Hosting Refugees and Voting for the Far-Right: Evidence from France

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Hosting Refugees and Voting for the Far-Right: Evidence from France*

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Abstract

Does exposure to refugees change the political preferences of natives towards far-right parties, and how does this change in preferences occur? This paper examines the political economy of refugee-hosting. Using the opening of refugee centers in France between 1995 and 2017, I show that voting for far-right parties in cities with such opening between two presidential elections has fallen by about 2 percent. The drop in far-right voting is higher in municipalities with a small population, working in the primary and secondary sectors, with low educational levels and few migrants. I show that this negative effect can not be explained by an economic channel, but rather by a composition channel, through natives' avoidance, and a contact channel, through natives' exposure to refugees. I provide suggestive evidence that too-disruptive exposure to refugees, as measured by the magnitude of the inflows, the cultural distance and the media salience of refugees, can mitigate the beneficial effects of contact on reducing far-right support.

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I. Introduction

Since 2015, Europe has been confronted with a migration crisis characterized by large refugee influxes. The European Union alone received 366,000 new refugees out of a total of 1.2 million asylum seekers in 2016. This surge in refugee and asylum seeker inflows has revived the public's concerns over hosting refugees and these fears have been used and fuelled by the long-term anti-immigrant extreme-right parties. Concurrently, Europe experienced a rise in extreme-right voting with parties such as the *AfD* in Germany, the

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FPö party in Austria, the *Lega* in Italy, the Golden dawn party in Greece, or the *Rassemblement National* in France gaining prominence.

In this paper, I focus on the native population directly exposed to refugee inflows and answer the following question: does exposure to refugees change natives' political preferences towards far-right parties, and how does this change in preferences happen? On the one hand, direct exposure to refugees could lead to an extreme-right vote due to the actual or feared economic and fiscal costs of immigration (Becker et al., 2016), negative externalities on the neighbourhood of residence (Halla et al., 2017a), on the educational environment of native children (Otto and Steinhardt, 2014a), or on natives' culture (Hainmueller and Hopkins, 2014). On the other hand, intergroup contact between natives and refugees could decrease natives' prejudice (Allport, 1954) by reducing the distance of practices and situations between the two groups (Agier, 2013). As Allport (1954) who complements his contact hypothesis by a set of prerequisites[1], I will undertake my analysis from the premise that it is not just any kind of contact that can lead to a change in immigration and political preferences (Valentine, 2008).

I use the opening of refugee centers in 446 municipalities in France between 1995 and 2017 to identify the effect of small-scale humanitarian inflows on the far-right vote at presidential elections. Using a difference-in-difference strategy in a staggered adoption design, I compare the evolution of extreme-right voting in both hosting and non-hosting municipalities before and after the opening of a refugee center. The opening of a refugee center is a good experiment to examine the political consequences of exposure to humanitarian migrants because they do not choose the centers to which they are assigned, but also because the process of opening refugee centers is centralized, leaving municipalities with very little discretion over the opening. I also use various specification strategies, using a control group of municipalities matched on observable characteristics, using a control group of municipalities that will open a refugee center at a later date, looking at variations within municipalities using data at the polling station level, and performing several robustness tests, to address potential identification concerns. This enables me to derive the causal impact of the opening of refugee centers on the extreme-right vote. I then use the detailed information on the center's openings to analyze the channels that can explain such an effect, and under which conditions the contact hypothesis channel is maximized.

On average, the vote-share for the extreme-right at presidential elections decreases by 1.8 to 3.7 percent following the opening of a refugee center in hosting municipalities compared to non-hosting municipalities. I show that the effect on the vote for the extreme right two elections after the opening almost doubles so that the vote for the extreme-right drops relatively from 3.7 to 10 percent from about 5 to 10 years after the opening of

[1] *"[Prejudice] may be reduced by equal status contact between majority and minority groups in the pursuit of common goals. The effect is greatly enhanced if this contact is sanctioned by institutional supports (i.e., by law, custom, or local atmosphere), and provided it is of a sort that leads to the perception of common interests and common humanity between members of the two groups." Allport (1954)*

a refugee center. I find a sharper decrease in municipalities with a small population, a higher proportion of primary and secondary workers, a lower proportion of highly skilled people, lower incomes, and a lower migrant population. The effect is also stronger in municipalities which voted more for the left-wing party at the beginning of the period, and that the relative decrease of the vote for the extreme-right seems to benefit the left-wing party whose vote-share at presidential elections increases afterwards. This is consistent with the fact that the openings can reduce the existing vote for the extreme right but also prevent it from growing, or in other words, shifting the minds of extreme-right voters but also hindering traditional-party voters from moving to the far-right.

I demonstrate that this decline in far-right voting is not due to an economic demand shock and I am the first to explain this effect through both a composition and a contact channel. I show that even in the absence of a native flight, the results can be partly explained by a native avoidance phenomenon. I then use the characteristics of the centers' openings to investigate how the contact hypothesis is maximized. First, I do not detect any variation in the effect with the duration of exposure to the refugee centre or with the distance from the city-center. I then find suggestive evidence that the drop in the vote for the extreme right is lower in housing centers with a higher capacity relative to the population, with a higher proportion of non-European immigrants, and that opened at at time where the salience of the refugee topic in the national press was higher. In line with the threshold and realistic group conflict theories, these latest findings point to the fact that too-intense or too-disruptive contact, due to higher levels of refugee arrival, higher cultural distances between refugees and the native population, or media salience of the refugee arrival, may not activate the contact hypothesis as well as a more proportionate contact.

This paper adds to the literature on the political economy of immigration in three ways. For immigration in general, a substantial part of the literature finds that immigration can increase anti-immigrant party voting (Otto and Steinhardt, 2014b; Becker and Fetzer, 2016; Barone et al., 2016; Halla et al., 2017b; Harmon, 2018; Dinas et al., 2019; Edo et al., 2019). Some indicate more nuanced findings. Mayda et al. (2018) show that an increase in low-skilled immigrants improves the votes for the Republican Party (more anti-immigration during the period considered), whereas a rise in high-skilled migration has the opposite effect. These studies concentrate primarily on contexts with significant migration inflows whose overall political effect may be explained by a variety of competing factors, such as real or perceived economic risks, changes in attitudes due to contact, compositional effects, etc... A similar statement holds for studies on refugee migration. On the one hand, Dustmann et al. (2019) find an increase in far-right voting in rural Danish municipalities hosting refugees, with opposite effects in urban municipalities. Dinas et al. (2019) show that far-right Golden Dawn votes increased in the Greek islands exposed to massive refugee inflows. Hangartner et al. (2019) corroborate that natives on

Greek islands subject to sudden and large increases in refugee inflows have become increasingly hostile to them. On the other hand, exposure to small-scale refugee inflows due to dispersal policies has been found to have a negative impact on extreme-right wing voting in Austria (Steinmayr, 2016), Italy (Gamalerio et al., 2020), and France (Vertier and Viskanic, 2019), a result that the authors attribute to the contact hypothesis.

This work also supports the contact hypothesis channel. However, unlike the above reviewed literature, this paper finds support for the contact hypothesis once accounted for potential confounding factors such as economic consequences of refugee-hosting or compositional changes (building on previous work by Batut and Schneider-Strawczynski (2019) using the same context), and qualifies it in relationship to the "threshold" (Schelling, 1971; Card et al., 2008; Aldén et al., 2015) and "realistic group conflict" theories (Campbell, 1965; Bobo, 1983; Quillian, 1995; Dustmann et al., 2019). More precisely, it provides comprehensive evidence that the relationship between the contact with refugees and votes for the far-right depends on the intensity of contact, as measured by the capacity of the center, the cultural distance of hosted refugees, or the media salience of the refugee topic. To the best of my knowledge, this paper is the first to provide several pieces of evidences to connect the threshold or the realistic group conflict theories to the contact hypothesis to show that a too-intensive contact may have counteracting effects. This last finding can explain the contrasting results found in the literature on the political economy of refugee migration (Steinmayr, 2016; Dinas et al., 2019; Vertier and Viskanic, 2019).

Second, it contributes to the context on which the literature usually studies the electoral consequences of refugee-hosting. My time frame of analysis of more than twenty years allows to look at small and regular inflows of refugees encompassing both periods of crisis and of normal inflows of refugees. For now, the literature on the political consequences of hosting refugees focus almost exclusively on the context of the 2015 refugee crisis (Dinas et al., 2019; Steinmayr, 2020; Vertier and Viskanic, 2019; Gamalerio et al., 2020). In particular, this an important aspect that differentiate my work from the one of Vertier and Viskanic (2019) as they focus on the French context of emergency relocation of refugees to provisional housing centers for dismantling the Calais Jungle during the refugee crisis and on a short-term exposure to refugees. To my knowledge, the only paper that focuses on the political effects of refugee-hosting outside of the refugee crisis is the one by Dustmann et al. (2019) who look at the effect of refugee hosting in Danish municipalities over the 1986-1998 period. Differently from that paper, I focus the French context on a longer and more recent period from 1995 to 2017 and I find different heterogeneity results for small and rural municipalities. Moreover, these studies focus on the effect of hosting refugees only in subsequent elections and do not have the opening date to account for exposure time, limiting the possibility of studying longer-term patterns. Unlike them, not only can I investigate the role of the exposure time, but I can also robustly estimate the effect on the two subsequent presidential election periods and, combined with data on

European elections, can plot the dynamics of this impact over up to five election periods. Thus, compared to literature, my analysis context enables me to study the short-term and long-term consequences of a representative and typical refugee-hosting pattern.

Finally, this paper provides a methodological contribution to literature on immigration's impact on political outcomes. The first part of the literature uses instrumental variable strategies to circumvent the endogeneity threat from immigrants' location choices. In the spirit of Altonji and Card (1991) and Card (2001), the literature often resorts to a shift-share instrument (Otto and Steinhardt, 2014b; Barone et al., 2016; Halla et al., 2017b; Edo et al., 2019; Lonsky, 2020). For refugee-hosting, the number of buildings suitable for group hosting has been used as an instrument for refugee-centers' location (Steinmayr, 2016; Harmon, 2018; Vertier and Viskanic, 2019; Gamalerio et al., 2020). Using an instrumental variable strategy only provides the Local Average Treatment on the Treated (ATT) (Angrist and Imbens, 1995), that is, the Average Treatment Effect (ATE) only for units that are effected by the instrument, while policy makers may be more interested in the ATE for all units. Another part of the literature on the effect of migration resorts to quasi-experimental settings such as mine in which the exogenous allocation of refugees is used for the identification of the ATE (Edin et al., 2003; Åslund and Rooth, 2007; Damm, 2009; Dustmann et al., 2019). I will use such a setting to estimate the ATE and I contribute to this strand of the literature by using the latest developments in the difference-in-difference econometric literature for the staggered adoption design (de Chaisemartin and d'Haultfoeuille, 2020). I also propose new specification strategies as robustness checks for the identification, such as using as a control group municipalities that have not yet opened a refuge-center or looking at within municipalities variations using polling-station-level data. Finally, to the best of my knowledge, I am the first to perform a dynamic analysis in this context, which is useful not only for distinguishing between short-term and long-term effects, but also to check for the existence of pre-trends.

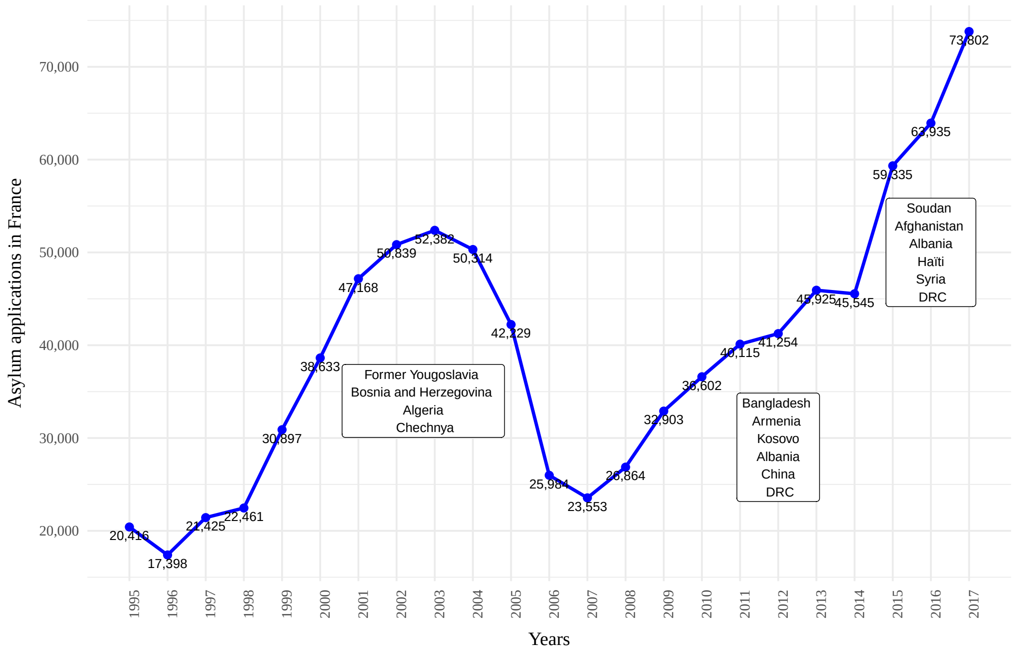
The paper is structured as follows. Section II presents the context and data. Section III details the empirical and identification strategy. Section IV investigates the effect of refugee center openings on the vote for the extreme-right. Section V concludes.

II. Context

II.1. Humanitarian migrants

France has a long tradition of hosting those who were named *refugees*, starting with Dutch refugees who arrived in 1787 to escape from the Orangists' revolution in the United Province of the Netherlands, the Poles in the 1830s, Spanish refugees fleeing Carlist wars in the 1840s, Russian whites and Armenian refugees in the 1920s, Spanish Republicans in the 1930s, or refugees from Vietnam, Cambodge or Laos from the mid 1970s (Burgess, 2008; Djegham, 2011). Since the refugee crisis of 2015, the scale of the refugee migration has raised public awareness of refugees. In 2015, we saw a significant increase in the number of sea and land arrivals from 6,913 monthly detected sea arrivals in the Mediterranean sea in January 2015 to 222,800 sea arrivals detected in October 2015. More than 1 million first-time asylum applicants were registered in 2015 and 2016 in Europe. Within the European Union the situation was more heterogeneous. Although the number of asylum seekers per year in Germany rose by 159 percent between 2014 and 2016 to about 587 thousand in 2016, France experienced a much milder increase of 12 per cent over the same period to reach 63 thousand new asylum seekers in 2016. In fact, as can be seen in Figure 1, France has experienced various waves of asylum seekers inflows over the last two decades.

Figure 1: Asylum seekers inflows in France – 1995 - 2017



Source: OFPRA activity reports. Note: Main asylum seekers' countries of origin are displayed for the specific periods.

