar trend before the opening of the refugee center. After the opening of the refugee center, the number of employed residents in the hosting municipalities decreased relative to control municipalities.

In our second and preferred approach, we keep municipalities within the same commuting zone in the control group. However, we control for the distance to the hosting municipality and its interaction with the time relative to the refugee center's opening. The inspiration comes from Clarke (2017) who derives a set of conditions with which difference-in-differences estimates can be unbiased even with a spillover effect on control units. To account for spillovers, we thus add $\sum_{k=-24}^{24} \nu_{ilg}.\mathbb{1}\{l = k\} \times d_{ig}$ to our equations of interest. d_{ig} measures the geographical distance to the hosting municipality in the control group and ν_{ilg} captures time-varying shocks related to the distance of municipality i to the treated

municipality in the event group g. If spillover effects do not depend on municipality-specific time variant components, then ν_{ilg} captures local spillovers, that is the effect of being close to the hosting municipality when a refugee center opens. The assumption here is that the spillover effect is a linear function of the distance to the opened center. ν_{ilg} is also likely to control much more precisely for local shocks, even at the sub-commuting zone level. In Appendix F, we further investigate potential spillover effects and propose another estimation strategy to take them into account.

4 Results

4.1 The mobility response of natives

4.1.1 Resident employed population

We estimate Equation (1) on a panel of French municipalities between 2002 and 2014 to look at the impact of refugee center openings on natives' mobility. Table 1 shows the estimated β for the number of working residents in five different specifications. Columns (1) and (2) present the estimated ATT (Average Treatment on the Treated) when a simple Difference-in-Difference approach is estimated using a 2-way fixed effects model. In column (2), we add event group fixed effects. Column (3) compares hosting municipalities with a matched sample of municipalities outside the hosting commuting zone. Columns (4) and (5) compare hosting municipalities with municipalities within their hosting commuting zone, but in column (5) we also take into account potential spillover effects. Column (5) is our preferred specification. Results are qualitatively similar across all specifications but vary in magnitude. On average, the number of residents employed decrease by about one to two percent relative to the control group following the refugee center opening.

[Table 1 here]

[Figure 4 here]

Figure 4 shows the estimated β_l coefficients of Equation (3) for the number of employed residents and their confidence interval using the same specification as in Column (5). β_l measures the impact on the population of the refugee center opening l months away in refugee-hosting municipalities compared to other municipalities in the same commuting zone. We see that hosting municipalities deviate from their control municipalities, starting from the time of the opening and not before. Twenty-four months after the opening,

the population of employed residents in the hosting municipalities decreased by about 1.8% compared to the control group. Since the median size of a refugee center relative to the local workforce is 0.3%, this implies that there are three to six fewer people in the municipality for one refugee place in the center. Because humanitarian migrants could stay in the municipality once they exit the center, this gives an upper bound of the relationship between refugee inflow and the native population.

[Figure 5 here]

As mentioned earlier, estimating equation (1) is equivalent to estimating equation (2) separately for each event group g. In Figure 5, we plot β_g municipality by municipality, sorting by the size of the municipality treatment effect.

The advantage of Figure 5 is that it does not hide the heterogeneity of the effect, unlike previous tables and figures. In some municipalities, the opening of a refugee center has a positive effect on the number of residents. They may be false positives or indicate that the situation is more complex depending on the municipality, for example, openings may coincide with other events in some specific cases. Still, the majority of β_g is significantly lower than or not significantly different from 0. The red line is the estimated average treatment on the treated effect that we observe in column (5) of Table 1. Overall, Figure 5 confirms the results obtained so far and shows that they are not driven by outlier municipalities.

4.1.2 Fiscal households

So far, we have used changes in the number of employed residents as a proxy for changes in the population. Municipal population statistics from the census are only collected every five years in most municipalities. Using the number of employed residents was thus the only way to obtain a monthly measure of population for all municipalities. However, the number of employed residents is an imperfect measure of the population, since it does not include inactive people and people working in the public sector. The average correlation in cross-sections between the number of residents employed and the census population is 0.83. Within municipalities, the correlation between the evolution of population and the number of working residents is 0.25[7]. To be sure that we are capturing the evolution of the population, we reproduce our approach in a yearly panel on the number of fiscal households that we obtained from the IRCOM dataset. Columns (1), (2) and (3) of Table 2 reproduce

[7]It is the within R-squared of the OLS relating census population to the number of working residents when controlling for municipality fixed-effects.

in the same order the models of columns (3), (4) and (5) from Table 1. They confirm our previous results both in sign and in magnitude.

[Table 2 here]

4.1.3 Native avoidance or native flight?

If we identify a significant decrease in population due to the opening of the refugee center, this could mean that more people leave the municipality (an increase in outflows) or that fewer people arrive (a decrease in inflow) or a combination of both. In the literature, these two phenomenons are referred to as "native flight" and "native avoidance".

For each municipality, we measure the outflows as the number of resident workers who were in the locality the year before but are no longer, and inflows as the number of resident workers who were not in the locality last year but who are now. We are able to do this because we know for every position occupied by a worker in the year T the location of his job and his residence and whether he worked in the year T-1. Our definitions of outflows and inflows are similar to the one used by Dustmann et al. (2017).

In Table 3, we reproduce the approach of column (5) of Table 1 in a yearly panel with the log number of inflows in the municipality in column (1) and the log number of outflows in column (2). Due to the decline in population, both inflows and outflows decreased following the refugee center opening. The decrease in the number of inflows is, however, much higher than the decrease in outflows, and only the decrease in inflows is statistically significant. These results indicate that the decrease in population is due to fewer people moving in hosting municipalities following the opening of the refugee center. Native avoidance, rather than native flight, could, therefore, be the mechanism explaining the decrease in population following the opening of a refugee center.

[Table 3 here]

4.1.4 Heterogeneity

With the IRCOM dataset, we can also look more closely at which households react more strongly to the opening of a refugee center. In Table 4, we look at the effect of the opening on the number of retiree households (i.e. households with at least one pensioner) and the number of households taxed (i.e. households that pay the French income tax). In France, households who pay the income tax represent about half of the total population in 2014.

This is because the income per unit to qualify for the income tax was 9,700€ in 2017 and is combined with other possible exemptions.

Retiree households are affected in the same way as the general population, which confirms that the origin of population decline cannot wholly be attributable to an effect on the labor market as we discuss in Appendix A.1. Wealthier households that pay income tax react more strongly to the opening, which makes sense given that they are likely to have more alternative locations. In Appendix B we investigate the effect on different sub-populations according to income skills and we do not find evidence of heterogeneous effects.

[Table 4 here]

4.1.5 Longer term effect

To investigate the longer term effect, we looked at the 76 openings between 2004 and 2010 in Figure 6 to extend the post-opening period of analysis to 48 months. As in our main analysis, we study the evolution of the number of employed residents. We also examine the effect of the openings on the year-to-year growth of the number of residents to identify exactly when the decrease in the population is the strongest. Taking year-to-year growth as a dependent variable is robust to violations of the parallel trend assumption in the number of residents. This identification now requires parallel growth rather than parallel trends, thus allowing for differential trends in the number of residents.

[Figure 6 here]

Figure 6 cannot be fully compared to Figure 4 as the samples of openings differ but results are similar. After the opening the number of employed residents and the growth in the number of employed residents decline. First, the number of employed residents increasingly decreases for more than two years. Then the decrease decelerates to become more or less stable after three years. This implies that after three years, either native avoidance becomes weaker, maybe because the signal effect of refugee centers fades, or native avoidance is compensated by something else.

4.2 Economic consequences of native avoidance

4.2.1 Firm reaction

For refugee-hosting municipalities, native avoidance represents a decrease in the local pool of consumers. In addition, in subsection 4.1.2, we bring evidence that households not

coming to the municipalities were richer than the average. We could expect the decrease in the number of workers shown in Appendix A.1 to be driven by the decrease of the local demand for labor and goods in general.

We use data from the FICUS-FARE datasets, which gather individual data and statistics on company profit declarations to the French Tax General Directorate. We aggregate at the municipality level data on the total value-added, the value of sales, and the number of firms. We reproduce in Table 5 the models of columns (3) and (5) from Table 1 to see how the logarithm of these three outcomes evolve after the opening of a refugee center.

[Table 5 here]

Both approaches yield similar results in magnitude but not in significance. The total value-added and the value of sold production decrease from two to five percent after the opening. Using matched municipalities outside the commuting zone exhibit less statistical power than using municipalities within the commuting zone. We see that the number of firms, the value-added, and the number of sales decrease when we compare refugee-hosting municipalities to municipalities within the same commuting zone.

The total value of sales is decreasing, which could also indicate that the number of consumers falls. This reduction of demand could lead firms to relocate or to go out of business. In one of our specifications, the number of firms decreases by about three percent. These results confirm that labor demand is likely to decrease as well. As the activity of local firms is decreasing after the opening of the refugee centers, the demand for native workers diminishes. Hosting refugees seem to depress economic activity because of native avoidance.

4.2.2 Impact on local taxes

At the municipal level, the reduction of the population can have a cost for local authorities in terms of tax base. We estimate that the opening of a refugee center is associated with a reduction of approximately five points of the tax base, as we can see in Table 6. The tax-base reduction can have important consequences for the finance of hosting municipalities. In order to learn more about the state of local finances, we exploit a dataset from the DGFIP that records the different sources of income of French municipalities since 2000. French municipalities have two main sources of financing: local taxes collected by the municipality and direct transfers from the State.

[Table 6 here]

There are different types of local taxes. One of the main ones is the housing tax, *taxe d'habitation*, which is a rental value tax paid by inhabitants. The second is a property tax on constructed lands, *taxe foncière sur les propriétés bâties*, which is paid by landowners. We expect the fiscal base of these taxes to decline with the openings such that their total income decrease as well. In Table 6 we check whether the revenues of the local taxes are negatively affected. The proceed of local taxes decrease by more than 3 percent when we compare refugee-hosting municipalities with municipalities within the same commuting zone, and by one percent with matched municipalities outside the commuting zone. This is because the proceeds of the housing tax and the property tax are both declining, by almost six and three percent in our less conservative estimates. Indeed, native avoidance can result in some non-rented housing units, and as owners can be exempted from the housing and property tax if the housing is vacant, the total municipal revenues from these taxes would decrease. In Appendix J we verify that this evolution is not explained by municipalities reducing local tax rates for housing and property taxes.