This paper focuses on these several waves of humanitarian migration involving asylum seekers, subsidiary protection beneficiaries, and refugees. Asylum seekers are individuals who apply for the refugee status. In France, and since 1991, they do not work during their asylum procedure[2] and receive the "Allocation pour demandeurs d'asile" , a monthly subsidy that is equivalent to 6.80 euros per day for a household of one person. The average time taken to process an asylum application since 2000 is 5 months, with peaks depending on period, as in 2015, when processing time rose to 7 months. Asylum seekers can be assigned to a housing center to wait for their asylum application to be processed. They

[2]Asylum applicants are allowed to ask for a temporary work permit if the processing of their application takes more than nine months. In practice, even if their application has been processed for more than 9 months, they do not engage in this demanding procedure to obtain the right to work.

have no choice as to the location of the housing center to which they are assigned, and the monthly subsidy is conditional on them accepting the housing solution offered. The "Office Français de Protection des Réfugiés et des Apatrides" (OFPRA, French Office for the Protection of Refugees and Stateless Persons) evaluates their asylum demand and either rejects their request, grants them the refugee status, or grants them the subsidiary protection status. Those who obtain the refugee status have a ten-year renewable residence permit and the same rights as a French citizen, aside from voting rights. Asylum seekers who do not meet the refugee status criteria but are at risk of serious harm in the origin country[3] obtain the subsidiary protection, which is a one-year renewable residency permit with the right to work. Those whose requests are denied either leave French territory or become undocumented.

II.2. Hosting scheme for humanitarian migrants in France

European countries have introduced dispersal policies for refugees to spread hosting costs across the territory[4]. The welcome scheme in France consists of different types of centers that host refugees or asylum seekers during the processing of their applications. The first center was set up in the 1970s to house South American refugees fleeing the dictatorships of the 1960s and 1970s, as well as boat people from Vietnam. Several waves of asylum seekers have put the hosting scheme under stress, and new centers have often been set up to cover these new waves. There are several types of centers for refugees and asylum seekers. The "Centres Provisoires d'Hébergements" (CPH) are the oldest centers created in the 1970s and host refugees and beneficiaries of subsidiary protection. The "Centres d'Accueil pour Demandeurs d'Asile" (CADA) were established at the beginning of the 1990s and are usually only for asylum seekers, but refugees can remain there for up to 3 months and sometimes more in practice if other hosting schemes are overflowing. Then there are the "Hébergements d'Urgence des Demandeurs d'Asile" (HUDA), the "Acceuil Temporaire - Service de l'Asile" (AT-SA) and "Programme d'Accueil et d'Hébergement des Demandeurs d'Asile" (PRAHDA) for hosting asylum seekers. In order to deal with the refugee crisis, another type of center, "Centres d'Accueil et d'Orientation" (CAO), was set up in 2015 to help absorb migrants and asylum seekers from the Calais camp[5], but most of them were temporary and closed within the year of their opening. The average capacity of a housing center is 66 humanitarian migrants. The largest increase in the

[3] Among the possible serious harm covered are: death penalty or execution, torture or inhuman or degrading treatment or punishment, serious and individual threat to the life or person by reason of indiscriminate violence resulting from a situation of internal or international armed conflict.

[4] Like Denmark from 1986 to 1998, Sweden from 1984 to 1994, the Netherlands from 1987, Norway from 1994, Ireland from 2000 or the United Kingdom from 2000 (Robinson et al., 2003; Dustmann et al., 2017).

[5] The Calais Camp (also known as Calais Jungle) are migrant and refugee camps near the town of Calais. They have been in existence since 2002,and they have been dismantled several times by law enforcement. It hosted up to 9,000 migrants in August 2016.

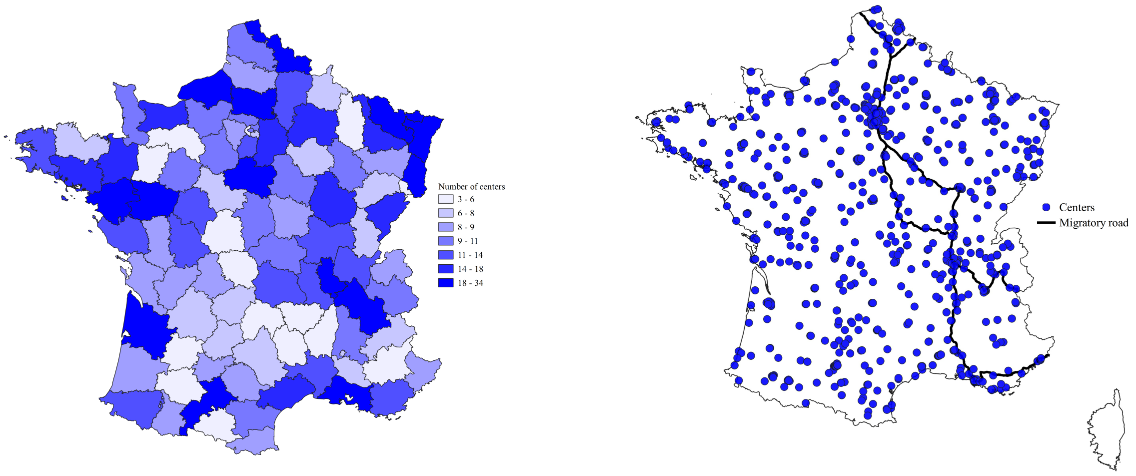
number of centers created was in 2015-2016 as can be seen on Figure 9a. As can be seen on Figure 10, most of the increase in that year was due to the opening of CAO centers. In 2018, there were about 1,000 housing centers spread across the territory. Figure 2 shows that, as of April 2018, housing centers for humanitarian migrants are relatively homogeneously spread across national territory.

The French government is implementing a centralized hosting scheme in order to “spread the burden” – to limit the feared adverse economic or social impact – of hosting humanitarian migrants. The French Interior Ministry launches project calls to open housing centers. Social housing landlords[6] and NGOs then apply to open and run a center. From discussions with French asylum actors (NGOs and social housing landlords), we know that the choice of a location to open a housing center is mainly driven by the availability of a building already owned or rentable for about 15 years. They also provided us with anecdotal evidence that if the project is not selected, they don’t change the location and that they "recycle" the project for next open center calls. This anecdotal evidence is also central to one of the specification strategies where I look at only municipalities in which a center opens. Importantly, municipalities cannot influence the allocation process: they cannot choose the allocation timing nor the number or the characteristics of refugees allocated to them.

Figure 2: Housing centers for refugees and asylum seekers in France

(a) Centers over French departments

(b) Centers localization in France



Source: Ministry of the Interior with data extraction in April 2018 (centers) and IOM - monitoring flows (migratory routes). Note: Figure 2a is a map of French departments, the darker the shade, the more centers in the department. Figure 2b shows the location of all housing center for refugee in France in April 2018 and the migratory routes.

The selection of winning projects is informed by a selection grid covering a number

[6] National housing landlords are called “bailleurs sociaux” in French and operate over the entire French territory. A social landlord is an organization that rents social housing to households in return for a moderate, means-tested rent, paid either by the occupants or paid for by the state. It may also be responsible for the construction of such housing.

of criteria, a shown in the example of a project call for the opening of CADA centers in 2013 on Figure 16 in Appendix A.1. The three main components are the architectural aspects (22 percent of the coefficients), the quality of the project and the future operator (47 percent of the coefficients) and the funding modalities of the center (31 percent of the coefficients). Because the choice of the project was top-down and that there were no criteria for whether the localities should accept the opening of the center, the municipality had no say in the opening process until 2015. Starting in the autumn of 2015, criteria for the "position of local representatives" were added to the project architecture section of the selection grid, as can be seen from the example of the project call for the opening of the CPH centers in 2019 in Figure 17. Although this was added, it represented a maximum of 12.5 percent of the coefficients and local representatives did not necessarily mean the mayor but could be for instance the agreement of the Departmental Representatives (prefects) who were in charge of facilitating the openings. We also know a number of examples of local authorities' opposition to the physical opening of centers post-2015, which means that municipal authorities were not necessarily consulted or their views were not taken into account in the centralized process. The selection process for the CAO centers opened in 2015 differed, as the Interior Ministry needed to quickly open them to dismantle the Calais and Dunkerque camps. The Interior Ministry and prefects played an active sourcing role in finding and persuading local authorities, generally mayors, to open CAO centers. Given the opening of CAO centers in 2015 and the slight change in selection criteria for other centers in 2015, we show that our results are still of a similar magnitude if we exclude CAO centers or focus on the period 1995-2012.

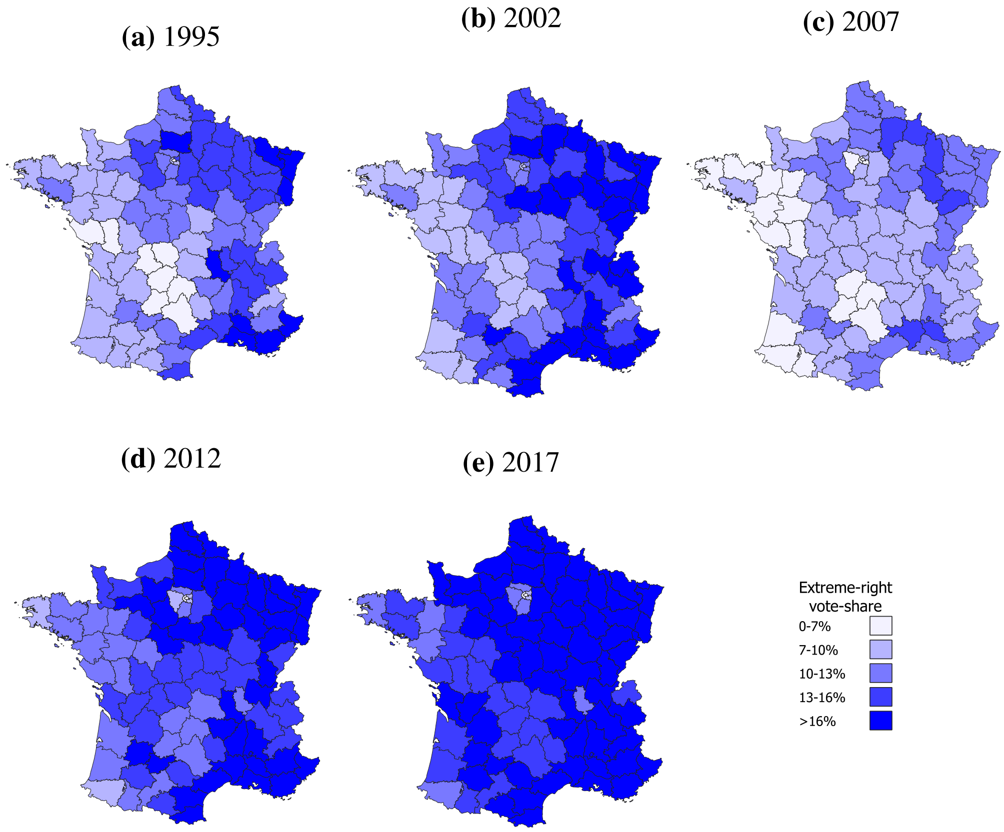
II.3. The extreme-right in France

The rise in the number of humanitarian immigrants following the 2015 refugee crisis was not the only dynamic of social change in Europe. Simultaneously, extreme-right parties grew rapidly, with examples like France's Rassemblement National, Germany's AFD, Italy's Lega Nord, and Austria's FPÖ. In France, the far-right share rose from 15% in 1995 to 21.3% in 2017 in the first round of presidential elections. In Germany, the AFD rose from 1.9% of the votes cast in the 2013 federal election to 12.6% of the votes cast in the 2017 federal election. In Europe, the rise of the extreme right was often linked to the current refugee crisis, given that if the population's discontent with immigration were expressed in the ballot box, the extreme right-wing party would be the one harvesting the votes.

In France, the main extreme-right party is the *Rassemblement National*. Formerly known as the *Front National*, it was founded on 5 October 1972, following the neo-fascist group *Ordre Nouveau* ideology. It was led for most of its history by Jean-Marie Le Pen, and since 2011 by Marine Le Pen. From the outset, the *Front National* developed an anti-immigration discourse as a party unifying theme. As early as 1988, Jean-Marie Le

Pen was spreading his anti-immigration discourse "The people who give in to foreign invasions have not long survived".[7]' From 1995 to 2017, the prevention of immigration to France has always been the main focus of the extreme-right *Rassemblement National* candidates. Their immigration policy platform during presidential elections ranged from expelling all foreigners and preventing any immigration to France from 1995 to 2007, to cutting immigration to 10,000 immigrants per year from 2012 to 2017.

Figure 3: Extreme-right vote share at presidential elections first round in France



Source: Ministry of the Interior. Note: Figure 3 presents maps of the extreme-right vote share at the first round of presidential elections in 1995, 2002, 2007, 2012 and 2017 over French departments. The darker the shade, the higher the extreme-right vote-share.

In France, presidential elections are held using a two-round majority system. Presidential elections were held every 7 years before 2002 and since 2002, every 5 years. In my paper, I look at the evolution of the share of votes received by the extreme-right in the first round of presidential elections from 1995 to 2017. Figure 3 shows the geographic distribution of the far-right voting shares in the first round of presidential elections over the period. The extreme-right appears to be particularly prominent in the eastern part of the country, and we can clearly see an increase in far-right voting, which increased by 40% between 1995 and 2017 in the first round of presidential elections.

[7] January 10, 1988. The National Convention of the *Front National*.

II.4. Data

To examine the impact of exposure to humanitarian migrants on extreme-right voting, I focus on housing centers opening for humanitarian migrants in France. I use a database that I obtained from the French Interior Ministry in April 2018 that provides information on housing centers in France. It includes all types of centers for asylum seekers (HUDA, CADA, AT-SA[8]) and all centers for refugees and beneficiaries of the subsidiary protection (CPH[9]), and CAO[10] centers. It provides information on the type of center, the name of the operator, the date of opening, the capacity of the center, and its address.

Presidential elections results at the municipal level for 1995, 2002, 2007, 2012 and 2017 are publicly available through the French Ministry of the Interior. I compute the share of votes for the extreme right as the number of votes cast for the extreme right over the number of registered voters. The *Front National* (FN) was the only party classified as extreme-right in the 1995, 2007, 2012 and 2017 presidential elections. Bruno Mégret's *Mouvement National Républicain* is classified as an extreme-right party alongside the FN for 2002.

I also rely on data from INSEE at municipal level on population age, proportion of workers in the primary, secondary and tertiary sectors, proportion of the population with baccalaureate or tertiary education, proportion of unemployed, proportion of vacant housing, proportion of immigrants, density or population size. I also use DGFIP IRCOM data for the average resident income in each municipality.