5 Estimating the aggregate welfare cost of refugee center openings

In this section, we model the effect of a refugee center opening as a negative amenity shock. Native avoidance means that the opening of refugee housing centers decreases the perceived quality of amenities by prospective residents. Following our previous findings, the opening of a refugee center should be seen above all as an amenity shock at the local level that may or may not have an indirect impact on the labor market. Focusing on the quality of amenities is also consistent with what people feel about immigration. Card et al. (2012a) find that compositional concerns are 2 to 5 times more important in explaining variations in individual attitudes towards immigration policy than concerns about wages and taxes.

We base our model on the spatial general equilibrium models described by Roback (1982) and Rosen (1979), which have been used to analyze the location decisions of individuals between cities. The specificity of their model is that, in addition to housing costs, both income and amenities can vary. In particular, housing costs and wages may fluctuate in order to compensate consumers for differences between cities in terms of actual or

perceived quality of life.

Residents choose to locate in a city that maximizes their utility, i.e. where their wage net of housing and amenity costs is the highest. However, there is no perfect mobility of labor, given that workers have different preferences over locations. Migration is costly, depending on the preference of workers. We can write the direct utility of the resident j in the city i as:

$$V_{ji} = \epsilon_{ji}.\frac{w_i.z_i}{P_i^{\beta}} \tag{4}$$

where ϵ_{ji} is a random variable measuring the preference for a city i by an individual j, w_i is the local nominal wage, z_i measures the value of local amenities between 0 and 1, P_i the local housing prices, and β is the share of housing costs in the resident's consumption. $\frac{w_i.z_i}{P_i^{\beta}}$ is the wage net of the cost for housing and amenities.

Each city i produces a homogeneous good and has the following Cobb-Douglas constant return to scale production function:

$$Y_i = A_i.L_i^{\alpha}.T_i^{1-\alpha} \tag{5}$$

where A_i is the total factor productivity in city i, T_i is the land available for business use, L_i is the number of working residents and α is the elasticity of production with respect to labor, also known as the labor share of income. For simplicity, we choose a production function without capital, but this would not alter our final predictions if we were to include it[8].

The local housing price is given by:

$$P_i = \bar{P}_i.L_i^{\gamma} \tag{6}$$

with γ being the inverse elasticity of housing supply with respect to the number of residents in the city, and $\bar{P}_i$ the part of the local housing price that does not vary with the number of residents. We assume γ to be the same across cities[9].

In this simple model, we assume, like Kline and Moretti (2014), that the joint distribution of ϵ_{ji} is given by $F_g(\epsilon_1, ..., \epsilon_N) = e^{-\sum_i^N \epsilon_i^{-\theta}}$ where $1/\theta$ measures the degree of labor mobility. This assumption means that the labor supply curve is upward slopping and that its slope depends on the heterogeneity in labor mobility. The inverse local labor supply of a given city is:

[8] Hsieh and Moretti (2019) arrive at the same conclusion with a production function where capital is a production factor.

[9] This assumption does not change the main predictions of the model.

$$W_i = V.\frac{\bar{P}_i^{\beta}.L_i^{1/\theta}}{Z_i} \tag{7}$$

where V is the average worker utility in all cities. This expression means that when a city experiences a change in wages, amenities, or housing prices, the number of people willing to move in or out depends on $1/\theta$. The case of perfect mobility is $\theta = \infty$.

At equilibrium, the marginal product of labor is equal to the local nominal wage and the inverse local labor demand that determines the number of residents is then:

$$L_i = (\frac{\alpha}{V}.A_i.T_i^{1-\alpha}.\frac{z_i}{\bar{P}_i^{\beta}})^{\frac{1}{1-\alpha+\beta(\gamma+1/\theta)}} \tag{8}$$

The partial local equilibrium of the model is given by equations (6) for housing prices, (7) for local wage, and (8) for the number of residents. Differences in population across cities are driven by differences in amenities, real wage, and housing prices.

In previous sections, we showed that the opening of a refugee center constituted a negative amenity shock for hosting municipalities. We now look at the effect of this redistribution of residents at an aggregate level. As hosting municipalities become less attractive, natives move out or avoid hosting municipalities such that demand for housing increases in non-hosting municipalities. To measure the aggregate welfare cost of such a refugee shock, we need to look at its impact on the aggregate utility V. In the following, we explicitly state the general equilibrium conditions and use the model to estimate the aggregate welfare costs of opening a refugee center.

The related intuition of this study is that people "vote with their feet" as in Tiebout (1956) and that the evolution of the population is an important outcome to consider when studying the opening of refugee centers. Following Hsieh and Moretti (2019), we use the assumption that the change in the number of people employed in a city relative to the national average is a sufficient statistics for the aggregate welfare effect of all the local shocks in the town in a Rosen-Roback model.

At complete equilibrium, residents need to be indifferent across cities, which means that the local price in a city Q_i $(= \frac{P_i^{\beta}}{z_i})$ relative to the national (resident weighted) average corrected for imperfect labor mobility $\bar{Q}(= \sum_i L_i^{1+1/\theta}.Q_i)$ is equal to the local wage (W_i) relative to the national average $(\bar{W})$:

$$\frac{Q_i}{\bar{Q}} = \frac{W_i}{\bar{W}} \tag{9}$$

If we now impose that aggregate labor demand is equal to aggregate labor supply

(normalized to one), we get the following expression for aggregate output Y:

$$Y = \sum_i (A_i . \frac{Q_i}{\bar{Q}} . T_i^{1-\alpha}) \tag{10}$$

Also, since the labor share of income is α, aggregate utility is given by:

$$V = \frac{\alpha . Y}{\bar{Q}} \tag{11}$$

The ratio of aggregate labor income to average local price across all cities is $\bar{Q}$. The general equilibrium of this model is defined by the equations for local population (8), housing prices (6), nominal wages (7), aggregate output (10) and aggregate utility (11).

As in Hsieh and Moretti (2019), population change is a sufficient statistic for the aggregate effect of all the local forces involved in the model: total factor productivity, amenities, and housing prices. To understand this, consider a change in aggregate utility after an amenity shock:

$$\Delta V \propto \Delta (\frac{z_i}{P_i^{\beta}})^{\frac{1}{1-\alpha+1/\theta}} \tag{12}$$

We can decompose this expression in two parts. The first part of the equation, $z_i^{\frac{1}{1-\alpha+1/\theta}}$, is the direct effect of a change in amenities on aggregate utility (the effect of changing weighted average local amenities). The second part $P_i^{-\beta . \frac{1}{1-\alpha+1/\theta}}$, represents the missallocation effect, which results from the change in the local marginal product of labor relative to the rest of the country, and the price effect, which results from how the change in local prices Q_i modifies the local prices average in the country $\bar{Q}$.

In addition, if we substitute equation (7) into equation (8), we find that the change in population is proportional to:

$$\Delta L_i \propto \Delta (A_i . \frac{z_i}{P_i^{\beta}})^{\frac{1}{1-\alpha+1/\theta}} \tag{13}$$

Comparing equations (12) and (13), we realize that the evolution of the local population in the event of an amenity shock is proportional to the change in aggregate utility if productivity (A_i) is fixed. In other words, the evolution of the local population is a sufficient statistic for the effect of local shocks on aggregate utility or welfare if we control for A_i.

From Table 1 and Figure 4, we know that the average change in the number of residents in hosting municipalities relative to other municipalities within the same commuting zone is between 1 and 2%. Following the model, we can measure the contribution of the refugee

center openings to welfare growth by calculating its contribution to overall population growth.

We are not assuming a measure of welfare here, but we are just measuring how much average welfare would have grown without one additional opening of a refugee center. The average hosting municipality population accounts for 0.05% of the total French population. This means that one opening reduces on average aggregate welfare growth by 10^{-4} percentage points (0.05×0.02) over two years.

We can go even further. According to equation (11), if we assume that local shocks do not affect national average prices and that the labor share of income remains stable, the evolution of the number of residents is a sufficient statistic for the effect of local shocks on aggregate output. As it turns out, 10^{-4} points of output amounted to around 2.1 million euros in France in 2014. Under these broad assumptions, this is then a measure of the indirect costs of hosting refugees. This figure should be used only for rough comparisons but gives an approximation of the costs – in terms of lost wages, higher housing prices, and lower non-refugee amenities – that on aggregate potential newcomers are ready to pay in order not to be living near refugee centers.

6 Conclusion

In this paper, we tried to understand the local economic impact of refugee accommodation policies. We show that municipalities hosting refugees experience a decrease in their population and in their economic performances. We demonstrate that this is not because refugee centers open in municipalities with far worse economic prospects nor the result of the actions of refugees. We are the first paper to explain this adverse economic impact of hosting refugees by inefficiencies that natives themselves have produced by avoiding refugee-hosting localities. We exploit the openings of housing centers for refugees and asylum seekers in about one hundred municipalities between 2004 and 2012 in France. We compare these municipalities with a control group that did not experience the opening of a refugee center. After a center opens, the population decreases by about two percent. We show that this decline is because fewer people move in refugee-hosting municipalities.

This change in population reflects a shift in the preference of natives for hosting municipalities following the opening of refugee housing centers. Households with higher incomes, and then probably more location choices, react more to the opening. On the one hand,

natives could see the opening of the refugee center as a signal for higher competition for local resources. On the other hand, natives may avoid hosting municipalities because of cultural insecurity or xenophobia. In our context, the competition channel is not credible as refugee inflows are negligible even at the local level and our results point toward the latter channel. We demonstrate that people living near refugees do not leave at a higher rate, which would indicate that the deterrent effect of refugees is due to prejudices of those who are not in contact with them.

We find a local negative economic impact of hosting refugees, although it is not due to direct harm caused by refugees but to native avoidance. Fewer natives entering host municipalities means fewer taxpayers and fewer consumers. We find that hosting municipalities, because of lower tax collection, and firms, because of lower sales, suffer from it.

We use the evolution of the population in hosting municipalities as a sufficient statistics to assess the aggregate welfare cost due to the redistribution of the population because of the openings. We find that opening a refugee center reduces aggregate welfare growth by 10^{-4} percentage points on average. For ballpark comparisons and under additional assumptions to equalize changes in welfare to changes in output, we measure that the cost of opening a refugee center amounts on average to 2.1 million euros over two years.

Our results show that refugee inflows can provoke segregation at the municipality level. The policy implications of our findings are twofold. First, refugee dispersal policies could cost more at the local level than it would appear because of native avoidance. Segregation after the opening of the refugee housing center has a significant impact on the local tax base and may depress the local economy. Second, we may reduce the cost of housing refugees by addressing the root of self-segregation – natives prejudices against refugees – rather than focusing on potential refugee rivalry.