Table 1: Descriptive statistics by hosting and non-hosting municipalities

	1995			2017		
	Non-hosting	Hosting	Difference	Non-hosting	Hosting	Difference
Far-right vote-share	11.61	11.54	0.08	21.45	15.97	5.48***
Men share	0.50	0.49	0.02***	0.50	0.48	0.02***
Young share (0-19 years-old)	0.26	0.26	-0.00*	0.24	0.23	0.00
Elderly share (>65 years-old)	0.18	0.17	0.01***	0.20	0.21	-0.01*
Migrants share	0.02	0.06	-0.04***	0.03	0.07	-0.04***
Unemployed share	0.09	0.11	-0.03***	0.09	0.14	-0.05***
Primary sector workers share	0.17	0.04	0.13***	0.09	0.02	0.07***
Secondary sector workers share	0.29	0.28	0.01	0.24	0.21	0.03***
Tertiary educated share	0.10	0.13	-0.03***	0.22	0.24	-0.02***
Vacant housing share	0.07	0.07	0.00	0.08	0.10	-0.01***
Rural municipality	0.50	0.25	0.25***	0.50	0.25	0.25***
Density	126	1274	-1147***	146	1390	-1243***
Population	1,150	21,477	-20,326***	1,345	23,047	-21,702***
Observations	33,520	446	33,966	33,520	446	33,966

Source: INSEE - French censuses (1990,1999,2006-2017). Note: For pre-2006 data, a linear interpolation is performed to convert data annually. The Table compares municipalities which experienced a refugee housing center opening between 1995 and 2017 (Hosting) and those that did not (Non-hosting).

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

[9] CPH:Provisional Accommodation Centre.

[10] CAO:Welcome and Orientation Centers.

Table 1 shows static differences between municipalities that opened a center after 1995 and those that never opened a center. We see that the share of the vote for the extreme right in the hosting and non-hosting municipalities was similar in 1995, and while both types of municipalities reported an increase in the vote for the extreme right, the non-hosting municipalities showed a 5 percentage points higher share of the vote for the extreme right in 2017 compared to hosting municipalities. We also see that centers open in less rural municipalities, that are more populated, with a higher proportion of migrants, a higher proportion of tertiary-educated population, more tertiary-sector specialized, and more unemployment.

III. Empirical Strategy

III.1. Specification

I estimate the following specification over the period 1995-2017:

$$Y_{it} = \alpha + \beta Opening_{it} + \omega_i + \delta_t + \varepsilon_{it} \tag{1}$$

with municipality i in election year t. Y_{it} is my outcome of interest, that is the log share of votes for the extreme right at the first round of presidential elections in my main specifications. $Opening_{it}$ is a variable equals to the number of refugee centers opened in municipality i at time t. δ_t, ω_i are election year and municipality fixed effects respectively. Municipality fixed effects capture any unit-specific time-invariant unobserved factors. Election year fixed effects capture any time-specific unit-invariant unobserved confounders. Standard errors are clustered at the municipality level. Under conventional identification assumptions, the OLS estimated coefficient of β measures the average deviation in the outcome of interest of hosting municipalities relative to non-hosting municipalities.

In Table 10, I estimate the effect by using a standard difference-in-difference design. However, recent developments in the estimation of difference-in-difference in staggered adoption designs (Borusyak and Jaravel, 2017; Goodman-Bacon, 2018; de Chaisemartin and d'Haultfoeuille, 2020) show that the estimated ATE is a weighted sum of different ATEs (comparisons between early treated and untreated, lately treated and untreated, early treated and lately treated before the treatment, lately treated and early treated after the treatment) with weights that may be negative. This can lead to substantial estimation errors.

Therefore, I use de Chaisemartin and d'Haultfoeuille (2020) estimation procedure to estimate the treatment effects in groups switching from no treatment to treatment compared to those remaining untreated:

$$\beta^S = E\left[\frac{1}{N_S}\sum_{(i,t):t\geq 2, D_t\neq D_{t-1}}[Y_{i,t}(1)-Y_{i,t}(0)]\right] \tag{2}$$

with municipality i and election year t. $N_S = \sum_{t\geq 2, D_t\neq D_{t-1}} N_t$ with N_t the number of registered voters at t. D_t denotes the average treatment at t, $Y_{i,t}(1)$ and $Y_{i,t}(0)$ the average potential outcomes with and without treatment respectively. β^S is the average of the treatment effect at the time when the treatment is received across all treated units. In our context, it will estimate the effect of opening refugee centers, in a municipality that did not have any center before, compared to municipalities that did not opened a center. This estimator estimates the treatment effect in the groups that switch to treatment, at the time when they switch, and does not rely on any treatment effect homogeneity condition. Results in Table 2 using the estimator of de Chaisemartin and d'Haultfoeuille (2020) are very similar to results using the standard difference-in-difference estimation, as shown on Table 10.

III.2. Identification

The identification hypothesis is that the same evolution in the vote for far-right parties would have occurred in control and treated municipalities in the absence of the opening of refugee centers.

The main concern is the selection into treatment. For instance, one might think that control municipalities do not open a refugee centers because they are against migrants or that treated municipalities open a refugee center because they are pro-migrants. First, I provide descriptive evidence in Section II.2 that this is unlikely given how centralized is the process of opening a refugee center is. I do note that starting from mid-2015, centers that opened had to, in theory, obtain local authority's consent, especially in the case of CAO centers. To test if this potential endogeneity can drive my results, I remove CAO centers from the estimation and I remove 2017 election year in Table 7. I show that the magnitude and significance of my coefficients remain unchanged when removing CAO centers or focusing on the 1995-2012 period.

Another concern might be whether control and treated municipalities experience different trends because they differ in the level of their socio-demographic characteristics. To ensure that the results are not driven by differences in socio-demographic characteristics, I use a propensity score matching procedure to address differences in socio-demographic characteristics. As shown in the column (3) of Table 2, the significance and magnitude of my estimate remains unchanged. In Table 7, I also controls for municipalities characteristics in column (5) and for department-specific time trends in column (4), and I show that my estimates remain similar. In Table 13 in Appendix A.4, I also predict the extreme-right vote-share at presidential elections in 1995 with all the control variables described in Table 1 and find no significant differences between the predicted values in control and treated municipalities.

I also provide three additional specifications to address any remaining concerns about differential trends in control and treated municipalities. In a first specification, I focus solely on municipalities that are treated at one point in time. This specification is based on anecdotal evidence described in Section II.2 that operators usually use the location of unsuccessful applications to open a center for next project calls. Comparing only treated municipalities, i.e. comparing at *t* hosting municipalities opening centers at time *t* to hosting municipalities opening centers at time *t+1*, mitigates the concerns about the differences between the control and treated municipalities. In columns (2) of Table 2, I show that the significance and magnitude of my results remain the same. In column (3) in Table 8, I also restrict the control group to only adjacent municipalities of localities in which centers opened and still find a significant and negative effect on far-right voting. Finally, in Appendix A.6, I use an instrumental variable strategy and find similar effects.

Finally, I provide additional robustness checks to address identification concerns and support the common-trend assumption. Combining data on European and presidential elections, I test for the presence of pre-trends by plotting the estimate with leads and lags using the de Chaisemartin and d'Haultfoeuille (2020) estimator in Figure 4 and show that the pre-trend assumption is valid for all my specifications. I also perform placebo tests on all my specifications as if treated municipalities opened a center in the previous period. Table 3 shows that all my specifications pass placebo tests. To address potential spillovers, I also test the robustness of the results by excluding all control municipalities that share a common border with a municipality that opens a refugee center. Column (2) in Table 8 shows that the effects remain very similar.

IV. Results

IV.1. Contemporaneous impact of opening a refugee center on far-right vote

This section examines the impact of opening a refugee center on the 1995-2017 vote for the extreme right in France. Table 2 shows the estimated β for extreme-right voting in three specifications. Column (1) presents the estimate when the control group consists of all non-hosting municipalities. Column (2) presents the estimate when the control group consists of municipalities not being treated at that time, but being treated at subsequent periods. Column (3) presents the estimate when the control group consists of municipalities matched by a propensity score as described in Appendix A.5. Results are significant and similar in magnitude in all specifications. On average, after a refugee center opening, the voting share for the extreme right decreases by about 2 percent compared to the control group. As, on average, municipalities that did not experience the opening of the refugee center had an extreme right-wing vote of 16.2 over the period 1995-2017, the voting share for the extreme right increased by 0.328 points less in the hosting municipalities than in the non-hosting municipalities.

Table 2: Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1) Full Sample	(2) Only Treated	(3) Matching
Center opening	-0.037***	-0.021***	-0.018***
	(0.006)	(0.008)	(0.007)
Election year Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Observations	135,048	1,522	3,928

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

The estimator relies on the assumption that municipalities that have opened a refugee center have experienced similar trends as municipalities that did not. To test this assumption, I use the placebo estimate of de Chaisemartin and d'Haultfoeuille (2020) which compares the evolution of the extreme-right voting from $t-2$ to $t-1$ in the municipalities that are treated and not treated between $t-1$ and t. Table 3 displays the results of these placebo tests for all specifications and none of these placebo tests have a significant effect on extreme right voting. This supports the claim that our estimate captures well the impact of the opening of a refugee center. Using the standard difference-in-difference estimation, I present qualitatively similar estimates in Appendix A.2 and provide evidence that some of the specifications are actually exposed to the negative weights issue when using the standard estimate.

Table 3: Placebo tests – Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1) Full Sample	(2) Only Treated	(3) Matching
Center opening	-0.012	0.011	0.010
	(0.010)	(0.013)	(0.009)
Election year Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Observations	100,839	860	2,755

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

IV.2. Dynamic impact impact of opening a refugee center on far-right vote

Table 4 extends the results to the next election period to show the dynamic effect of the opening of a refugee center on extreme-right vote one and two presidential elections after the opening. The negative effect on the far-right vote significantly doubles two elections after the opening as the vote for the extreme-right dropped between 3.7 to 10 percent. Since presidential elections were held every five years from 2002, two elections after the opening correspond to about 5 to 10 years. This suggests a significant long-term impact of the opening of refugee centers on reducing extreme-right voting.

Table 4: Treatment dynamics – Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1) Full Sample	(2) Only Treated	(3) Matching
Center opening at election t + *1*	-0.037***	-0.021***	-0.018***
	(0.006)	(0.008)	(0.007)
Center opening at election t + *2*	-0.100***	-0.042**	-0.037**
	(0.017)	(0.20)	(0.018)
Election year Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Observations t + *1*	135,048	1,522	3,928
Observations t + *2*	100,561	860	2,755

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

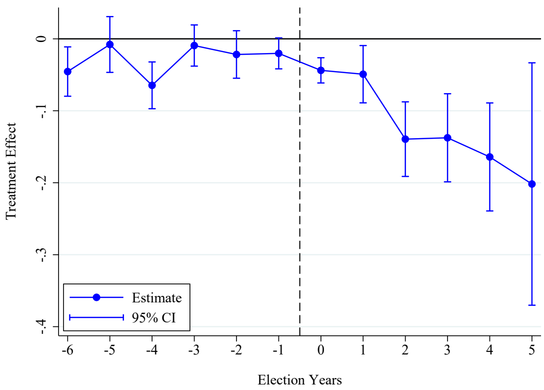
On Figure 4, I extend the number of periods by complementing presidential elections with European elections to take a better look at the dynamics of the effect. I take advantage of the fact that, as shown on Figure 18 in Appendix A.3, European elections are held regularly between presidential elections and the *National Front* has participated in all those elections.

In this section, I estimate the impact of refugee center opening on *National Front*'s voting share. Several new estimators were proposed to estimate the dynamic treatment effect of staggered adoption designs. For example, Callaway and Sant'Anna (2018) use groups that are never treated as their control group, and Abraham and Sun (2018) use groups that become treated at the last period as a control group. I chose the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) because it uses both never treated units and non-treated units at t+1, forming a larger control group that could lead to a more precise estimator. To estimate the dynamic treatment effect, I replace in equation 2 all $Y_{i,t}(0)$ by the counterfactual outcome of the locality i at period t, that is $Y_{i,t}(0, D_{i,t-1}, \dots, D_{i,0})$, and I set the past treatment status equal to its actual values.

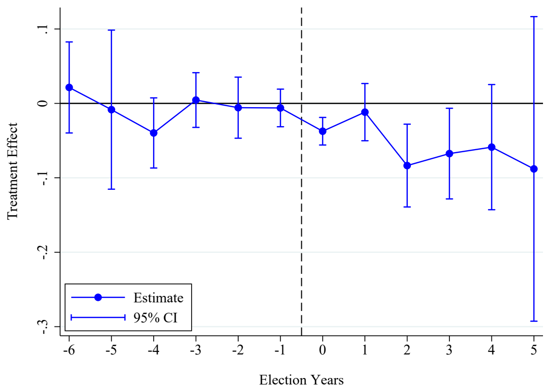
Figure 4 plots the effect coefficients comparing the outcome evolution from $t1$ to $t+1$, between groups treated at period t, and groups still untreated at period $t+1$ with $t \in \{-6;5\}$. 95 percent confidence intervals are presented with the vertical lines. There are no trends prior the opening of a refugee center in all specifications. After the opening of a refugee center, we observe a shift in the trend towards a decline in National Front voting share from the very next elections after the opening. In Table 11 it can be seen that the magnitude of the effect of opening of a refugee center when pooling European and presidential elections together indicates a decrease in the *National Front* vote of about 4 percent in all specifications.

Figure 4: Treatment dynamics – Effect of a refugee center openings on voting for the *National Front* voting at presidential and European elections

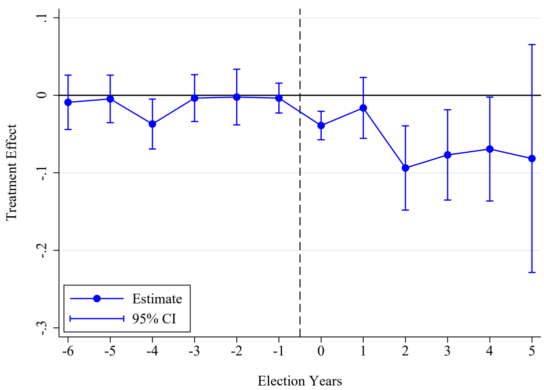
(a) Full sample



(b) Only treated



(c) Matching



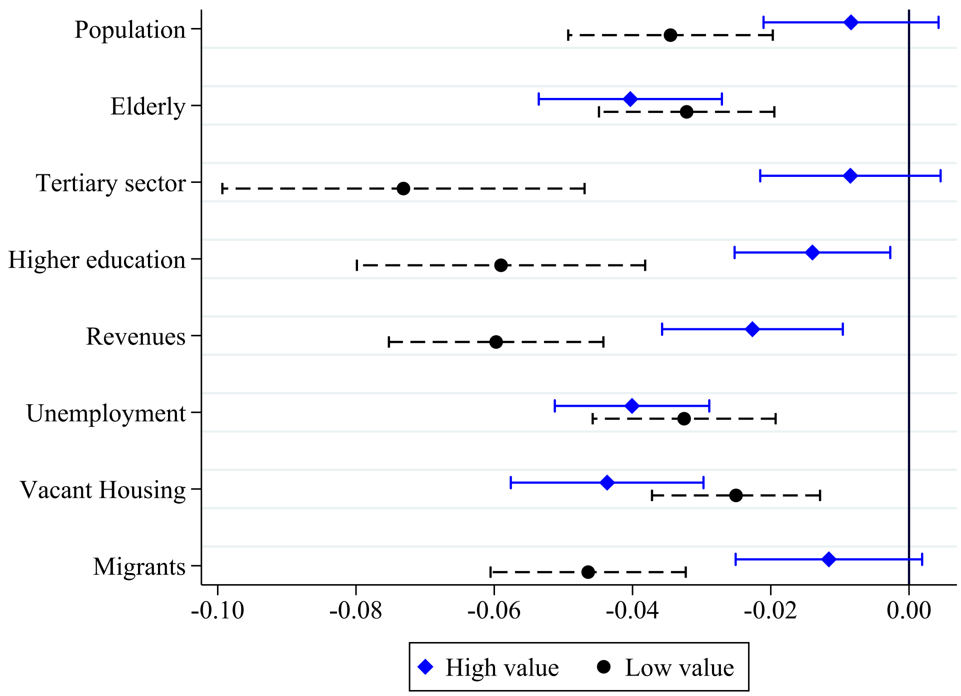
Source: Ministry of the Interior. Note: Estimated β_S from equation (2) where the x-axis is the number of elections relative to the opening of the refugee housing center and where the outcome is the log vote-share for the national front at presidential and european first round of elections. The incertitude of each point is asserted with a 95% confidence interval.