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Appendix A Alternative channels

A.1 Local Labor market

[Table 8 here]

We examine whether the competition on the labor market between refugees and natives could explain the decrease in local population. For instance, natives could flee or restrain from entering local labor market because of increased competition. To do so, we start by investigating potential evidence of labor supply shock from refugees. We study the effect of openings on the logarithm of the number of hires of foreigners in municipalities, which we obtained from the DMMO database, in Table 8. We find no indication that the number of hires for foreigners increases, which is what we expected given the small size of humanitarian inflows and that asylum seekers do not have the right to work. However, the opening of a refugee center could lead to an increase of unobserved informal employment. We then look at the effect of openings on workers in the municipality, which include both resident and non-resident workers. Table 9, Figure 7a and Figure 7b can be interpreted in the same way as Table 1 and Figure 4. In Table 9, we use the approach of column (3) and (5) of Table 1 to look at two different outcomes that are the number of workers employed and their average hourly wage. Looking at the 95% confidence intervals, we see that we can exclude a decrease of more than 1.5% of the hourly wage. The number of workers may slightly decrease, but this effect is far more imprecise than the one on residents.

[Table 9 here]

[Figure 7 here]

To investigate whether the evolution of employment may be caused by the change in the resident population, we look at the evolution of the number of non-resident workers in the municipality following the opening of the center. Table 10 shows no significant effects of the openings on the number of non-resident workers. Compared to Figure 7a where we consider all workers, Figure 8 does not provide any evidence of a decrease in the number of non-resident workers, which indicates that the decrease in the number of workers is likely to be driven by workers who are also residents.

[Table 10 here]

However, we cannot totally exclude the possibility that we don't capture a significant effect on the number of workers in the hosting municipality because this is a more noisy variable given that people change more of jobs than of residence. Some coefficients in Tables 1 and 10 are negative and of the same magnitude as in Table 9, although their standard error is much higher. Nevertheless, given the small size of refugee inflow compared to the local labor supply, we believe that this might be due to a decrease in labor demand. A simple labor and supply model would show that in order to achieve a reduction in the number of workers without a decrease in wages, there must be either a decrease in labor demand or an exceptionally high labor supply elasticity. A ballpark calculation using β_{+24} for the number of workers and hourly wage from Figure 7 gives an estimated labour supply elasticity of about 6.5 if labor demand does not adjust. This much higher than the range of elasticities of 1 to 1.9 reported by Falch (2010) for Norwegian teachers and the estimate of 2 reported by Dal Bó et al. (2013).

[Figure 8 here]

Finally, our results are in accordance with the ones of Vertier and Viskanic (2019) where they look at the effect of opening CAO housing centers in France from 2012 to 2017. They find no significant difference in net job creation per inhabitant between municipalities which eventually received a CAO center and those that did not. We complement this result by showing that employment and wages in hosting municipalities are also not affected.

A.2 Criminality

Can the population decline be explained by an increase in crime due to the arrival of refugees? This is a relevant question if we assume that safety concerns could affect the relocation choice of natives. Moreover, even a small inflow of criminals could have a large impact on criminality.

Gehrsitz and Ungerer (2017) studied the 2015 refugee wave and its influence on the number of crimes committed in Germany. They find that there is a substantial and positive correlation between the number of refugees distributed to a county and an increase in crime, even after they removed immigration violations from the crime statistics. In particular, they put forth a 1.5% increase in crime in regions that welcomed a higher number of refugees compared to regions that welcomed a lower amount. This section aims to replicate these results in the French case, to investigate whether this could be a explanation of why natives

change their location decision after the opening of a refugee center.

We look at departmental crime databases from 2002 to 2014, where the police report the monthly number of crimes and misdemeanours. We compare departments where refugee centers have opened and departments where they have not. We divide crime and misdemeanour into several categories, such as murders or murder attempts, thefts, sexual assaults of any kind, drug-related crimes or misdemeanours. In Table 16 of the Appendix, we describe how we classify the original 108 categories of crimes and misdemeanours in these four broader categories.

[Table 11 here]

Table 11 shows the difference-in-difference coefficients when the outcome is the logarithm of the monthly count of crimes at the departmental level. The minimum detectable effect for the total number of crimes is 1.22[10], which means that if the number of crimes increased by more than 1.2%, we would have detected it. This is a level below what Gehrsitz and Ungerer (2017) found. Figure 9 plots the evolution of the number of crimes or misdemeanours committed in departments experiencing the opening of a refugee center compared its average evolution in other departments. Events are weighted by the capacity of the refugee center to give greater importance to departments with a higher influx of refugees and asylum seekers.

In this section, we do not claim to draw a definite conclusion as to the relationship between the opening of refugee centers and crimes. The report of crimes is endogenous, as an increase in the number of crimes or misdemeanours reported may indicate a real increase or an increase in reporting. Moreover, unlike Gehrsitz and Ungerer (2017), we do not identify the victim or the perpetrator. Finally, we only observe crime statistics at the departmental level and not at the municipal level as in the rest of our study, and we are unable to identify anti-refugee crimes with the existing French Police classification.

[Figure 9 here]

Overall, there is no real difference between the criminality time series in refugee-hosting departments and in other departments. We cannot link the opening of a refugee center to an outbreak of crime. Looking at American cities in the 1990s, Cullen and Levitt (1999) finds that a 10% increase in crime has led to a 1% decline in population. Criminality is therefore unlikely to explain the full extent of what we observe in refugee-hosting municipality.

[10] This is an ex-post measurement, based on the fact that the minimum detectable effect is 2.8 times the standard error.

Appendix B Sub-populations in the DADS

We built different sub-populations of employed residents at the municipal level from the DADS. First, we divided the population by job type. On the one hand, there are residents working in low-skilled jobs (manual workers and employees PCS 5 and 6 in the French professional categories) and, on the other hand, residents employed in high-skilled jobs (professionals, executives and middle managers, PCS 3 and 4 in the French professional categories). Second, we divided the population in terms of their income by looking at the number of employed residents working above and below the national median wage.

[Table 12 here]

In Table 12, we do not observe any obvious heterogeneity of the effect. We might not see heterogeneous effects because we can only look at broad categories. Otherwise, we could end up dropping many small municipalities where the sub-population is 0. Also, the working wage may not be an important determinant for location decisions but rather wealth and capital income that we cannot measure at the local level. Finally, avoiding a hosting municipality is less costly than moving out of it. Avoidance is thus less related to income (and skill-related income) than flight. Overall, these results reinforce the unlikeliness of a labor market shock as it would likely be heterogeneous along skills and wages.

Because previous migrants can be more exposed to the economic impact of subsequent migrant inflows (Ottaviano and Peri, 2012), it would have been interesting to distinguish national and foreign workers. Already established migrants could bear the brunt of the local economic impact of the refugee center opening. Unfortunately, the DADS does not provide a coherent way to differentiate between national and foreign workers. In Figure 10, we show inconsistently large variations in the number of foreign workers every year that are likely explained by different information collection methods over the years. Nevertheless, we used the DMMO database to look at whether openings of refugee centers impacted the number of hires of foreigners in hosting municipalities. In Table 8 of the Appendix A.1, we find no effect of the openings on the number of hires of foreign workers. In addition, several elements convince us that avoidance is not a reaction of the migrant population. First, migrants are underrepresented in the retiree population and retirees react as much (Table 4) than the general population (Table 2), which can also confirm that the avoidance behavior seems not to be related to a labor market threat of refugees. Second, municipalities with

a higher migrant share do not experience higher native avoidance as shown in Figure 11.

[Figure 10 here]

[Figure 11 here]

Appendix C Refugees and housing centers

C.1 Humanitarian migrants in France

[Figure 12 here]

C.2 Refugee centers openings

[Figure 13 here]

C.3 Refugee centers in the press

Our period of analysis is relatively calm regarding the public interest in refugee issues. Figure 14 shows the evolution of the proportion of articles in the French regional and national press relating to the terms "refugees" and "housing centers" between 2000 and 2018. Between 2004 and 2012 there were 4 times fewer articles including those two terms than after 2014, or than during the refugee crisis of the Yougoslav Wars from 2001. We believe this minimizes the risk that residents anticipated the openings.

[Figure 14 here]

Appendix D Definition of population

[Figure 15 here]

Appendix E Matching

[Table 13 here]

To assess the quality of the matching procedure, we start by checking whether the propensity score properly balances the characteristics between hosting municipalities and comparison group units. Table 13 shows the mean for all the Census variables used to estimate the propensity score matching for the five nearest neighbors in the control sample

(our preferred matching algorithm[11]). Matching was efficient as the characteristics of both samples are reasonably balanced, with no statistically significant differences.

We also investigate whether municipalities that have similar characteristics have the same likelihood of being both in the control and treatment group. Testing for the common support assumption is a way to test the performance of the propensity score-matching algorithm. In Figure 16, we thus look at the distribution of propensity scores for hosting and control municipalities. Both distributions are very close, indicating that the propensity score matching algorithm performs well.

[Figure 16 here]

Appendix F Spillover effect

F.1 Identification

To ensure the proper identification of spillover effect in our case, there are two identification hypothesis:

1. Parallel trends between the treatment group and the municipalities further away from the refugee center but still within the commuting zone (which act as a kind of "super control" group).

2. Parallel trends between municipalities in the commuting zone close and further away from the refugee center (as municipalities closer to the center serve as a second treatment group and are compared to the "super control" group).

We take into account potential spillover effects assuming it evolves homogeneously and linearly with distance.

F.2 Estimation

In this subsection, we look at spillover effects from the opening of a refugee center within a commuting zone. To do so, we plot the evolution of the coefficients of the interaction between categories of distance to the hosting municipality and event time. Effectively, it measures how the opening of a housing center ripple through the neighboring area. Native avoidance may also affect neighboring municipalities of refugee-hosting municipality.

[11] The result of our approach with one and ten nearest neighbors is available in Appendix I

The following figures show how the interaction terms between distance and event time for four categories (ν_{ilg} in our model) evolve over time relatively to the opening for residents and workers. Overall, like in previous instances, we capture no effect significantly different from zero when looking at workers (Figure 18). However, the number of residents in municipalities more than ten kilometers away from the refugee center increased by about one percent compared to closer municipalities while they follow similar trend before the opening (Figure 17). We interpret this as an evidence that after the opening, residents move from the hosting town to go the municipalities further away from the housing center. It is a confirmation of the need to control for potential spillover effects.

[Figure 17 here]

[Figure 18 here]

To go further in this direction, we present in Table 15 an alternative estimation methodology to take into account potential spillover effect. We estimate as before equation (1) but $Opening_{it}$ is now equal to one for the hosting municipalities after the refugee-center opening but also for all municipalities less than 10 kilometers away. When we do so, we obtain similar coefficients than in Tables 1 and 9 even though we probably underestimate of the effect of refugee center openings (because we consider as treated municipalities that are less likely to be impacted by the opening).

[Table 15 here]

The choice of a 10 kilometer cutoff may seem arbitrary but this methodology has the advantage of not assuming any particular parametric form to the spillover effect on the contrary of our preferred estimation method. Moreover, in Figure 19, we plot the coefficients of the interaction terms between $Opening_{it}$ and d_{ig} when taking $d_{ig} = 10km$ as a base level. We notice that there is a real divergence between municipalities less and more than 10 kilometers away from the center. This is what motivated us to choose this particular distance cutoff.

[Figure 19 here]

Appendix G Main types of crime and misdemeanour

[Table 16 here]

Appendix H Another amenity shock: day-care centers closures

Our methodology has its limitations. It assumes that after only two years an equilibrium has been reached, that an amenity shock has no impact on productivity, or that housing prices alone would be able to offset the welfare loss due to the opening of housing centers. While evaluating these figures, these vulnerabilities must be taken into account. For the sake of comparison, we replicate this method to compute the welfare cost of closing day-care centers in rural France.

As a sanity check for our methodology, we try to use it to study the welfare effect of the closings of day-car centers. The approach is similar to the one we used throughout the paper: we conduct an event analysis at the municipality level to compare how population (the number of fiscal households according to the IRCOM dataset here) evolve at the time of the day-care center closing compared to a control group of municipalities. There were about 245 day-car centers closing between 2004 and 2012, so we have 245 different event groups (more than two times the number of event groups for our study of refugee center openings).

[Table 17 here]

Table 17 can be read the same way as Table 2, its coefficients represents the average population decrease after two years in municipalities that experienced a day-care center closing relatively to matched municipalities outside the commuting zone (column 1) and municipalities within the commuting zone accounting for spillover effect (column 3). Population decreased by almost 1.5 percent. The magnitude of the population effect is similar to the one a migrant housing center opening. The average hosting municipality accounts for 0.05% of the French population. It means that one closing reduces on average aggregate welfare growth by 7.5×10^{-5} points. This is 75% of the effect of a refugee center opening. Please remember that the validity of this estimate is subject to the identification assumptions exposed before. If the no spillover assumption is not met then we might underestimate or overestimate the welfare cost of the closing and if the parallel trend assumption is not met, we are not sure which local shock we are studying the effect of.

Appendix I Other matching algorithms

[Table 18 here]

Appendix J Local tax rates

[Table 7 here]

In Table 7, we see that the housing and property tax rates do not decrease following the opening of a housing center. This confirms that the reduction in the revenues from the housing and property taxes must come from a decrease in the tax-base.

Appendix K Identification problem

[Figure 20 here]

In Figure 20, we give a representation of the classical identification problem with a directed acyclic graph[12] (DAG). Each node represents a variable or a group of variables and each arrow a causal relationship between them.

In this paper, we want to study the direct effect of the opening of a refugee center (the treatment) on several local outcomes. To do so, we cannot directly compare municipalities with and without openings. Unobserved or not, confounders are municipality characteristics that may affect both the opening of a refugee housing center and local outcomes. It may generate spurious correlations between the two groups of variables (Treatment $\leftarrow$ Unobs. confounders $\rightarrow$ Outcome). Our identification strategy has then to condition for potential confounders, e.g. past economic performance, local economic shocks, etc...

Yet, it should not control for potential mediator variables. The opening of a refugee center may affect the local economic environment and even other variables: local politics, demography, the provision of public goods, etc... They may in turn affect local outcomes. Conditioning on these variables would partial out the measured effect of the opening from these indirect paths.

To finish with, the identification strategy should also not control for potential collider variables. That is to say, other municipality characteristics affected by the opening but also by the evolution of local outcomes (population, local wage, ...), like the workforce

[12] See Pearl (2009) and Imbens (2019) for a more thorough discussion on the relevance of such approach in social sciences.

composition or the municipalities tax base. To control for them may induce a spurious bias between the opening and local outcomes. For example, if size had no significant correlation with marking ability in the NBA, it would not not mean that size does not help to score basketball points but that smaller NBA basketball players can compensate with other skills. To sum up, controlling for collider and mediator variables is a threat to identification and any post-opening local variable could be suspect of being part of these two groups.

To solve these issues, we exploit our panel of hosting and non-hosting municipalities from 2002 to 2014. We control for confounders by comparing hosting and non-hosting municipalities before and after the opening. Not all non-hosting municipality can be part of the control group: we compare each hosting municipalities to other municipalities within their commuting zone and only look at how outcome trends evolve before and after the opening of the center. We do so because our identifications assumptions are then more credible.

[Figure 21 here]

The identification problem can be further refined as shown in Figure 21 in a panel with two periods (pre-treatment in $t-1$ and post-treatment in t) and two types of unit (hosting municipalities (1) and non-hosting ones (2)). This is the approach of most event analysis and difference-in-difference strategy. Figure 21 was inspired by the one-way fixed effect model representation of Kim (2019) and is just an extension to two-way fixed effects. The identification of β (the treatment effect) requires that all non-causal paths are blocked by conditioning on middle-variables on the non-causal paths. By definition, unobserved confounders cannot be controlled for, which means the causal path |Center opening in $t \leftarrow$ Unobserved cofounders $\rightarrow Y_{t,1}$ | cannot be blocked.

The linear structural models which decribes how the different variables are linked together in Figure 21 can be written as follow:

- $Y_{t,1} = \beta.T_{t,1} + \gamma_1.U_i + \delta.t$
- $Y_{t-1,1} = \gamma_1.U_i + \delta.(t-1)$
- $Y_{t,2} = \gamma_2.U_i + \delta.t$
- $Y_{t-1,2} = \gamma_2.U_i + \delta.(t-1)$

To control for unobserved cofounders, one solution is to use a first difference estimator $(Y_{t,1} - Y_{t-1,1})$ to recover β. It is possible if the repeated outcomes are affected by the same unobserved cofounders to the same extent (γ_1). However, doing so is problematic because the first difference will also capture the effect of time on outcome Y (as $Y_{t,1} - Y_{t-1,1} = \delta + \beta$). To control for it, we need to do another difference with the first difference for a group of unit which is not experiencing a center opening $(Y_{t,2} - Y_{t-1,2} = \delta)$. This double differences will then be an estimator of β (as $(Y_{t,1} - Y_{t-1,1}) - (Y_{t,2} - Y_{t-1,2}) = \beta$).

To sum up, there are four identification hypothesis:

1. Repeated outcomes are affected by the same unobserved cofounders to the same extent $(Y_{t,i} - Y_{t-1,i} = \gamma_i + \delta$ i.e. common trend assumption for unit fixed effects).

2. Control and treatment units are following the same time trends $(Y_{t,2} - Y_{t-1,2} = \delta$ i.e. common trend assumption for time fixed-effects).

3. There is no causal paths between Center opening in t and $Y_{t,2}$ (no spillover of the opening on non-hosting municipalities).

4. There is no causal paths between Center opening in t and $Y_{t-1,1}$ (no anticipation of the opening in hosting municipalities)[13].

[13] During our period of analysis, refugee hosting was not at heart of news as shown in Figure 14. It makes this assumption likely.

7 Tables

Table 1: Effect of a refugee center opening on the resident employed population

	Log number of employed residents in the municipality				
	(1)	(2)	(3)	(4)	(5)
$Opening_{it}$	-.09618***	-.08568***	-.01721**	-.01074*	-.02113***
	(.00828)	(.00818)	(.00734)	(.00642)	(.00482)
Method	DD	DD	Matching	EA	EA
Observations	5567538	5565114	18461	840633	840633
R^2	0.992	0.993	0.999	0.999	0.999

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. "DD" stands for Difference-in-Difference and "EA" stands for Event Analysis. Weighted by the population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects in columns (2) to (5). An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA and DD, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (5). Reading: According to column (5), after the opening of a refugee center, the number of employed residents decrease by 2,1 % in refugee-hosting municipalities compared to other municipalities within the same commuting zone.

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Table 2: Effect of a refugee center opening on the fiscal population

	Log number of fiscal households in the municipality		
	(1)	(2)	(3)
$Opening_{it}$	-.0121***	-.0258***	-.0261***
	(.0032)	(.0027)	(.0027)
Method	Matching	Event Analysis	Event Analysis
Observations	3064	98370	98370
R^2	0.999	0.999	0.999

Source: IRCOM 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for the event analysis, or of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

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Table 3: Effect of a refugee center opening on inflows and outflows of employed residents

	Inflows	**Outflows**
	(1)	(2)
$Opening_{it}$	-.0282***	-.0149
	(.0098)	(.0104)
Method	Event Analysis	Event Analysis
Observations	86695	86695
R^2	0.997	0.997

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. Inflows corresponds to the *log* number of employed residents that move in the municipality. Outflows corresponds to the *log* number of employed residents that move out of the municipality. Standard errors are clustered at the municipality level. Weighted by population two years before the opening. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone. We control for spillovers as in Clarke (2017).

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Table 4: Effect of a refugee center opening on retirees and taxed households

	Number of retiree households			**Number of taxed households**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Opening_{it}$	-.0173***	-.0313***	-.0210***	-.0281***	-.0491***	-.0541***
	(.0040)	(.0038)	(.0038)	(.0046)	(.0041)	(.0041)
Method	Matching	EA	EA	Matching	EA	EA
Observations	2935	96802	96802	2919	97048	97048
R^2	0.999	0.999	0.999	0.999	0.999	0.999

Source: IRCOM 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of fiscal households receiving pensions, retirements, or annuities in the municipality in columns (1) to (3), and the *log* number of fiscal households that pay the income tax in the municipality in columns (4) to (6). "EA" stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in columns (3) and (6).