IV.3. *Heterogeneity of the effect*

IV.3.1. *Municipalities' characteristics*

This section examines the effect's heterogeneity to see if certain characteristics of the municipality play a role in the magnitude of the results. I divide the samples at the median value of the observable characteristics of treated municipalities in 1995. For example, the median population in 1995 in municipalities that will open a center between 1995 and 2017 is 782. I define "High" a sample of municipalities with a population in 1995 that is greater than or equal to 782 and "Low" a sample of municipalities with a population in 1995 that is less than 782. Figure 5 and Table 14 in Appendix A.4 present an analysis of the heterogeneity of the effect based on population size, proportion of elderly, proportion of people employed in the tertiary sector, proportion of people with tertiary education, income, proportion of unemployed, share of vacant housing and proportion of migrants in the municipality.

Figure 5: Treatment heterogeneity by municipal characteristics – Effect of refugee center openings on far-right voting at presidential elections



Source: Ministry of the Interior, INSEE - French censuses, and IRCOM data. Note: The incertitude of each point is asserted with a 90% confidence interval. Estimated β_S from equation (2) in the full sample specification. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

First, we see that the reduction in the vote for the extreme right after the opening of the refugee center is higher in small municipalities, which could be because the vote for the extreme right generally increases more in small municipalities or because the contact or compositional channels are more active in small towns. Table 12 in Appendix A.4 displays the extreme-right vote-share in 1995 and 2017 with samples divided by the same cutoff is in Table 14. If the extreme-right vote-share was higher in small towns in 1995, it was much higher in the highly populated municipalities in 2017, as the extreme-right vote increased more in large cities. This suggests that the most indicated channels at work are contact or composition changes.

Secondly, with regard to the characteristics of the population living in the municipality, it can be seen in Table 14 that the vote for the extreme right decreased more in municipalities where the population worked mainly in the secondary or primary sector, was less skilled, and with lower incomes. This points to the intuition that the effect of opening a refugee center is mainly due to a contact that has changed the minds of those who are traditionally more opposed to immigration (low education, lower incomes, not tertiary sector). These findings differ from those found by Dustmann et al. (2019) in Denmark where hosting refugee has a positive effect on far-right rural voting but a negative effect on urban areas.

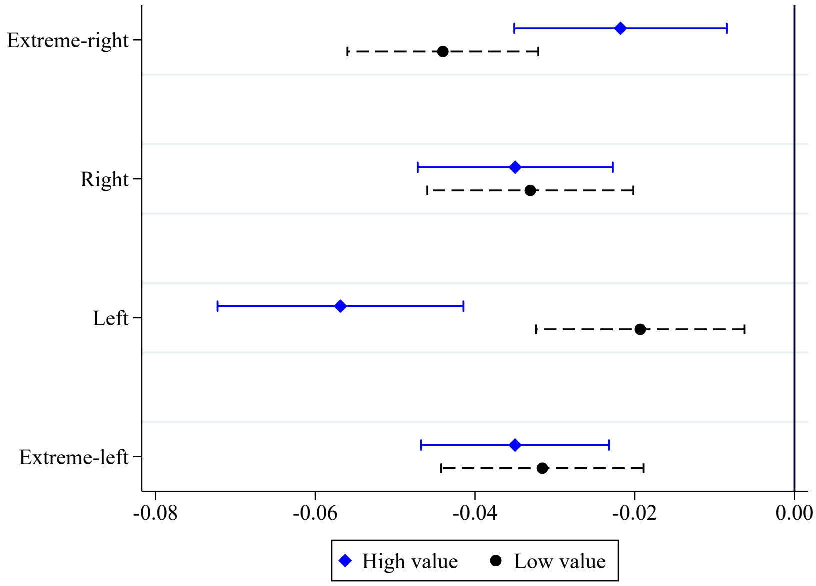
Finally, this is reinforced by our last finding that the effect of the decrease in the vote for the extreme-right is higher in municipalities that initially had a low proportion of immigrants. This again points to the contact hypothesis as a refugee center opening in municipalities in which fewer immigrants were living would increase the salience of refugee's presence but would decrease the likelihood of disruptive/frictional contact with an immigrant population according to the threshold hypothesis.

IV.3.2. Vote for other political parties

On Figure 6 and in Table 16 in Appendix A.4, I investigate whether the drop in extreme-right voting after the opening of a refugee center was more pronounced in more left or right-wing municipalities. To do so, I split the sample of treated municipalities at the median vote share for the extreme-right, right-wing, left-wing, and the extreme-left vote share in 1995. It shows that the reduction in vote the for the extreme-right was significantly higher in municipalities with a high share of left-wing votes at the beginning of the period.

Table 5 reproduces the main analysis presented in Table 2 for other political parties. The left-wing parties are the ones who benefit from the relative decrease in the far-right vote. The descriptive statistics in Table 1 showed that the vote for the extreme-right still rose in treated municipalities but less than in control municipalities. This suggests a buffering effect of opening a refugee center such that it prevents people from starting to vote for the extreme-right. As the effect predominates in municipalities that were more left-wing at the beginning of the period, it is sensible that the left should be the political parties that gain from the opening of a refugee center because natives do not switch their votes to the extreme right.

Figure 6: Treatment heterogeneity by political parties vote-share in 1995 – Effect of refugee center openings on far-right voting at presidential elections



Source: Ministry of the Interior. Note: The incertitude of each point is asserted with a 90% confidence interval. Estimated β_S from equation (2) in the full sample specification. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

Table 5: Effect of refugee center openings on turnout and other political parties voting at presidential elections

Outcome: turnout and vote-share	**Turnout** (1)	**Right** (2)	**Center** (3)	**Left** (4)	**Extreme-left** (5)
Center opening	-0.012***	0.008	-0.003	0.024***	0.005
(Full sample)	(0.003)	(0.006)	(0.009)	(0.005)	(0.005)
Center opening	-0.004	0.013	-0.018	0.014*	-0.001
(Only Treated)	(0.004)	(0.008)	(0.013)	(0.008)	(0.007)
Center opening	-0.009***	0.006	-0.014	0.015**	-0.000
(Matching)	(0.003)	(0.006)	(0.009)	(0.007)	(0.001)
Election year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Obs. (Full sample)	135,843	135,722	135,315	134,548	134,308
Obs. (Only Treated)	1,522	1,522	1,522	1,522	1,522
Obs. (Matching)	3,961	3,961	3,961	3,960	3,959

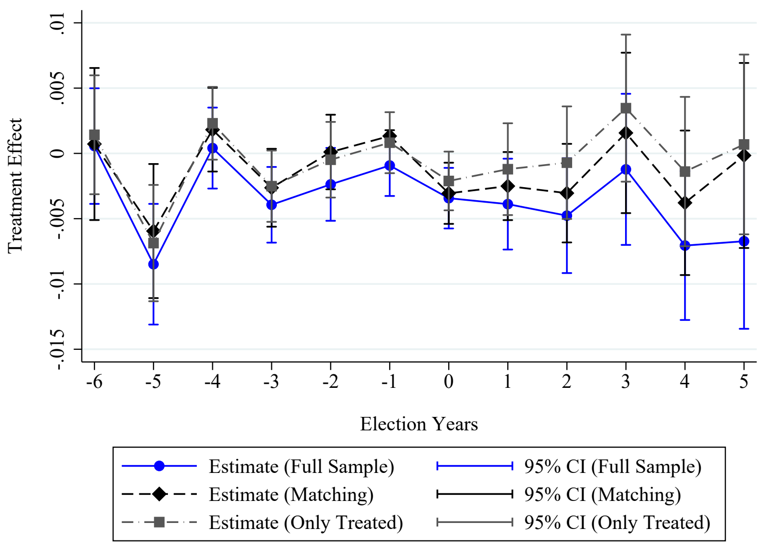
Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log turnout in (1), and the log vote share at presidential election's first round of right-wing parties in (2), center-wing parties in (3), left-wing parties in (4), and extreme-left parties in (5). Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

IV.4. Channels

IV.4.1. Economic changes

Better economic or labor market conditions could explain the decline in the extreme right-wing vote after the opening of the refugee center. A positive economic shock could be plausible as humanitarian migrants are eligible for monetary subsidies[11] which could lead to increased demand and expenditure on local services. Vertier and Viskanic (2019) that examine the effect of opening CAO housing centers in France from 2012 to 2017, do not find any significant difference in net job creation per inhabitant between municipalities that eventually received a CAO center and those that did not. Batut and Schneider-Strawczynski (2019) investigate the same refugee-allocation context as I look at in France in 2002-2014, and find that the opening of a refugee center does not impact the employment or salary of workers in treated municipalities. However, Batut and Schneider-Strawczynski (2019) observe a reduction in the firms' economic activity and a decrease in the municipalities' taxes income due to the native avoidance entailed by the refugee-center opening. If bad economic conditions were to spur the vote for the extreme-right party, it would work against the effect of a decrease in the vote for the extreme-right, making the reported estimate a lower bound of the true effect.

Figure 7: Effect of refugee center openings on population's revenues



Source: IRCOM data. Note: Estimated β_S from equation (2) where the x-axis is the number of years relative to the opening of the refugee housing center and where the outcome is the log average revenues of the municipal population. The incertitude of each point is asserted with a 95% confidence interval.

I still check for the presence of positive effects of opening a refugee center on local population's incomes as variations in this characteristic could affect electoral outcomes.

[11] Asylum seekers are entitled to the "Allocation pour Demandeurs d'Asile" while refugees over 25 years-old can apply for the "Revenu de Solidarité Active".

To do so, I use the annual IRCOM tax data on revenues of the municipal population and show no evidence of a revenue shock from the arrival of refugees in Figure 7. This is also useful for checking the presence of economic pre-trends prior to the opening of a refugee center, and it can be seen that hosting municipalities do not differ in income trend from non-hosting municipalities prior to opening a refugee center.

IV.4.2. Compositional changes

Batut and Schneider-Strawczynski (2019) show that the opening of a refugee center leads to a decline in the municipal population, which stagnates at around 2 per cent four years after the opening. This decline in population is not due to a native flight – to locals leaving hosting municipalities – but rather to native avoidance – to natives avoiding moving to refugee-hosting municipalities. Thus, the municipal population could differentially change between hosting and non-hosting municipalities, with people coming less to treated municipalities and going to control municipalities instead. This could be a factor explaining the decline in far-right voting if, for example, prospective far-right voters were less likely to come to host municipalities. For the time being, the literature has overlooked this potential channel, although it is particularly relevant when looking at municipal data.

Table 6: Effect of opening a refugee center on voters

Outcome:	(1) Registered Voters	(2) ER voters	(3) ER vote share with simulated population transfers
Center opening (Full sample)	-0.010***	-0.048***	-0.030***
	(0.002)	(0.007)	(0.006)
Center opening (Only Treated)	-0.005	-0.026***	-0.016*
	(0.003)	(0.007)	(0.009)
Center opening (Matching)	-0.007***	-0.025***	-0.012*
	(0.003)	(0.006)	(0.007)
Election year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Obs. (Full sample)	135,854	135,048	135,048
Obs. (Only Treated)	1,522	1,522	1,522
Obs. (Matching)	3,961	3,961	3,961

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variables are the log number of registered voters in (1), the log number of voters who cast a ballot for the extreme-right in (2), and the log vote-share of the extreme-right after simulating populations changes in (3). Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

In Batut and Schneider-Strawczynski (2019), the compositional change takes at least one year to occur. It can be seen on Figure 13 that 40 percent of the openings occurred in the year before the previous election. I check in column (1) of Table 6 whether the number of registered voters decreased more in hosting municipalities compared to non-hosting municipalities after the opening of a refugee center. The matching and full sample specifications detect a decrease by 0.7 to 1 percent of the registered population between the elections before and after the opening of a refugee center. To check whether this population change could explain the results, I first look in column (2) whether the measure of the decline in voting for the extreme right as a share of registered voters does not decrease mechanically due to the higher decrease in registered population in hosting municipalities compared to non-hosting municipalities. Estimates in all three specifications still suggest a drop in the number of extreme-right voters from 2.5 to 4.8 percent.

Column (3) of Table 6 displays the result of an exercise in which I try to cancel the native avoidance channel by doing as if treated municipalities would receive 2 percent more of their number of registered voters after the opening of the refugee center coming from control municipalities[12], and that all of them would vote for the extreme-right. This is a conservative simulation as it assumes a 2 percent change based on Batut and Schneider-Strawczynski (2019) population's estimates and not the 0.7 to 1 percent found in registered voters data, and because it assumes that all newcomers would vote for the extreme-right. Accounting for the compositional channel does reduce the effect of the opening of a refugee center on voting for the extreme-right, but a part of the effect remains and is significant. This suggests that a compositional effect may explain a part of the decline in extreme-right voting after the opening of a refugee center, but not all of it. Following the literature, the rest of the effect could be attributed to the contact hypothesis channel.

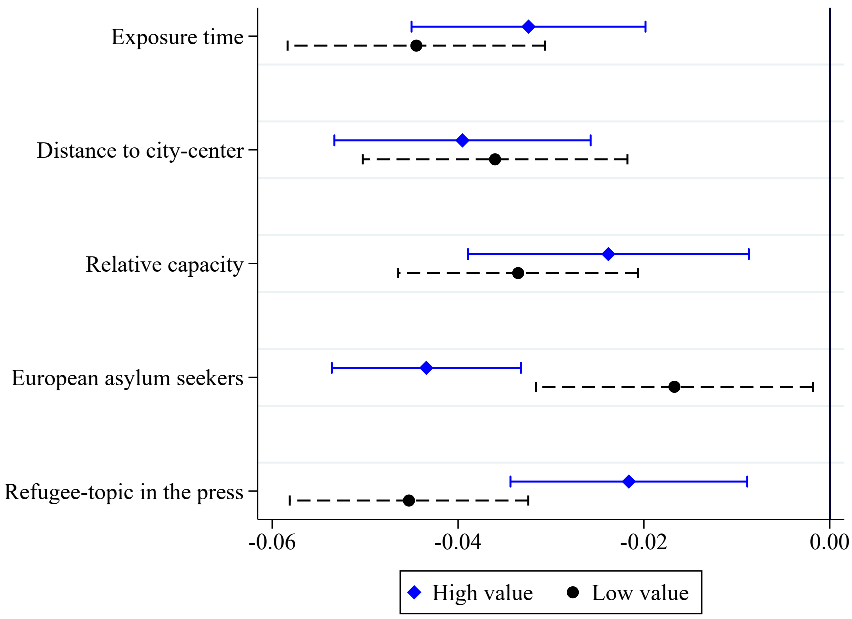
IV.4.3. Contact hypothesis

The Allport (1954) Intergroup Contact Theory postulates that contact between majority and minority groups could reduce prejudice of majority against minority groups. According to Allport (1954), the intergroup contact theory is activated when both groups have i) similar characteristics, ii) work together towards a common goal, and iii) support their environment's authorities, laws or customs. Inherent differences in the characteristics of natives and refugees, however, prevent condition iii) from being met. The analysis is thus based on the premise that, in this context, it is not just any type of contact between two different groups that can lead to a reduction in prejudice (Valentine, 2008). In particular, I qualify the "contact theory" in relationship to the "threshold (or tipping-point) theory" (Schelling, 1971; Card et al., 2008; Aldén et al., 2015) to show that the relationship between contact with refugees and voting for the far right (prejudiced against immigration)

[12] I take this 2 percent of population from control municipalities weighted by their vote-share for the extreme-right.

depends on the perceived contact intensity. In other words, it depends on how much contact is perceived as potentially disruptive, which is consistent with the "realistic group conflict theory" (Campbell, 1965; Bobo, 1983; Quillian, 1995; Dustmann et al., 2019). I provide a number of measures of different dimensions that could characterize disruptive contact, such as the duration of exposure to refugees, the capacity of the center, the proximity of the refugee center to the city-center, the cultural distance with the hosted refugees, and the salience of the refugee arrival in the media. Figure 8 examines whether the effect of the drop in the extreme-right vote following the opening of the refugee center is modified with the characteristics of the center opening. I use the full sample specification and divide the treated municipalities at the election period in which they become treated between municipalities below or above the median value of centers' characteristic[13]. In Figure 20 in Appendix A.4, I split the sample over more percentiles to investigate the heterogeneity's dynamic in more details.

Figure 8: Treatment heterogeneity by centers' characteristics – Effect of refugee center openings on far-right voting at presidential elections



Source: Ministry of the Interior, "annuaire de l'administration", OFPRA, and Europress data. Note: The incertitude of each point is asserted with a 90% confidence interval. Estimated β_S from equation (2) in the full sample specification. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

First, I investigate whether the magnitude of the drop in extreme-right voting varies depending on the duration of exposure to refugees before the next election. The time-distance variable between the refugee center opening and next election ranges from 0

[13]This ensures that treated municipalities are evenly split across election periods and prevents that treated municipalities concentrate on certain periods of time for some characteristics.

to about 80 months, as shown in Figure 13 in Appendix A.1. Figure 8 shows that the duration of exposure to the refugee center does not produce a significant differential effect on the vote for the far-right. Compared to the results described in Section IV.2, this suggests that the subsequent reduction in extreme-right voting is not a long-term effect of exposure to the center, but rather a continuation of the political change driven by opening the refugee center. Figure 8 and Figure 20b in Appendix A.4 highlight the role of the distance between the refugee center and the municipal center in the extreme-right vote decline. This distance is proxyed by the distance between the refugee center and the city-hall, which is often located in the active center of the municipality. There are no significant differences in the reduction of the vote for the far-right when the center is close or far from the city center. Taken together with the results on the duration of exposure, this is suggestive evidence that the contact effect may not result from direct interactions between refugees and natives, but rather from accepting a modified version of the living environment in which the likelihood of contact is not too disruptive.

Figure 8 shows how the effect on decreasing far-right voting varies with the relative capacity of the center, measured as the number of places available in the refugee center relative to the municipality's population. Looking at variations at the median, there do not seem to be a significant difference, but Figure 20c in Appendix A.4 show some suggestive evidence that the effect on the extreme-right vote reduction appears to be higher when the relative capacity of the center is low (below the 20 percentile of the distribution). This is in line with the findings of Vertier and Viskanic (2019) and Gamalerio et al. (2020). Figure 8 and Figure 20d in Appendix A.4 investigate the role of cultural distance between the hosted refugees and the native population on the decrease in decreasing extreme-right voting after the opening of a refugee center. I use monthly data on the country origin of asylum seekers taken from the OFPRA[14] to proxy for the origin of humanitarian migrants in the refugee center on its opening date. I find that the greater the proportion of European asylum seekers in the center, ie. the lower the cultural distance with the hosted refugees, the higher the effect on the extreme-right vote reduction. Finally, Figure 8 and Figure 20e analyze whether the decline in far-right voting is heterogeneous when the center opened at a time when the media focused more on refugee issues. I matched Europress data on the monthly share of refugee articles in the national generalist press with the opening date of refugee centers. Figure 11 in Appendix A.1 displays the occurrence of the refugee topic in the national press from 1995 to 2018. I find that the more press coverage of refugees at the opening time of the refugee-center, the lower the decline in the extreme-right vote. These three findings suggest that the possibility of a too-disruptive contact between natives and refugees – enhanced by a higher number of hosted refugees, a greater cultural distance with the hosted refugees, and a higher salience of the refugee topic in the press – can mitigate the positive effect of opening a refugee center on the reduction of

[14]The OFPRA is the agency responsible for the processing asylum claims in France.

far-right voting.

IV.5. Robustness

In this section, I test the robustness of the results by varying the specification, the sample of control municipalities, and the sample of treated municipalities to address the remaining identification concerns.

Table 7: Robustness tests – Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1)	(2)	(3)	(4)	(5)
Center opening	-0.059***	-0.039***	-0.036***	-0.039***	-0.034***
(Full sample)	(0.010)	(0.007)	(0.005)	(0.006)	(0.006)
Center opening	-0.026**	-0.017*	-0.010	-0.022*	-0.016*
(Only Treated)	(0.011)	(0.009)	(0.008)	(0.012)	(0.009)
Center opening	-0.026**	-0.018**	-0.030***	-0.027***	-0.015**
(Matching)	(0.011)	(0.008)	(0.007)	(0.007)	(0.007)
Election year FE	Yes	No	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Department-time FE	No	No	No	Yes	No
Obs. (Full sample)	101,181	134,380	134,490	135,048	134,659
Obs. (Only Treated)	1,336	1,033	1,065	1,522	1,522
Obs. (Matching)	2,974	3,309	3,419	3,961	3,961

Source: Ministry of the Interior, INSEE - French censuses. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects. In (1), I remove the election-year 2017 and perform the analysis over the 1995-2012 period. In (2), I remove all CAO centers from the estimation. In (3) I run the regression on a subsample of large NGOs operating housing centers. In (4), I include department-time fixed-effects. In (5) I control for the share of workers in the primary sector, in the secondary sector, the share of elderly, the share of migrants, the share of the population with a higher education, and the share of vacant housing.

Given the increased media attention received by refugees following the 2015 refugee crisis, one may be worried that this increased interest may have change the practice of opening refugee centers in a way that was more careful not to challenge local authorities' preferences about hosting refugees. Since this could be a source of endogeneity, I examine whether the results hold in the period 1995-2012, i.e. before the refugee crisis when there was much less public concern or awareness of refugees. In column (1) of Table 7, I remove the election year 2017 and show that the results hold for the period 1995-2012 and even seem a bit higher than for the period 1995-2017. Given the heterogeneity results on press exposure in Figure 8, this may suggest that increased media exposure to refugees following the refugee crisis as can be seen on Figure 11 in Appendix A.1 actually prevented part of the beneficial impact of opening refugee centers on the reduction of extreme-right voting.

As noted by Vertier and Viskanic (2019), who focused their analysis on *Centres d'Accueil et d'Orientation* (CAO) centers that were open for the dismantling of the Calais jungle in 2015, another concern is that the opening of this specific CAO type of center did not follow explicit allocation criteria. We cannot be certain that mayors were not involved in the process for the opening of some CAO centers, although it was claimed that the allocation across regions followed socio-demographic criteria. In column (2) of Table 7, I replicate the analysis without the municipalities opening a CAO center and find very similar results to my main estimate.

One might also be concerned that relatively small or local NGOs may be more exposed to contact with local authorities, leading to potential endogeneity in choosing where to open a refugee center, as opposed to large or national NGOs managing multiple buildings across national territory. I exclude all centers that were opened by a lower-scale or local NGOs[15] in column (3) of Table 7 and find quantitatively similar results as of the main estimates presented in Table 2.

In column (4) of Table 7, I add department-time fixed effects to allow for department-specific trends by capturing time-varying shocks in the department. In the full sample analysis, the estimated β coefficient thus measures the average deviation of hosting municipalities relative to their department trends after the opening. I also find very similar effects as the ones described in Table 2.

In column (5) of Table 7, I provide an alternative specification to the matching specification presented in Table 2 where I use the full sample specification controlling for socio-demographic variables at the municipality level, and still find similar results.

In Appendix A.6, I also use an instrumental variable strategy using the availability of group accommodation as an instrument for the opening of refugee centers in the spirit of the paper by Steinmayr (2020). Even though my context of analysis does not require the use of an instrumental variable strategy, I still show that I obtain similar results when applying the instrumental variable strategy usually performed in the literature.

Table 8 presents different specification according to municipalities that share a border (adjacent) with municipalities in which a refugee center opens. In column (1), I consider adjacent municipalities as treated and exclude municipalities in which a refugee center opens from the treatment group. The effect remains significantly negative but decreases in magnitude, which indicates that there might be some spillovers of the effect on adjacent municipalities. I address the threat of potential spillovers by excluding all control

[15] I restricted the sample of treated municipalities to those who open a center run by *ADOMA*, *l'Armée du Salut*, *France Horizon*, *Forum Réfugiés*, *le Diacconat Protestant*, *Emmaus*, *l'Escale*, *l'Entraide Pierre Valdo*, *Audacia*, *Accueil et promotion*, *Afla3A*, *ADDSEA*, *COS*, *SOS solidarité*, *la Croix Rouge Française*, *COALLIA*, *France Terre d'Asile*.

municipalities sharing a common border with hosting municipalities. If close control municipalities were also exposed to treatment, the effect of opening a refugee center could be underestimated. In column (2), the treatment group is composed of municipalities in which a refugee center opens but I exclude the adjacent municipalities from the control group. We can see that the effect remains similar in magnitude as the ones presented in Table 2, which indicates that it is unlikely that spillovers substantially bias the results and that the estimated effects only capture the impact on treated municipalities. In column (3), the treatment group is composed of municipalities in which a refugee center opens but I restrict the control group to only adjacent municipalities. The effect remains significantly negative though the magnitude of the effect decreases, as it was expected given the possibility of spillovers.

Table 8: Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1) Treatment group: Only adjacent mun.	(2) Control group: Without adjacent mun.	(3) Control group: Only adjacent mun.
Center opening (Full sample)	-0.027***	-0.046***	-0.013**
	(0.002)	(0.006)	(0.007)
Center opening (Only Treated)	-0.011***	-0.021***	
	(0.002)	(0.008)	
Center opening (Matching)	-0.012***	-0.024***	
	(0.004)	(0.007)	
Election year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Obs. (Full sample)	129,643	122,314	14,511
Obs. (Only Treated)	1,612	1,522	
Obs. (Matching)	1,971	3,349	

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects. In (1) the treatment group is composed of municipalities that are adjacent to one where a refugee center opens. In (2), adjacent municipalities are excluded from the control group. In (3), the control group is composed of only adjacent municipalities.

In a final specification, I focus on polling stations within the same municipality where I compare polling stations in areas in which a refugee center has opened to other polling stations in the same municipality in which a refugee center has not opened. Looking at variations within the same municipality, the existence of a selection bias at the municipal level municipal influencing the effect can be discarded. I use the results of the 2007, 2012 and 2017 polling station-level presidential elections purchased from a private company specializing in consulting and numerical tools for politics. The polling station boundary data was not available for the election years 1995 and 2002. For municipalities that did not change their polling stations, I inferred the 2002 polling station area from the 2007 data. Additionally, polling station boundaries were not available for some municipalities which led to significant data loss, with around 10% missing in 2017, 11% missing in 2012, 21% missing in 2007, and around 30% missing in 2002. Some centers may also have multiple housing units spread throughout the municipality, though the data only record the address of the main buildings, which would increase the risk of spillovers in the polling stations analysis. Decreased sample size, reduced number of available periods, and higher risk of spillovers are likely to reduce the ability to detect an effect.

Table 9: Polling-station level – Effect of refugee center openings on far-right voting at presidential elections

Outcome: vote-share of the extreme-right	(1) Effect at t	(2) Effect at $t+1$	(3) Placebo
Center opening	-0.020*	-0.039	-0.015
	(0.011)	(0.036)	(0.017)
Polling-station FE	Yes	Yes	Yes
Municipality-time FE	Yes	Yes	Yes
Observations	4,289	2,228	2,392

Source: Ministry of the Interior, INSEE - French censuses, and IRCOM (revenues data). Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

In Table 9, I provide an estimate of when the treated units are polling stations in which a refugee center opened and the control units are polling stations in the same municipality where a refugee center has not opened. In column (1), the effect of the opening of the refugee center on the far-right vote can be seen, with an estimate quantitatively similar to that obtained in Table 2. This is reassuring because it means that the results hold when considering variations within municipalities. There is no significant effect two elections after the opening of a refugee center with that specification. The failure to detect long-term effects could be explained by the flaws described above. Finally, I run the placebo test as I did in Table 3 which confirms the hypothesis that polling station areas opening a refugee center experienced similar trends as polling stations in the same municipality that did not open a refugee center.

V. Conclusion

This paper seeks to understand the political economy impact of hosting refugees. I show that French municipalities hosting refugees experience a decline in the vote for the extreme-right. I exploit the openings of housing centers for refugees and asylum seekers in more than four hundreds French municipalities between 1995 and 2017. I compare these municipalities with those that did not experience the opening of a refugee center. After a center opens, the vote for the extreme-right decreases by about two percent. I demonstrate that this decline is not due to an economic demand shock and I am the first to explain this effect through both a composition and a contact channel. I show that even in the absence of a native flight, part of the results could be explained by a native avoidance phenomenon. I then consider the contact theory in relation to the threshold and realistic group conflict theories, to provide suggestive evidence that too-disruptive contacts, as measured by the magnitude of the inflows, the cultural distance and the media salience of refugees, can mitigate the beneficial effects of contact on the reduction of far-right support.

This paper provides new evidence on the impact of hosting refugees on voting for far-right parties and on the factors explaining the change in preference of natives towards this immigrant population. These findings could help policymakers to adapt accommodation and dispersal schemes in order to enhance a better acceptance of the refugee population. In particular, it is important to pay attention to the type of contact that will result from the opening of a refugee center, not only in terms of the number of refugees hosted, but also in terms of the countries of origin composition of the refugees hosted. It is also important to focus on how the media-salience of the arrival of refugees is displayed to the public, as even an increase in the salience can mitigate the beneficial impact on the reduction of far-right voting. Finally, it may seem important to keep opening centers in municipalities that may not initially be in favor of hosting refugees as a not too much disruptive contact with refugees could help reduce natives' prejudice against them.