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Table 5: Effect of a refugee center opening on firms outcomes

	Value-added		**Sales**		**Number of firms**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Opening_{it}$	-.0290	-.0285*	-.0391	-.0572***	-.0036	-.0292***
	(.0183)	(.0160)	(.0267)	(.0215)	(.0132)	(.0100)
Method	Matching	EA	Matching	EA	Matching	EA
Observations	2405	77150	2400	76733	2407	77498
R^2	0.996	0.975	0.994	0.967	0.998	0.982

Source: FICUS-FARE 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* total value added of firms in the municipality in columns (1) and (2), *log* total value of firms' sales in the municipality in columns (3) and (4), the *log* number of firms in the municipality in columns (5) and (6). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

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Table 6: Effect of a refugee center opening on local taxes

	Tax base		**Local taxes**		**Housing tax**		**Property tax**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Opening_{it}$	-.0143	-.0556***	-.00868	-.0334***	-.0307**	-.0592***	-.0145*	-.0360***
	(-.0136)	(-.0045)	(.0118)	(.00841)	(.0108)	(.00673)	(.00721)	(.00585)
Method	Matching	EA	Matching	EA	Matching	EA	Matching	EA
Obs.	3064	98364	2830	117095	2830	117095	2830	117095
R^2	0.996	0.999	0.998	0.996	0.998	0.998	0.999	0.998

Source: IRCOM 2002-2014 in (1) and (2) and DGFIP 2002-2014 in (3) to (8). Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* average tax reference income in the municipality in columns (1) and (2), the *log* amount of local taxes that are levied by local authorities in the municipality in columns (3) and (4), the *log* amount of housing taxes that is levied by the municipality in columns (5) and (6), the *log* amount of property taxes that is levied by the municipality in columns (7) and (8). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

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Table 7: Effect of a refugee center opening on local tax rates

	Housing tax rate		**Property tax rate**	
	(1)	(2)	(3)	(4)
$Opening_{it}$	-.0179	-.0144	.00135	.00192
	(.00965)	(.00811)	(.00569)	(.00669)
Method	Matching	EA	Matching	EA
Observations	2830	117170	2830	117170
R^2	0.971	0.949	0.991	0.968

Source: DGFIP 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the housing tax rate that is applied in the municipality in columns (1) and (2) and the property tax rate that is applied in the municipality in columns (3) and (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

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Table 8: Effect of a refugee center opening on hires of foreign workers

	Log number of foreign workers hired		
	(1)	(2)	(3)
$Opening_{it}$	.0108	.0013	-.0062
	(.0142)	(.0054)	(.0111)
Method	Matching	EA	EA
Observations	3366	31246	31246
R^2	0.579	0.461	0.462

Source: DMMO 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. "EA" stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

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Table 9: Effect of a refugee center opening on resident and non-resident workers

	Hourly wage		**Number of workers**	
	(1)	(2)	(3)	(4)
$Opening_{it}$	.00534	.00039	-.02036	-.00365
	(.00662)	(.00578)	(.01619)	(.01104)
Method	Matching	EA	Matching	EA
Observations	16599	707348	16602	707645
R^2	0.838	0.987	0.996	0.999

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* average hourly wage of residents and non-resident workers in the municipality in columns (1) and (2), and the *log* number of residents and non-resident workers in columns (3) and (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

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Table 10: Effect of a refugee center opening on non-resident workers

	Log number of non-resident workers		
	(1)	(2)	(3)
$Opening_{it}$	-.02562	-.00319	.00395
	(.01885)	(.00829)	(0.01272)
Method	Matching	EA	EA
Observations	17072	693657	683510
R^2	0.995	0.998	0.998

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. "EA" stands for Event Analysis. Weighted by population two years before the opening. We control for event group fixed effects and standard errors are clustered at the municipality level. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

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Table 11: Effect of a refugee center opening on crimes in the department

	Total	**Murders**	**Thefts**	**Sexual Assaults**	**Drugs**
	(1)	(2)	(3)	(4)	(5)
$Opening_{dt}$	.0002	-.0047	.0021	.0154	-.0340**
	(.0044)	(.0395)	(.0045)	(.0136)	(.0152)
Observations	14361	14361	14361	14361	14361
R^2	0.984	0.436	0.984	0.832	0.830

Source: French Ministry of the Interior. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of crimes committed in the department in column (1), the *log* number of murders in (2), the *log* number of thefts in (3), the *log* number of sexual assaults in (4), the *log* number of drug-related crimes in (5). Standard errors clustered at the departmental level. Reading: According to column (1), after the the opening of a refugee center, the total number of crimes increased on average by 0.02 % in refugee-hosting departments compared to other departments after the openings.

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Table 12: Effect of a refugee center opening on residents employed by skill and income

	Highly skilled		**Low-skilled**		**Low income**		**High income**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Opening_{it}$	-.0196**	-.0247***	-.0109	-.0226***	-.0171*	-.0163**	-.0146*	-.0214***
	(.0082)	(.0064)	(.0108)	(.0064)	(.0097)	(.0064)	(.0075)	(.0067)
Method	Matching	EA	Matching	EA	Matching	EA	Matching	EA
Obs.	18551	844922	18205	856735	18542	855284	18444	853153
R^2	0.998	0.999	0.998	0.999	0.998	0.999	0.998	0.999

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of resident employed in high-skilled jobs (professionals, executives and middle managers, PCS 3 and 4 in the French professional categories) in columns (1) and (2), the *log* number of residents employed in low-skilled jobs (manual workers and employees PCS 5 and 6 in the French professional categories) in columns (3) and (4), the *log* number of employed residents with an income below the national median wage in columns (5) and (6), the *log* number of employed residents with an income above the national median wage in columns (7) and (8). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

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Table 13: Hosting vs. matched municipalities 2 years before the openings

	Matched municipalities	**Hosting municipalities**	**P-value**
Unskilled (%)	41.3	42.0	0.845
Rural (%)	8.7	7.2	0.641
Unemployed (nb)	1663	2088	0.272
Active (nb)	9094	11324	0.213
Men (%)	50.9	50.5	0.694
Population	33430	38934	0.389
Migrants	3997	4928	0.254
Vacant buildings (nb)	1454	1563	0.729
Observations	485	98	

Source: Census 2002-2010. The table compare municipalities which experienced a refugee housing center opening between 2004 and 2012 with their matched municipalities two years before the opening.

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Table 14: Hosting vs. other municipalities in 2002

	Other municipalities	**Hosting municipalities**	**P-value**
Unskilled (%)	47.5	44.2	0.003
Rural (%)	50.1	7.2	0.000
Unemployed (nb)	63	2134	0.000
Active (nb)	533	11951	0.000
Men (%)	50.3	47.9	0.000
Population	1611	39377	0.000
Migrants	93	3158	0.000
Vacant buildings (nb)	55	1515	0.000
Observations	34536	98	

Source: Census 2002. The table compares municipalities that experienced a refugee housing center opening between 2004 and 2012 with all other municipalities in 2002.

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Table 15: Effect of a refugee center opening on the population employed in the municipality and in municipalities within a 10-km vicinity

	Residents	**Residents' wage**	**Workers**	**Workers' wage**
	(1)	(2)	(3)	(4)
$Opening_{it}$	-.0090***	.0008	-.0072*	-.0003
	(.0015)	(.0008)	(.0037)	(.0018)
Observations	871177	871177	718231	717922
R^2	0.999	0.996	0.999	0.987

Source: DADS 2002-2014 Note: *** p<0.01, ** p<0.05, * p<0.1. We estimate as equation (1) with $Opening_{it}$ is now equal to one after the refugee-center opening for hosting municipalities and municipalities less than 10 kilometers away. The dependent variable is the *log* number of employed resident in the municipality in column (1), the *log* average hourly wage of employed residents in (2), the *log* number of employed resident and non-residents in the municipality in (3), the *log* average hourly wage of employed residents and non-residents in (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone. In this Event Analysis, we control for spillovers as in Clarke (2017).

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Table 16: Main type of crimes by category

Category	Type of crime or misdemeanours
Murder or murder attempts	Murder for robbery and theft
	Murder for other reasons
	Attempted homicides to steal and during robberies
	Attempted homicides to steal and during robberies
	Assault and battery followed by death
Theft	Violations of residence
	Armed robberies with firearms against financial institutions
	Armed robberies with firearms against industrial or commercial establishments
	Armed robberies with firearms against CIT companies
	Armed robberies with firearms against individuals in their homes
	Other armed robberies with firearms
	Edged-arms robberies against financial, commercial or industrial institutions
	Flights with edged weapons or by destination against private individuals in their homes
	Other thefts with knives or by destination
	Theft with unarmed violence against financial, commercial or industrial establishments
	Theft with unarmed violence against individuals in their homes
	Theft with unarmed violence against women on public streets or other public places
	Theft with unarmed violence against other victims
	Burglary of main living quarters
	Burglaries of second homes
	Burglary of industrial, commercial or financial premises
	Burglaries from other places
	Flights with trickery into any place
	Pickpocketing
	Shoplifting
	Cargo transport vehicle theft
	Auto theft
	Two-wheeled motor vehicle theft
	Caravan flights
	Theft of accessories on registered motor vehicles
	Simple flights on site
	Simple theft on farms
	Other simple thefts against public or private establishments
	Other simple thefts against private individuals in private premises
	Other simple thefts against individuals in public premises or places
Sexual assaults of any sort	Rape of adults
	Rape of minors
	Sexual harassment and other sexual assaults against adults
	Sexual harassment and other sexual assaults against minors
	Sexual offences
Drugs related	Trafficking and resale without the use of drugs
	Use and resale of drugs
	Use of drugs
	Other drug law offences
Forgeries	Forgery of identity documents
	forgery of documents concerning vehicle traffic
	Other forgery of administrative documents
	Forgery of public and authentic writing
	Other forgeries in writing
Clandestine employment	Clandestine employment
	Employment of foreigners without a work permit

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Table 17: Effect of day-care centers openings on the number of fiscal households

	Log number of fiscal households		
	(1)	(2)	(3)
$Opening_{it}$	-.0134***	-.0260***	-.0149*
	(.0021)	(.0039)	(.0083)
Method	Matching	EA	EA
Observations	6827	133056	133056
R^2	1.000	0.999	0.999

Source: IRCOM 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. "EA" stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

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Table 18: Effect of a refugee center opening on the resident employed population with other matching algorithms

	Log number of employed residents	
	(1)	(2)
$Opening_{it}$	-.00151	-.01301*
	(.00684)	(.00682)
Method	No replacements	10 nearest neighbors
Observations	8416	26698
R^2	1.000	0.999

Source: DADS 2002-2014. Note: *** p<0.01, ** p<0.05, * p<0.1. The table reproduce the approach of the column (3) in Table 1 with two different matching algorithms: in (1), a propensity score matching to the nearest neighbor with no replacements and in (2), a matching to the 10 nearest neighbors with replacements. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and matched municipalities.

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8 Figure

Figure 1: Housing centers in France

(a) Housing centers localisation and migratory routes

(b) Refugee center openings by commuting zones between 2004 and 2012

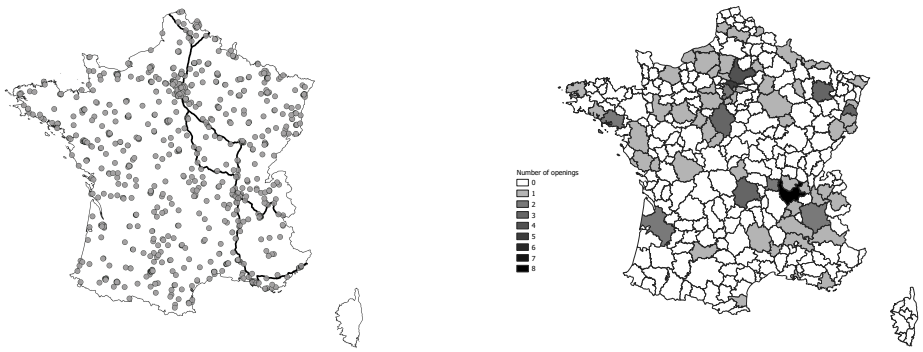

Figure 1a shows the location of all housing center for refugee in France in April 2018 and the migratory routes (from IOM - monitoring flows). Figure 1b is a map of the French commuting zones. White areas are commuting zones that did not have any opening between 2004 and 2012. Colored areas are commuting zones that had at least one opening during the period.

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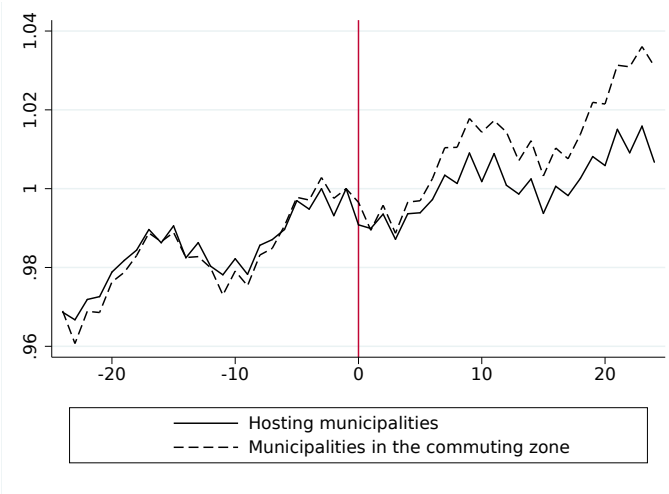
Figure 2: Number of employed residents in hosting and non-hosting municipalities



Source: DADS 2002-2014. Note: The solid line represents the evolution of the number of employed residents in hosting municipalities and the dashed line the evolution within other municipalities from the same commuting zone. They are standardized to one the month before the refugee housing center is opened. Time series are deseasonalized for better readability.

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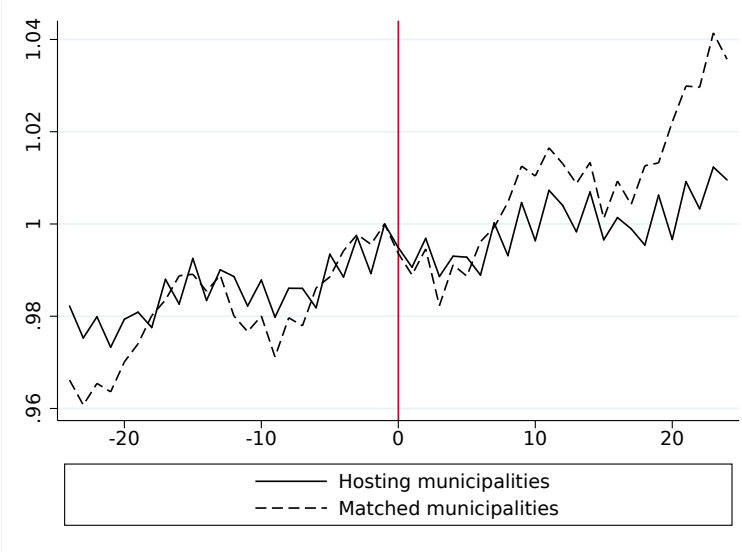
Figure 3: Number of employed residents in hosting and matched municipalities



Source: DADS 2002-2014. Note: The solid line represents the evolution of the number of employed residents in hosting municipalities. The dashed line represents the evolution of the number of employed residents in matched municipalities. They are normalized to one month before the opening of the refugee housing center. Time series are deseasonalized for better readability.

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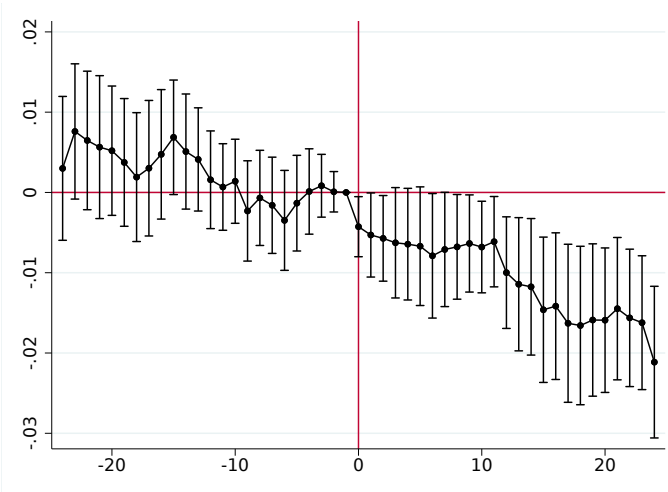
Figure 4: Dynamic effect of a refugee center opening on the resident employed population



Source: DADS 2002-2014. Note: Estimated β_l from equation (3) where l (x-axis) is the number of months relative to the opening of the refugee housing center and where the outcome is the number of residents employed. The incertitude of each point is asserted with a 95% confidence interval. Reading: Twenty-four months after the opening, the number of working residents has decreased by about 2 percent in refugee-hosting municipality compared to the other municipalities within their commuting zone.

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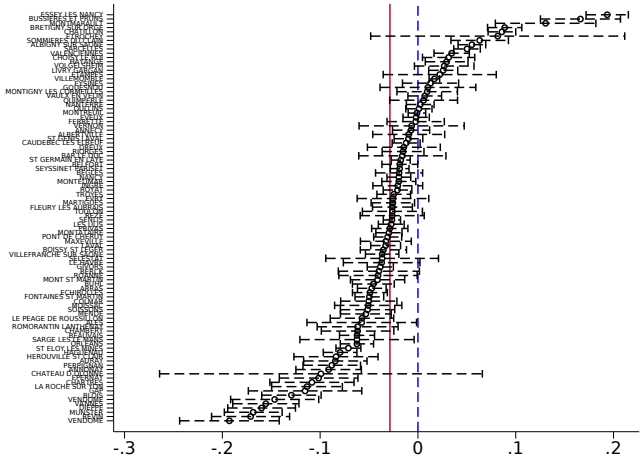
Figure 5: Effect of a refugee center opening on the resident employed population by event



Source: DADS 2002-2014. Note: Distribution of the estimated β_g from equation (2) in each event group g. An event group is composed of a hosting municipalities and of other municipalities in the same commuting zone. It is referenced here by the name of the hosting municipality. The red line marks the average treatment effect on the treated.

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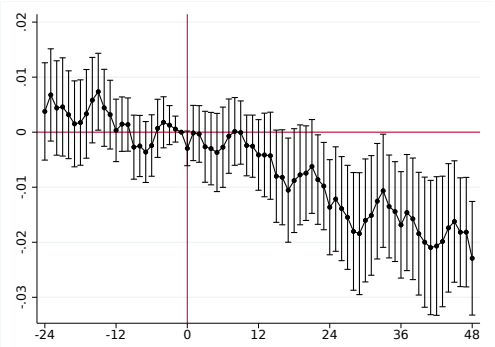
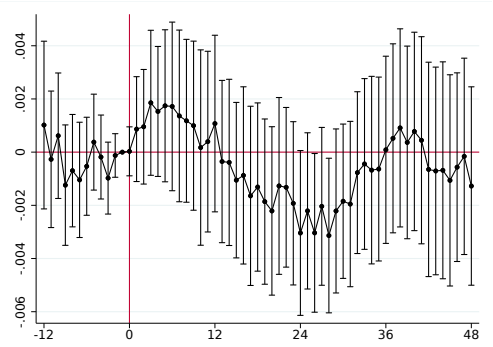
Figure 6: Longer-time dynamic effect of a refugee center opening on the resident employed population

(a) Number of employed residents



(b) Year-to-year Growth



Source: DADS 2002-2014. Note: Estimated β_l from equation (3) where l (x-axis) is the number of months relative to the opening of the refugee housing center and where the dependent variable is either the number of residents employed or its year-to-year growth. Compared to Figure 4, the period of analysis has been extended to 4 years after the opening of a refugee center. The incertitude of each point is asserted with a 90% confidence interval.

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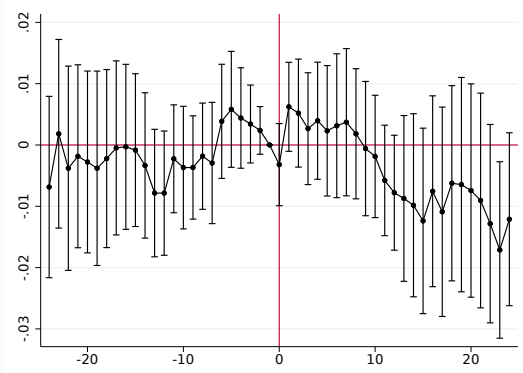
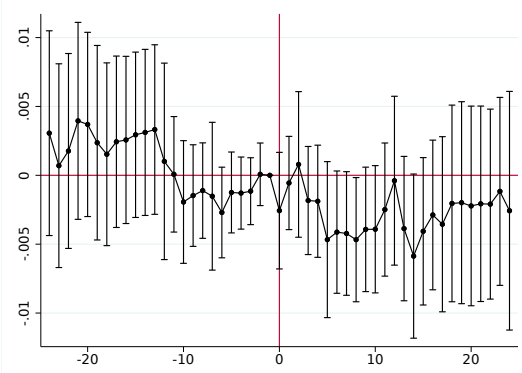
Figure 7: Dynamic effect of a refugee center opening on the local labor market

(a) Log number of workers



(b) Hourly wage



Source: DADS 2002-2014. Note: Estimated β_l from equation (3) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of residents and non-resident workers and their *log* hourly wage. The incertitude of each point is asserted thanks to a 95% confidence interval.

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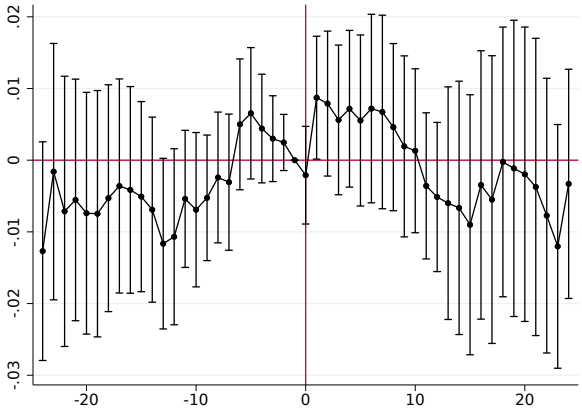
Figure 8: Dynamic effect of a refugee center opening on the number of non-resident workers



Source: DADS 2002-2014. Note: Estimated β_l from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the log number of non-resident workers. The incertitude of each point is asserted thanks to a 95% confidence interval.