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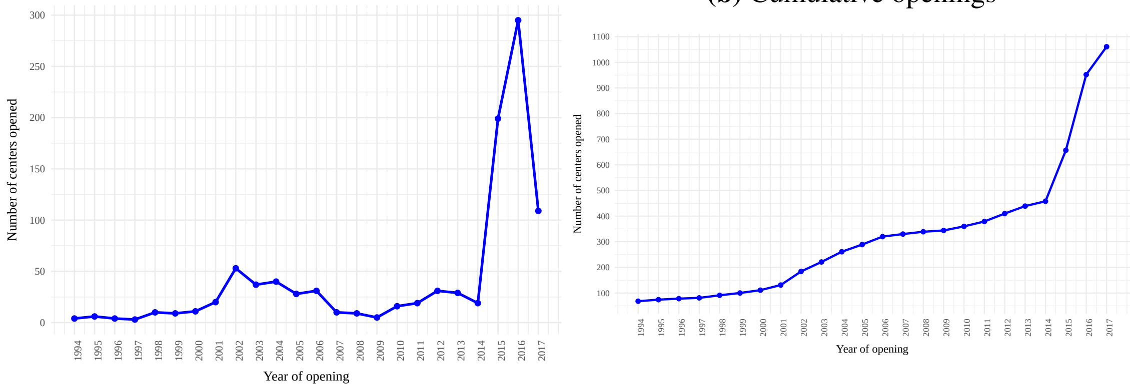
A. Appendix

A.1. Context

Figure 9: Housing centers for humanitarian migrants openings in France

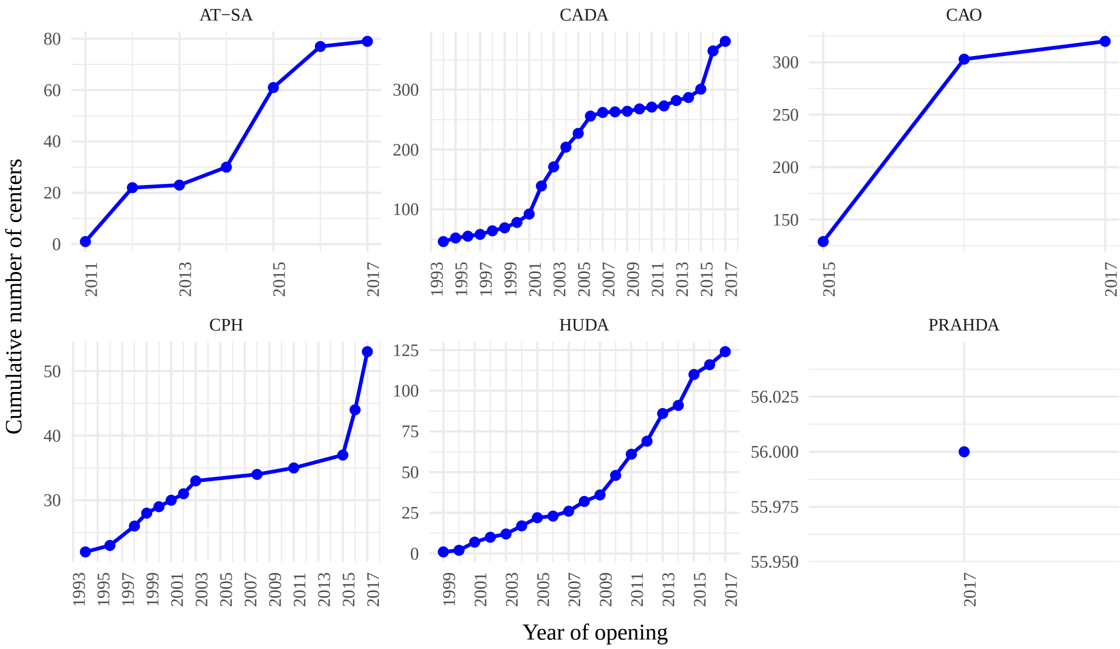
(a) Yearly openings

(b) Cumulative openings



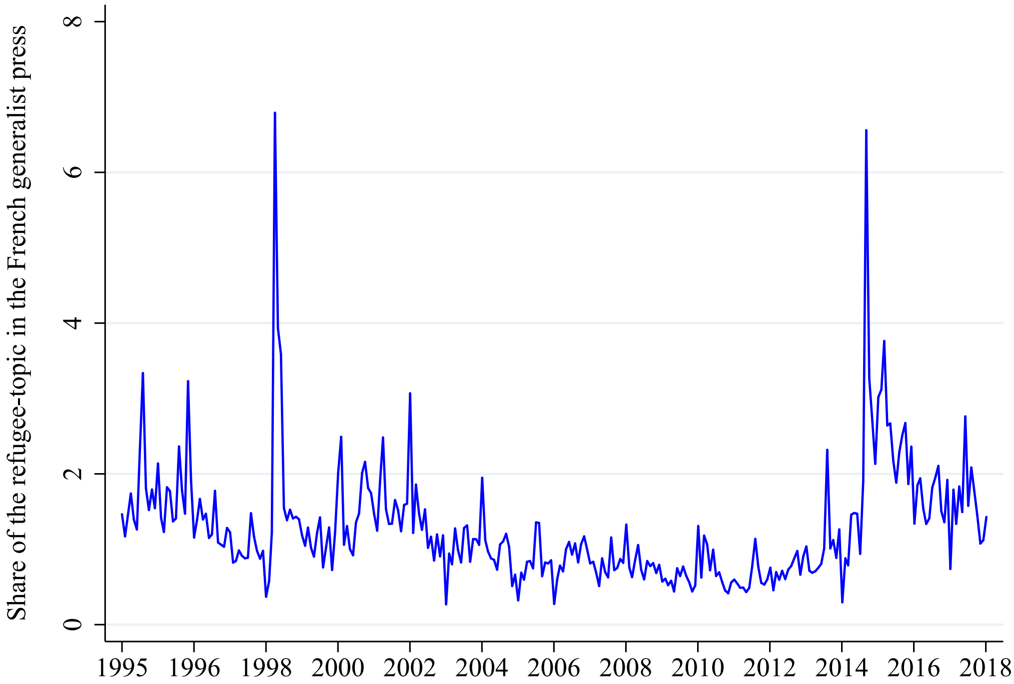
Source: Ministry of the Interior.

Figure 10: Cumulative number of centers by type of centers



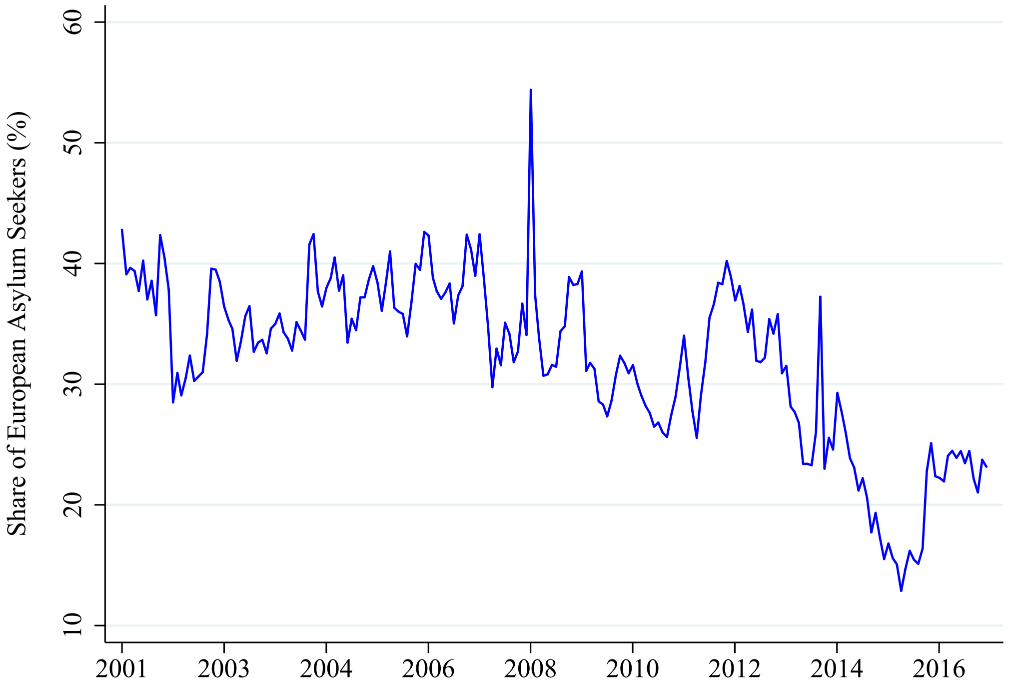
Source: Ministry of the Interior.

Figure 11: Share of refugee-topics in the French national generalist press



Source: Europress.

Figure 12: Share of European Asylum Seekers in France



Source: OFPRA.

Figure 13: Distribution of the month-distance between the opening of a refugee center and the next election

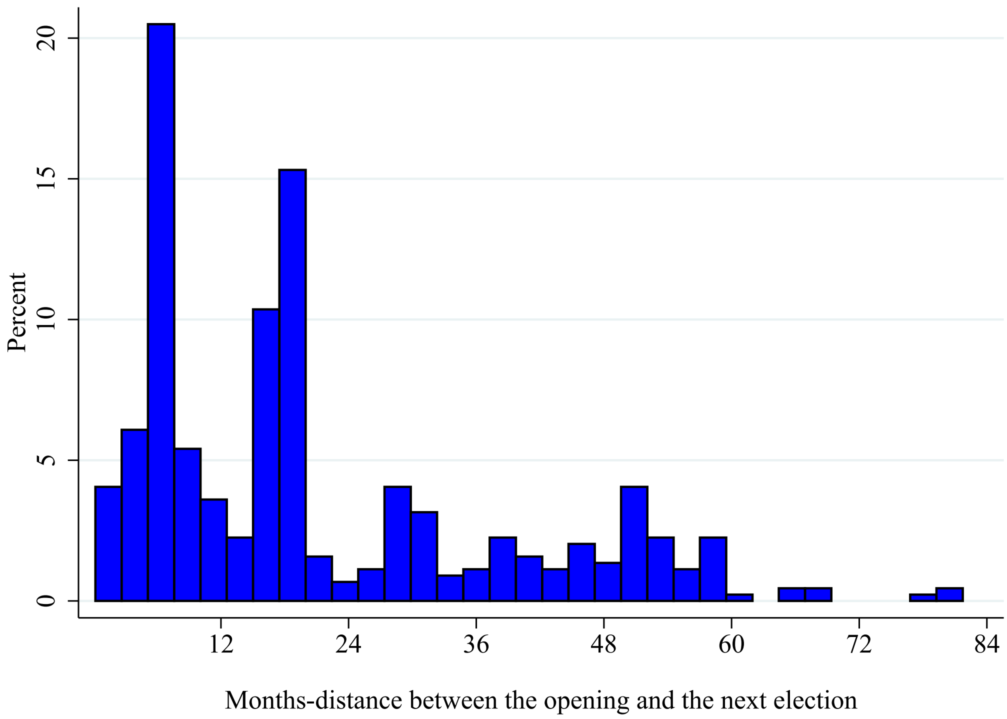


Figure 14: Distribution of the capacity (number of places) of refugee centers

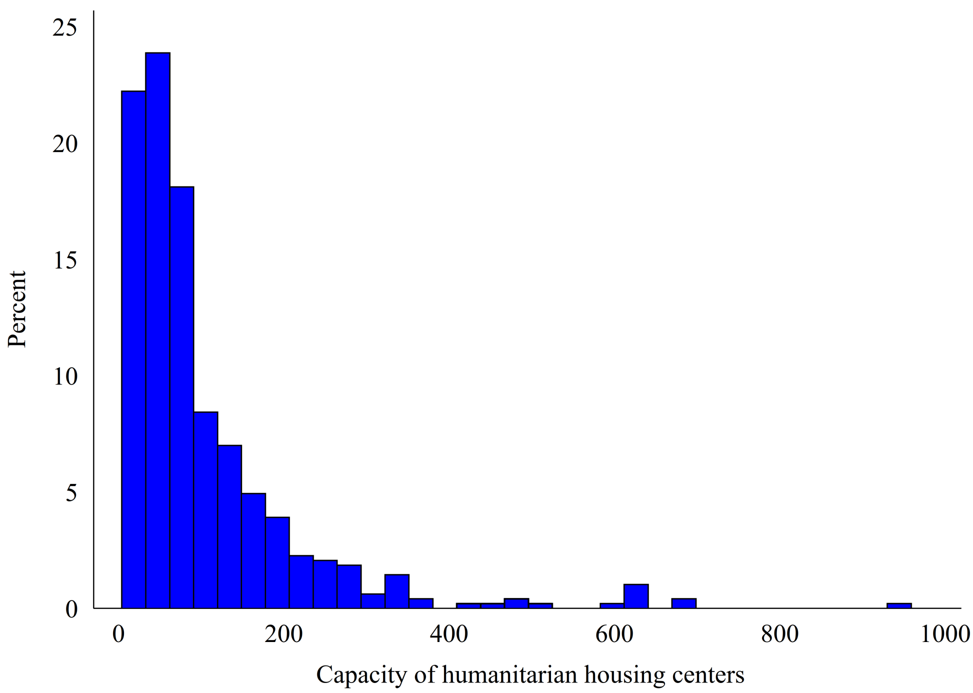


Figure 15: Distribution of the distance between refugee hosuing centers and cities' town-hall

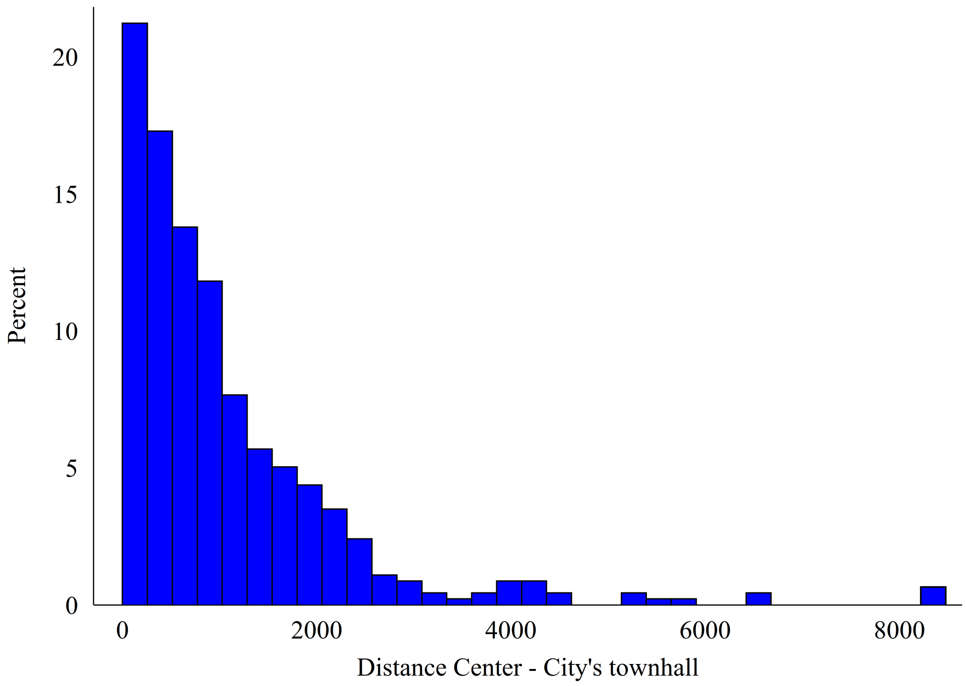


Figure 16: Selection grid to choose CADA centers to open in 2013

Annexe 5

GRILLE DE SÉLECTION
APPEL À PROJETS CRÉATION DE PLACES DE CADA

	CRITÈRES	**Coef. pondé-rateur**	**Cotation (1 à 3)[1]**	**TOTAL**	**Commentaires/ Appréciations**
Projet architectural	Type de structure envisagée *Diffus : 1 point* *Mixte : 2 points* *Collectif : 3 points*	1			
	Type de création de places *Création : 1 point* *Transformation : 2 points* *Extension : 3 points*	1			
	Taille critique de la structure atteinte *Moins de 80 places : 1 point* *Plus de 120 places : 2 points* *De 80 à 120 places : 3 points*	1			
	Accessibilité de la structure aux personnes à mobilité réduite ou atteintes de pathologies lourdes	2			
	Localisation et implantation géographique de la structure par rapport aux besoins locaux	2			
Qualité du projet et de l'opérateur	Personnels : taux d'encadrement adapté et qualification des ETP	3			
	Qualité générale de l'accompagnement proposé	3			
	Implantation locale de l'opérateur et coopération avec des partenaires extérieurs	3			
	Niveau d'expérience de l'opérateur en matière de prise en charge des demandeurs d'asile	1			
	Indicateurs de pilotage des établissements gérés par l'opérateur le cas échéant (taux d'occupation et de présence indue)[2]	2			
	Coopération de l'opérateur avec les services de l'État	3			
Modalités de financement	Coûts de fonctionnement à la place et rapport coût-efficacité au regard du référentiel de coûts	4			
	Mutualisations de moyens proposées et incidences budgétaires	3			
	Cohérence du chiffrage budgétaire avec les moyens annoncés	3			
TOTAL		**32**			**/96**

[1] 1 étant la note la plus basse, et 3 la note la plus élevée.

[2] Si l'opérateur ne gère aucun établissement, ce critère ne sera pas pris en compte et la note maximale sera ramenée à 90 points.

Figure 17: Selection grid to choose CPH centers to open in 2019

ANNEXE 2 Grille de sélection

GRILLE DE SELECTION
APPEL A PROJET – CREATION DE PLACES DE CPH en 2019

	CRITERES	Coef. Pondérateur	Cotation (1 à 3)	TOTAL	Commentaires / Appréciations
ralarchitectuProjet	Capacité à mettre en œuvre rapidement	4			
	Modularité des places proposées (accueil familles et isolés)	4			
	Accessibilité de la structure aux personnes à mobilité réduite.	1			
	Localisation et implantation géographique de la structure (niveau de demande de logement social, accès à la santé, à l'enseignement, aux transports) et position des élus locaux	4			
Qualité du projet et de l'opérateur	Personnels : Taux d'encadrement (minimum 1 ETP pour 10 résidents et qualification des ETP).	2			
	Contenu des prestations administratives et sociales conformes au cahier des charges.	3			
	Implantation locale de l'opérateur et coopération avec les partenaires extérieurs.	3			
	Niveau d'expérience de l'opérateur en matière de prise en charge des réfugiés.	2			
	Indicateurs de pilotage des établissements gérés par l'opérateur le cas échéant (taux d'occupation, durée de séjour, taux de sortie vers le logement, accès à l'emploi).	2			
	Coopération de l'opérateur avec les services de l'Etat.	3			
Modalités de financement	Coût de fonctionnement à la place au regard du coût ciblé par le cahier des charges (25 €)	2			
	Cohérence du chiffrage budgétaire avec les moyens annoncés.	3			
TOTAL		33			**/ 99**

A.2. *Original Difference-in-Difference*

Table 10: Extreme-right vote share at presidential elections

	Municipality level			Polling station level
	(1) Full Sample	(2) Only Treated	(3) Matching	(4) Only Treated
Center opening	-0.058***	-0.025***	-0.014	-0.028***
	(0.006)	(0.007)	(0.009)	(0.010)
Election year FE	Yes	Yes	Yes	No
Municipality FE	Yes	Yes	Yes	No
Polling-Station FE	No	No	No	Yes
Municipality-time FE	No	No	No	Yes
% ATTs with negative weights	8.2%	46.5%	17.7%	0.007%
Observations	169,169	2,230	5,560	6,110

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level for (1) to (3) and at the polling station-level for (4). "FE" stands for Fixed Effects.

Table A.2 shows that the weights using a standard difference-in-difference can be negative. The negative weights are an issue when the treatment effect is heterogeneous between groups or over time as one one could have that the treatment's coefficient in those regressions is negative while the treatment effect is positive in every group and time period.

A.3. *Pooled European and Presidential Elections*

Figure 18: Timeline of European and Presidential Elections in France

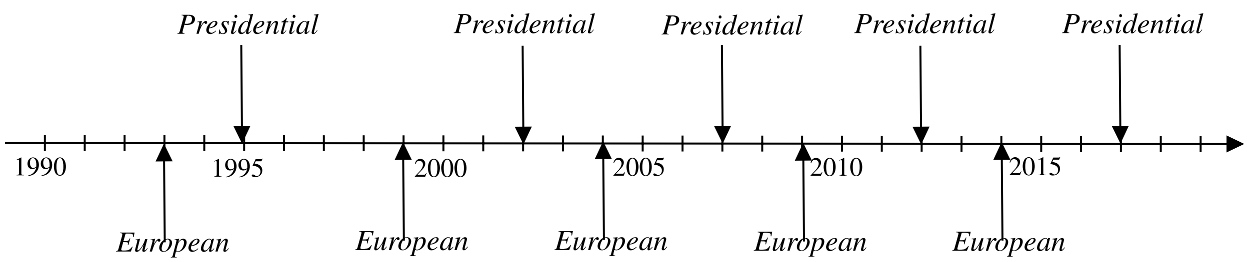
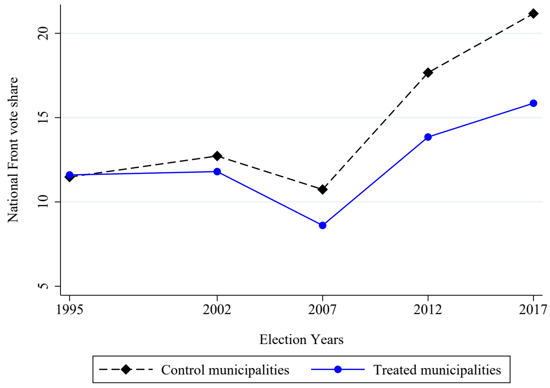
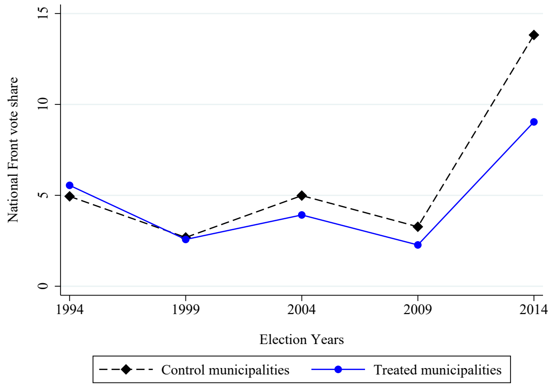


Figure 19: National Front vote-share at Presidential and European elections

(a) Presidential elections



(b) European elections



(c) Presidential and European elections

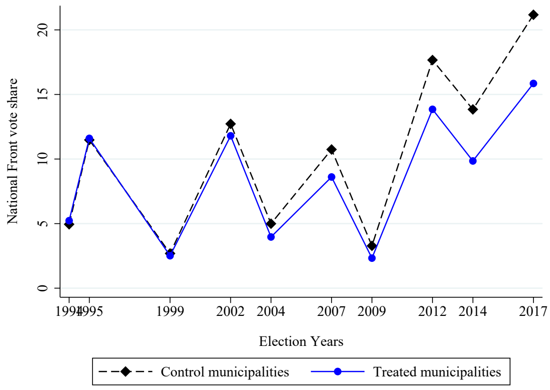


Table 11: Effect of a refugee center openings on voting for the National Front voting at presidential and European elections

	(1) Full Sample	(2) Only Treated	(3) Matching
Center opening	-0.043***	-0.037***	-0.039***
	(0.009)	(0.009)	(0.009)
Election year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Observations	285,434	3,372	7,153

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

Table 12: Extreme-right vote share at presidential elections- Heterogeneity by municipalities' characteristics

	Population			**% Old**			**% Tertiary**			**% High skilled**		
	High	Low	Diff	High	Low	Diff	High	Low	Diff	High	Low	Diff
Extreme-right vote share (1995)	11.60	12.40	-0.80***	13.35	10.13	3.22***	11.61	11.62	-0.00	11.54	11.81	-0.27***
Extreme-right vote share (2017)	21.53	14.66	6.87***	22.24	20.63	1.61***	22.02	18.21	3.82***	22.44	18.74	3.69***
Observations	33122	783	33905	15662	18243	33905	28159	5746	33905	24185	9720	33905
	Revenues			**% Unemployed**			**% Vacant House**			**% Migration**		
	High	Low	Diff	High	Low	Diff	High	Low	Diff	High	Low	Diff
Extreme-right vote share (1995)	10.31	13.12	-2.81***	11.69	11.41	0.28***	12.40	10.62	1.78***	11.47	12.65	-1.17***
Extreme-right vote share (2017)	21.44	21.30	0.15	21.04	22.36	-1.32***	21.36	21.40	-0.05	21.64	19.48	2.16***
Observations	18140	15765	33905	25168	8737	33905	18970	14935	33905	29751	4154	33905

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Predicted extreme-right vote share at 1995 Presidential election

	Mean	S.D.	Min	Max	Obs.
Predicted extreme-right vote-share (Treatment)	11.53	2.62	5.16	22.41	446
Predicted extreme-right vote-share (Control)	11.62	2.45	-1.98	23.60	33,424

Predicted value of the vote-share for the extreme-right at 1995 presidential election from a regression with all controls described in Table 1.

Table 14: Heterogeneity by municipalities' characteristics – Effect of refugee center openings on far-right voting at presidential elections

	Population		**% Old**		**% Tertiary**		**% High skilled**	
Outcome: vote-share	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
of the extreme-right	High	Low	High	Low	High	Low	High	Low
Center opening	-0.008	-0.035***	-0.040***	-0.032***	-0.008	-0.073***	-0.014**	-0.059***
(Full sample)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.016)	(0.007)	(0.013)
Center opening	-0.016*	-0.045**	-0.016	-0.016*	-0.010	-0.052**	-0.012	-0.044**
(Only treated)	(0.009)	(0.021)	(0.011)	(0.009)	(0.009)	(0.021)	(0.009)	(0.020)
Center opening	-0.010	-0.022**	-0.016**	-0.015*	-0.005	-0.045***	-0.007	-0.028**
(Matching)	(0.007)	(0.009)	(0.008)	(0.008)	(0.008)	(0.011)	(0.007)	(0.025)
Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs. (Full sample)	3,125	131,695	72,389	62,423	22,785	112,036	38,578	96,248
Obs. (Only treated)	800	715	737	778	777	738	788	727
Obs. (Matching)	1,457	2,497	1,940	2,006	1,718	2,237	1,607	2,353

	Revenues		**% Unemployed**		**% Vacant House**		**% Migration**	
Outcome: vote-share	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
of the extreme-right	High	Low	High	Low	High	Low	High	Low
Center opening	-0.022***	-0.060***	-0.040***	-0.033***	-0.043***	-0.025***	-0.011	-0.046***
(Full sample)	(0.008)	(0.009)	(0.006)	(0.008)	(0.009)	(0.007)	(0.008)	(0.008)
Center opening	-0.014	-0.027**	-0.024***	0.006	-0.017	-0.010	-0.012	-0.028**
(Only treated)	(0.009)	(0.011)	(0.009)	(0.010)	(0.014)	(0.010)	(0.011)	(0.013)
Center opening	-0.008	-0.030***	-0.024***	0.007	-0.016	-0.007	-0.008	-0.028***
(Matching)	(0.009)	(0.008)	(0.007)	(0.009)	(0.011)	(0.008)	(0.009)	(0.010)
Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs. (Full sample)	62,799	72,017	34,661	10,0158	59,315	75,494	16,410	118,410
Obs. (Only treated)	772	743	756	759	750	765	790	725
Obs. (Matching)	1,925	2,025	1,810	2,143	1,792	2,151	1,741	2,213

Source: Ministry of the Interior, INSEE - French censuses, and IRCOM data. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

Table 15: Heterogeneity by political parties in 1995 – Effect of refugee center openings on far-right voting at presidential elections

Outcome: Vote-share of the extreme-right	**Extreme-Right**		**Right**		**Left**		**Extreme-Left**	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low	(7) High	(8) Low
Center opening (Full sample)	-0.022***	-0.044***	-0.035***	-0.033***	-0.057***	-0.019**	-0.035***	-0.032***
	(0.008)	(0.007)	(0.007)	(0.008)	(0.009)	(0.008)	(0.007)	(0.008)
Center opening (Only Treated)	-0.012	-0.017	-0.014	-0.017*	-0.035***	-0.011	-0.008	-0.016*
	(0.010)	(0.011)	(0.010)	(0.009)	(0.011)	(0.010)	(0.011)	(0.009)
Center opening (Matching)	-0.009	-0.015	-0.006	-0.018**	-0.034***	-0.002	-0.013	-0.012
	(0.008)	(0.010)	(0.008)	(0.008)	(0.010)	(0.009)	(0.008)	(0.008)
Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs. (Full sample)	66,963	68,071	93,075	41,960	57,010	78,024	59,123	75,908
Obs. (Only Treated)	759	749	749	760	749	759	758	747
Obs. (Matching)	2,153	1,794	1,977	1,971	1,840	2,107	1,989	1,955

Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

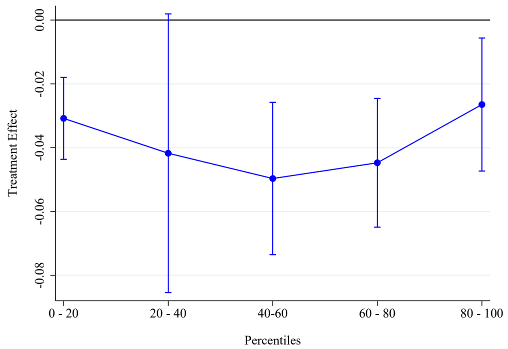
Table 16: Treatment heterogeneity by centers' characteristics – Effect of refugee center openings on far-right voting at presidential elections

Outcome: Vote-share of the extreme-right	**Exposure time**		**Distance to city-center**		**Capacity**		**European asylum-seekers**		**Refugee-topic in the press**	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low	(7) High	(8) Low	(9) High	(10) Low
Center opening (Full sample)	-0.032***	-0.044***	-0.039***	-0.036***	-0.024***	-0.033***	-0.043***	-0.017*	-0.021***	-0.045***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.00)	(0.009)	(0.008)	(0.008)
Center opening (Only Treated)	-0.019**	-0.018*	-0.024**	-0.015	-0.023*	-0.012	-0.027***	0.005	-0.029***	-0.023**
	(0.009)	(0.011)	(0.011)	(0.011)	(0.013)	(0.010)	(0.009)	(0.011)	(0.011)	(0.011)
Center opening (Matching)	-0.015*	-0.019**	-0.021**	-0.012	0.006	-0.013	-0.024***	0.006	-0.001	-0.024***
	(0.008)	(0.009)	(0.010)	(0.009)	(0.010)	(0.008)	(0.008)	(0.010)	(0.008)	(0.009)
Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs. (Full sample)	134,161	134,148	134,213	134,095	134,155	134,147	134,387	133,919	134,312	133,988
Obs. (Only Treated)	761	751	808	703	756	749	948	561	885	618
Obs. (Matching)	3,082	3,081	3,130	3,032	3,088	3,068	3,312	2,848	3,241	2,913

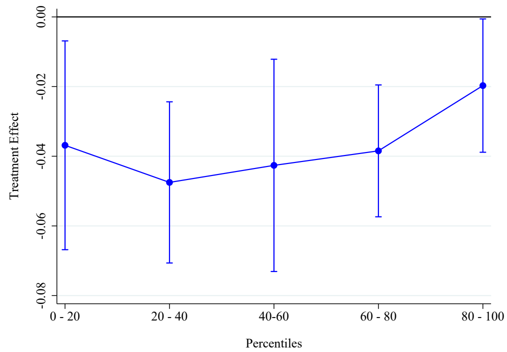
Source: Ministry of the Interior. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level. "FE" stands for Fixed Effects.