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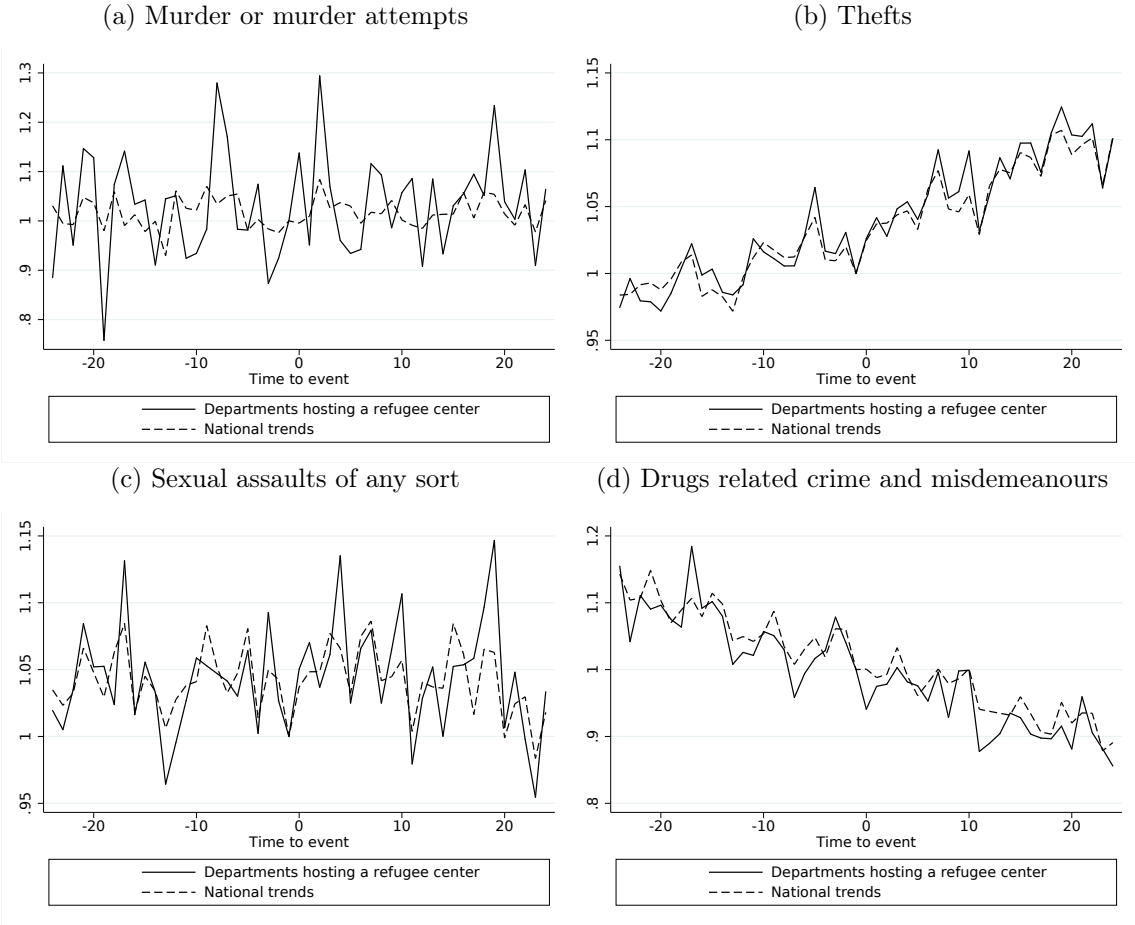
Figure 9: Event analysis of the effect of a refugee center opening on crimes



Source: Ministry of the Interior 2002-2014. Note: Comparison of the evolution of different types of crimes or misdemeanours in departments that hosted a refugee center (solid line) and in other departments (dashed line). The number of crimes is normalized to one the month before the opening of the refugee center.

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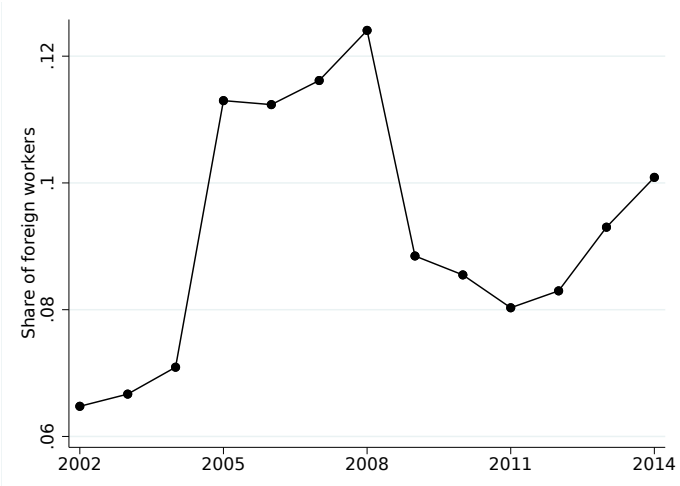
Figure 10: Number of foreigners in the DADS



Source: DADS 2002-2014. Note: Share of workers identified as foreigners in the DADS from 2002 to 2014 in all municipalities.

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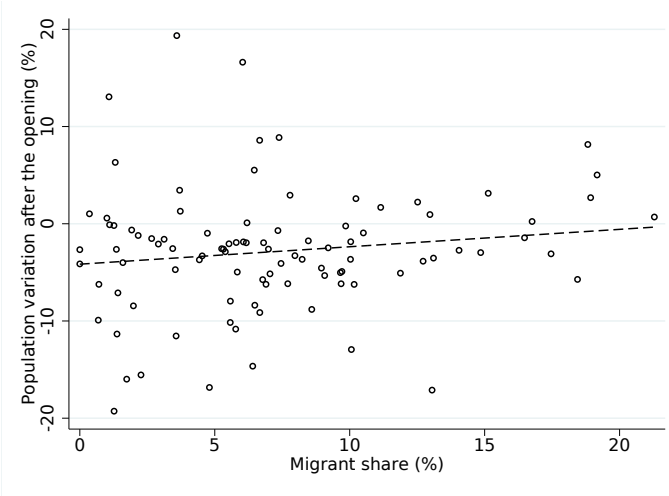
Figure 11: Effects on the population and migrant shares



Source: DADS & Census 2002-2010. Note: The figure relates the migrant share two years before the opening with the municipality treatment effect. Reading: When the migrant share amounts to 5%, the population in the municipality decreases on average by 2%.

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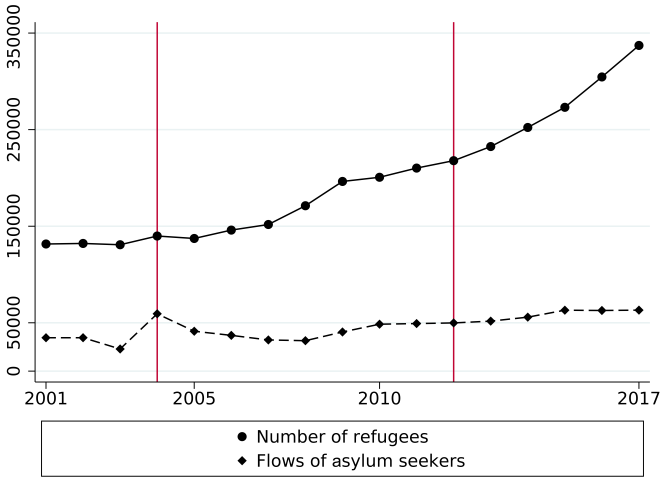
Figure 12: Humanitarian migration between 2001 and 2017 in France



Source: OFPRA. Note: The figure presents the number of refugees (solid line) and asylum seekers (dashed line) on a yearly basis between 2001 and 2017.

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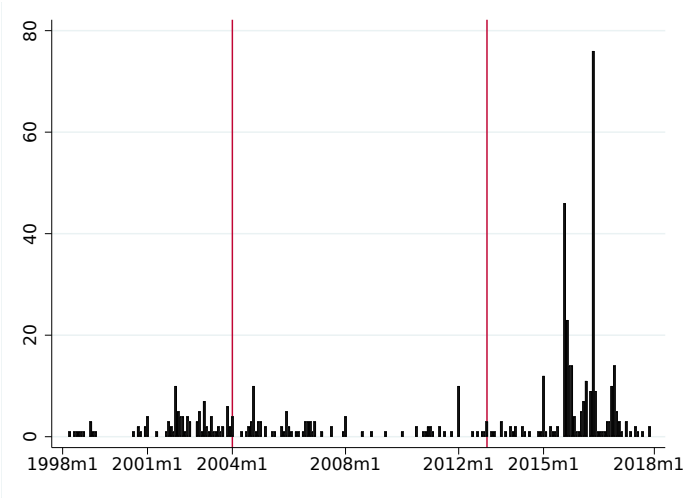
Figure 13: Refugee center openings between 1998 and 2018 in France



Source: Authors' computations. Note: The figure represents the number of refugee center openings on a monthly basis between January 1998 and December 2018.

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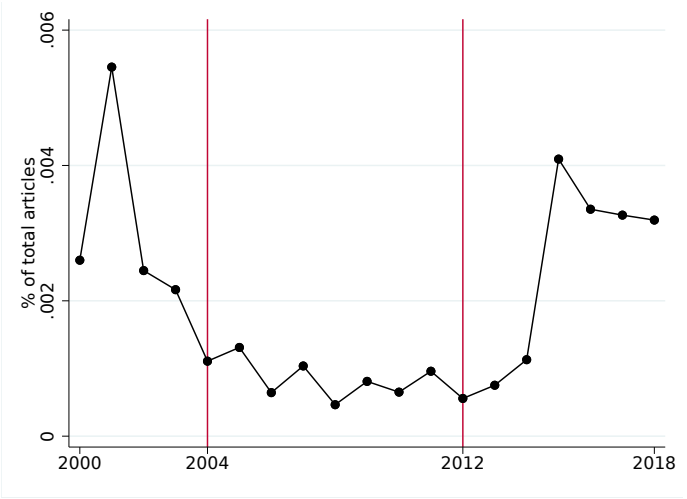
Figure 14: Refugees and housing centers occurrences in French newspapers



Source: Europresse. Note: The figure represents the proportion of articles in the national and regional press that include the keywords "Centre d'hébergement" and "réfugié".