Figure 20: Treatment heterogeneity by centers' characteristics – Effect of refugee center openings on far-right voting at presidential elections

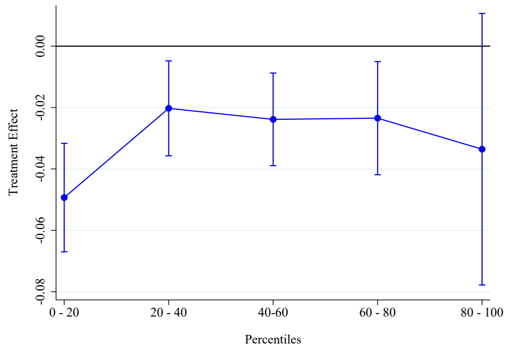
(a) Time distance (months) between the date of centers' opening and the next election



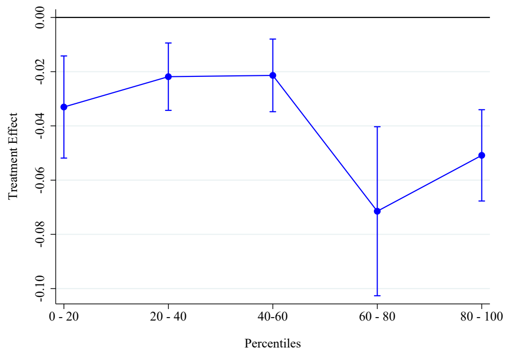
(b) Distance (km) between the center and the town-hall



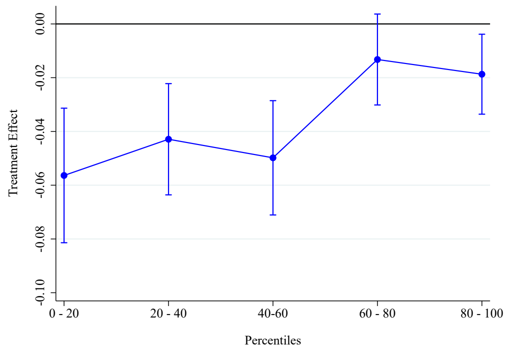
(c) Capacity of the center relative to the municipality's population



(d) Share of European asylum seekers at the time of centers' opening



(e) Salience of the refugee-topic in the national press at the time of centers' opening



Source: Ministry of the Interior, "annuaire de l'administration", OFPRA, and Europress data. Note: Figure 20 examines whether the effect of the drop in the extreme-right vote following the opening of the refugee center is modified with the characteristics of the center opening. I use the full sample specification and subdivide the treated municipalities into 5 subsamples at 20, 40, 60, 80 percentile of the distribution of the target characteristic at the election period when they become treated. The incertitude of each point is asserted with a 90% confidence interval. Estimated β_S from equation (2) in the full sample specification where the percentile distribution over variables described in (a), (b), (c), (d), and (e). The dependent variable is the log vote share of the extreme-right at presidential election's first round. Weighted by the number of registered voters at the beginning of the period. Standard errors are clustered at the municipality level.

A.5. Propensity Score Matching

As discussed by Ryan et al. (2019), matched difference-in-differences tend to perform better in dealing with non-parallel trends. As suggested by Kahn-Lang and Lang (2019), difference-in-difference estimates are more plausible if the treatment and control groups are similar first in levels and not only in trends.

I match control and treated municipalities regarding the following socio-demographic characteristics in 1995: the population number, density, whether the municipality is rural, share of vacant housing, share of immigrants, share of unemployed persons, share of young persons, share of farmers, share of executives, share of the population with no diploma, share of the population with a baccalaureate, share of the population with higher education, residents' income and road distance to the department's prefecture. I perform the propensity score matching with 2 neighbors, no replacements, and a caliper of 0.1. In Tables 17 and 18 I report the covariates means in the control and treatment groups with and without matching, as well as the p-value of t-test of the difference between the mean of each covariate in the treatment and control group.

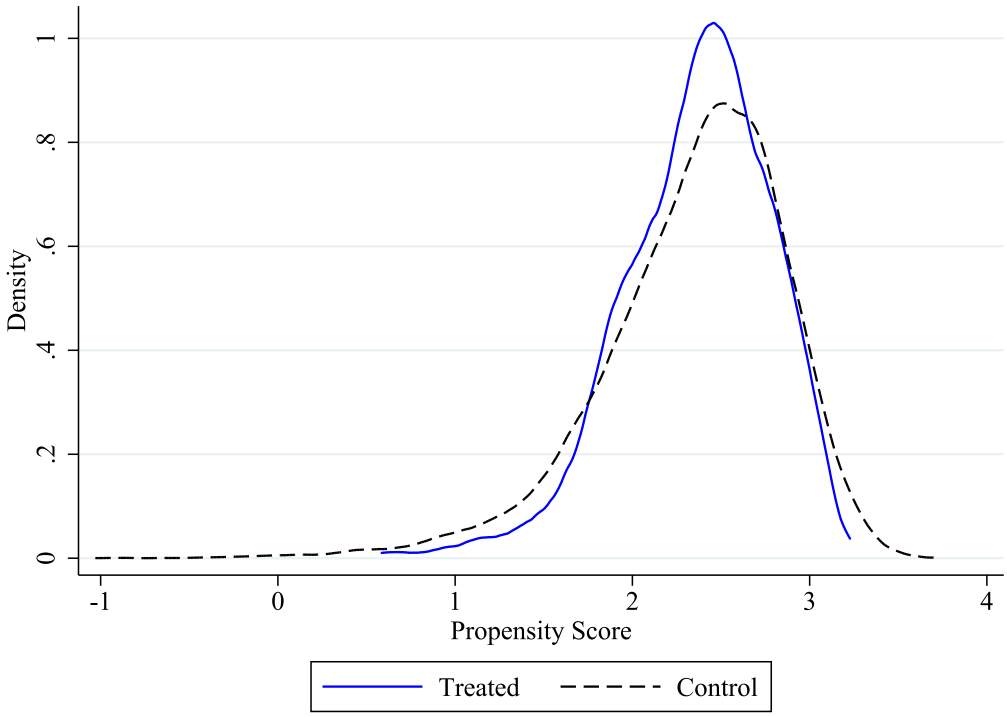
Table 17: T-tests without matching - 1995

	Control group (mean)	Treatment group (mean)	P-value
Population (log)	9.0523	6.1757	0.000***
Density	12.842	1.3433	0.000***
Rural municipality	.26022	.48683	0.000***
Vacant housing share	.07259	.07201	0.752
Immigrants share	.05901	.02133	0.000***
Unemployed share	.1146	.08494	0.000***
Youth share	.2605	.25879	0.525
Farmers share	.03866	.15632	0.000***
Executives share	.6779	.54728	0.000***
No diploma share	.5432	.57798	0.000***
Baccalaureate share	.22684	.22969	0.335
Higher education share	.12569	.09605	0.000***
Residents' income	12,265	11,911	0.032**
Road distance to prefecture	9.795	10.515	0.000***
Observations	35,571	467	

Table 18: T-tests with matching - 1995

	Control group (mean)	Treatment group (mean)	P-value
Population (log)	9.0373	9.0548	0.868
Density	12.809	13.004	0.877
Rural municipality	.26247	.2744	0.683
Vacant housing share	.07254	.07421	0.385
Immigrants share	.05883	.06417	0.119
Unemployed share	.11447	.11416	0.912
Youth share	.26053	.26108	0.840
Farmers share	.03895	.03723	0.683
Executives share	.67747	.67538	0.778
No diploma share	.54364	.54253	0.850
Baccalaureate share	.22674	.22401	0.324
Higher education share	.1254	.1293	0.379
Residents' income	12,267	12,499	0.207
Road distance to prefecture	9.8177	9.8821	0.441
Observations	731	467	

Figure 21: Common Support



A.6. *Instrumental Variable Strategy*

As described in the Sections II.2 and III.2, the context of the spatial allocation of housing centers in France can be used as a quasi-experimental design to study the effect of the opening of a refugee center on the vote for the extreme-right. As an additional robustness test, this section follows the literature that look at potentially non-randomly allocated centers (Steinmayr, 2020; Vertier and Viskanic, 2019; Gamalerio et al., 2020) and use instrumental variables to circumvent potential endogneity issues.

In the Austrian context, Steinmayr (2020) uses the existence of group accommodations in a municipality as an instrument as it can increase the likelihood of hosting asylum seekers in a municipality. Similarly, the empirical strategy of this section uses the existence of group accommodation as an instrument for the opening of refugee centers. The exclusion restriction requires the number of group accommodation to be unrelated to changes in the far-right vote share; other than by increasing the probability to open a refugee center. To circumvent potential reverse causality concern, I use the existence of group accommodation measured at the beginning of the period, before refugee centers open. To do so, I use the FINESS data from 2004 to 2017 on the number of group accommodation for persons with disabilities, elderly, child protection and other institutions[16] excluding centers for asylum seekers and refugees.

Municipalities with group accommodations may have different socio-economic characteristics that could generate differential political trends. To account for this endogeneity threat, I condition on a set of covariates that capture relevant municipality characteristics that could be correlated with far-right voting. I use the population size, the share of workers in the primary sector, the share of workers in the secondary sector, the share of men, the share of 0 - 19 years-old individuals, the share of more than 65 years-old individuals, the share of migrants, the unemployment share, the share of people with no education, some education but without the baccalaureate, and tertiary-level education, the share of vacant housing, and the average revenue of the municipality population. These variables are introduced as controls in levels at the beginning of the period in 2007, and in changes in the previous electoral period 2002 - 2007.

As in Steinmayr (2020) and in Vertier and Viskanic (2019), the identification hypothesis is that, conditional on this set of covariates, it is unlikely that municipalities with and without group accommodations follow different political trends. The first-stage equation is the following:

$$\Delta Opening_{i,t} = \alpha_0 + \alpha_1 GroupAccommodation_{i,t-1} + \mu X_i + \lambda_p + \gamma_d + \upsilon_{i,p} \qquad (3)$$

with municipality i and period $t \in [2007; 2012; 2017]$; ; $Opening_{i,i}$ equals the number of centers' openings in municipality i at time t; $GroupAccommodation_{i,t}$ equals the number

[16] "Établissements d'accueil, hébergement, réadaptation et services", Finess Code 4000.

of group accommodation in the municipality at the beginning of each election periods (in 2007 and in 2012); X_i are control variables; λ_p and γ_d period and department fixed-effects respectively. Standard errors are clustered at the department level. For the 2SLS estimation, I estimate the following equation:

$$\Delta log(FarRightVote_{i,t}) = \beta_0 + \beta_1 \Delta Opening_{i,t} + \delta X_i + \lambda_t + \gamma_d + \varepsilon_{i,t} \tag{4}$$

with municipality i and period $t \in [2007;2012;2017]$; $log(FarRightVote_{i,t})$ is the difference in log vote share for the far-right between 2012 and 2007, and between 2017 and 2012; $Opening_{i,t}$ equals the number of centers' openings in municipality i in the period 2007 - 2012 or in the period 2012 - 2017; X_i are control variables; λ_t and γ_d period and department fixed-effects respectively. Standard errors are clustered at the department level.

Table 19: First stage (IV): Effect of group accommodation on refugee centers openings

Outcome: refugee centers openings	(1)	(2)	(3)
Group Accommodation	0.016^{***}	0.017^{***}	0.017^{***}
	(0.001)	(0.002)	(0.002)
Controls in 2007	No	Yes	Yes
Controls Δ 2002-2007	No	No	Yes
Period & department FE	Yes	Yes	Yes
Observations	67,653	67,349	67,285
F-statistic	124.70	97.72	96.45

Source: Ministry of the Interior, FINESS, Census, and IRCOM data. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table displays the results of the first-stage of a 2SLS regression. Weighted by the number of registered voters in 2007. Standard errors are clustered at the department level. " FE" stands for Fixed Effects.

Table 19 shows the first stage regression results using the number of accommodation buildings as an instrument for the opening of refugee centers. The specifications with controls suggest that one additional group accommodation increases by 1.7 the number refugee housing centers. The first stage is strong with a F-statistic for the excluded instrument of 96.45 with all controls, which is higher than Stock and Yogo (2005) threshold of 16.38. According to Lee et al. (2020), this F-statistic is equivalent to a 5.11 percent test or to a critical value of 1.98. Table 20 shows the results of the second stage 2SLS estimation where the opening of a refugee center decreases the growth rate of the vote for the far-right by 1.7 to 1.8 percentage points.

Table 20: Second stage (IV): Effect of refugee center openings on far-right voting at presidential elections

Outcome: far-right vote-share	(1)	(2)	(3)
Center opening	-0.069***	-0.018***	-0.017***
	(0.009)	(0.006)	(0.006)
Controls in 2007	No	Yes	Yes
Controls Δ 2002-2007	No	No	Yes
Period and department FE	Yes	Yes	Yes
Observations	67,653	67,349	67,285
F-statistic	377.13	49.61	39.37
R^2	0.63	0.06	0.07

Source: Ministry of the Interior, FINESS, Census, and IRCOM data. Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table displays the results of the second-stage of a 2SLS regression. Weighted by the number of registered voters in 2007. Standard errors are clustered at the department level. " FE" stands for Fixed Effects.