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Figure 15: Different population definitions for a given municipality A

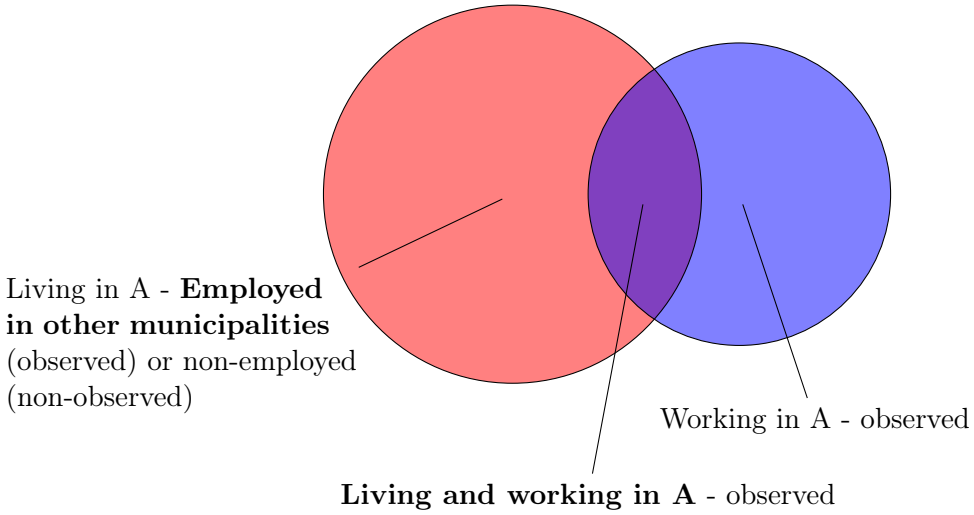

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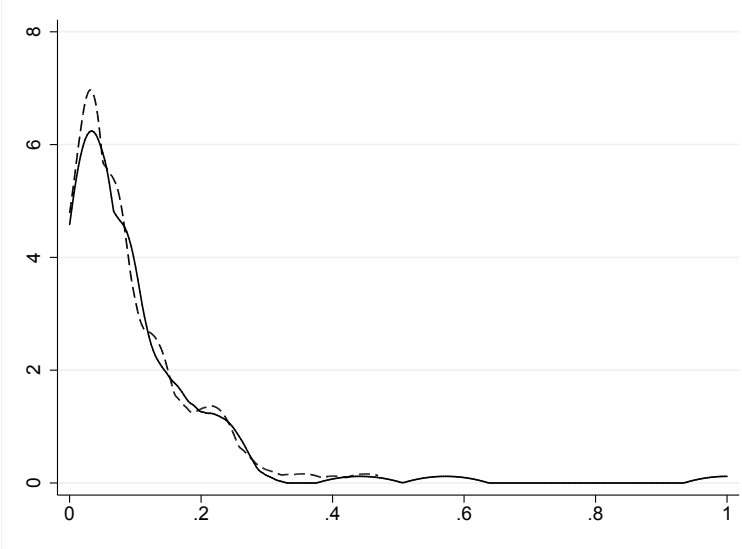
Figure 16: Common support of the matching specification



Source: Census 2002. Note: The solid line represents the distribution of the propensity score when a matching algorithm with five nearest neighbors is used with the 2002 census variables. The dashed line shows the distribution of the propensity score for municipalities selected in the control group.

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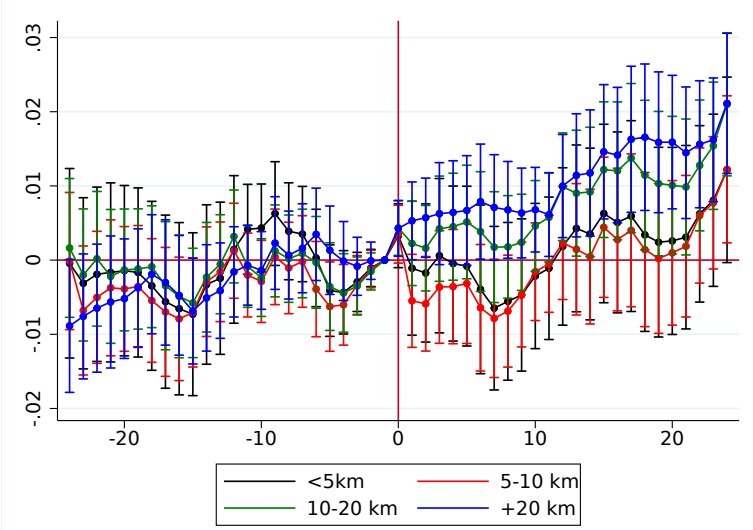
Figure 17: Spillover effects of a refugee center opening on the resident employed population



Source: DADS 2002-2014. Note: Estimated ν_{ilg} from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of employed residents. The incertitude of each point is asserted thanks to 95% confidence interval.

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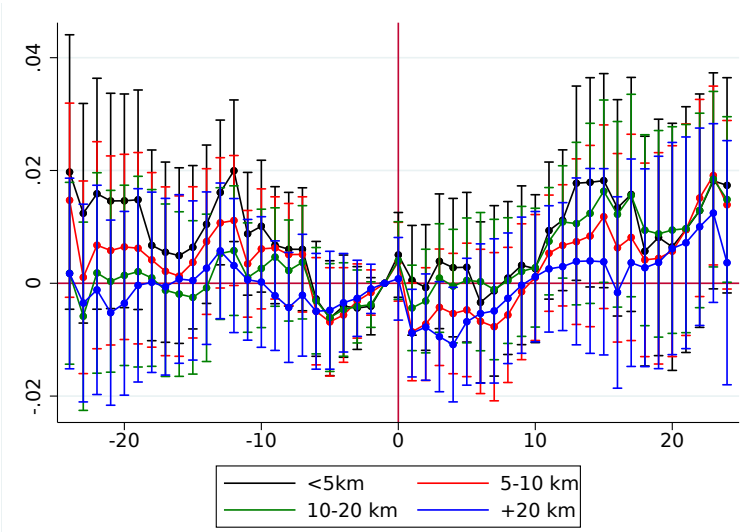
Figure 18: Spillover effects of a refugee center opening on the number of workers



Source: DADS 2002-2014. Note: Estimated ν_{ilg} from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of workers. The incertitude of each point is asserted thanks to 95% confidence interval.

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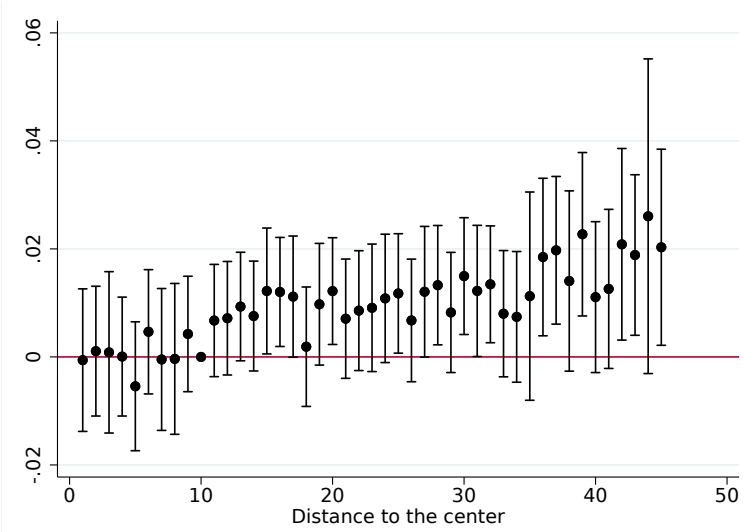
Figure 19: Effect of the distance of non-hosting municipalities to the refugee center



Source: DADS 2002-2014. Note: Estimated $d_{ig} \times Opening_{it}$ from Equation (1) when taking $d_{ig} = 10$ as a base level. Reading: The number of employed residents has increased by 2 percent more in municipalities 45 kilometers away from the refugee center compared to municipalities 10 kilometers away.

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Figure 20: Identification problem – A Classical Problem – DAG

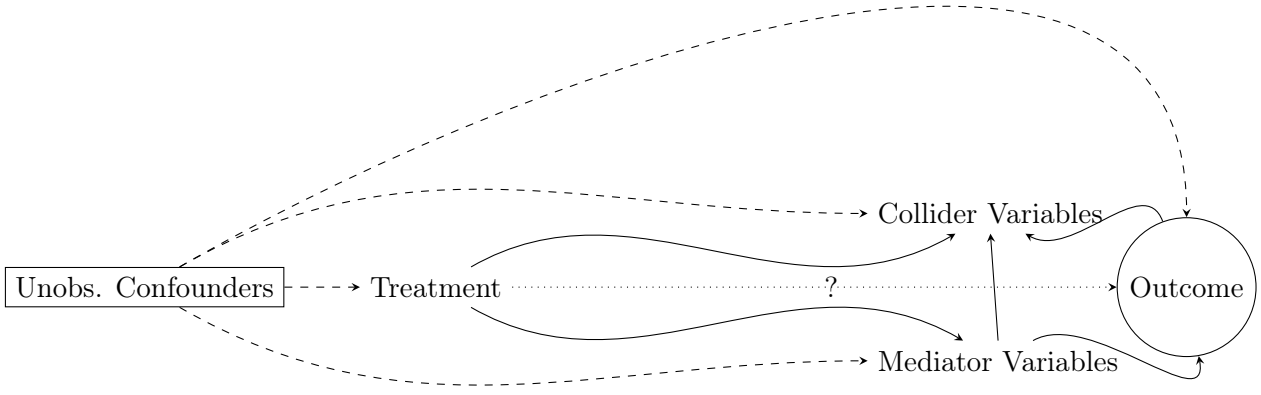

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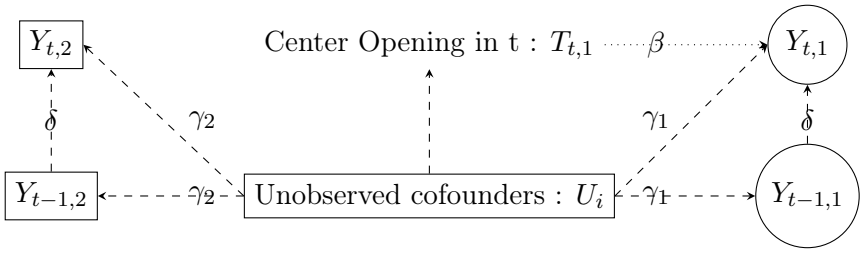
Figure 21: Identification problem - Two-way fixed effects estimator – DAG



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