Dismantling the "Jungle": Migrant Relocation and Extreme Voting in France

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Abstract

Large migrant inflows have spurred anti-immigrant sentiment, but can small inflows have a different impact? We exploit the redistribution of migrants after the dismantling of the "Calais Jungle" in France to study the impact of the exposure to few migrants, which we estimate using difference-in-differences and instrumental variables. We find that in the presence of a migrant center (CAO), the growth rate of vote shares for the main far-right party (Front National (FN), our proxy for anti-immigrant sentiment) between 2012 and 2017 is reduced by about 12 percentage points. This effect, which crucially depends on the inflow's size, points towards the contact hypothesis (Allport 1954).

Keywords: Political Economy; Voting; Migration; EU; France; Migrants

JEL Classifications: C36, D72, J15, P16, R23

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1 Introduction and Background

In recent years, the number of asylum applications in the European Union increased from 431 thousand in 2013 to 627 thousand in 2014 and approximately 1.3 million in 2015 (Eurostat 2016). Given the high numbers of migrants reaching Europe and the future increased immigration projections both across and within countries, anticipating how natives respond when interacting with immigrants is crucial. Migrants will influence the labor force's composition, interact with natives in many commercial transactions, and influence politics both on the supply and demand side.

The considerable rise in the number of asylum applications and the difficulties experienced by European countries in redistributing asylum seekers across countries have drawn media, politicians, and scholars' attention. However, the existing literature has provided contradictory evidence, as some studies show that immigration increases the support for far-right parties (Barone et al. 2016; Brunner and Kuhn 2018; Edo et al. 2019; Halla et al. 2017; Harmon 2017; Mendez and Cutillas 2014; Otto and Steinhardt 2014; Viskanic 2017), while others find opposite results (Gamalerio et al. 2021; Lonsky 2020). Specifically for refugee and asylum seekers, Hangartner et al. (2019) and Dinas et al. (2019) show that exposure to migrants on the Greek islands, but no contact with them, increases hostility of natives towards them and voting for the extreme right-wing party "Golden Dawn". In contrast, Steinmayr (2020) shows that the interaction between migrants and natives in Upper-Austria has led to a decrease in votes for the Extreme Right. Additionally, Dustmann et al. (2019) show that the effects of refugee relocation on voting behavior in Denmark differ across rural and urban areas.

This evidence calls for further research on the mechanisms behind these results. Specifically, what is missing in the existing literature is an analysis of the potential role of the immigration inflows' size. It is poorly understood if *small* immigration inflows shape the anti-immigrant sentiment of natives differently than large inflows. This difference is particularly salient in the setting of asylum seekers' migration. This knowledge gap makes it more challenging to develop efficient relocation schemes for refugees across and

within countries. Many national and local governments refuse to host refugees and asylum seekers as they fear a rise in anti-immigrant resentment in places supposed to host the migrants. Hence, understanding whether the effect of refugee migration inflows changes with their size can inform policymakers.

A few reasons can explain this knowledge gap. First, it is challenging to separate the direct effect on voting behavior from the indirect effect through mediating variables. In many of the studies above, the effects are likely to be *indirect*. Large migration waves are likely to affect different intermediate variables, such as amenities, public spending, the labor market, or the local economy, which in turn affect voting. Therefore, identifying the direct effects is empirically challenging, as it requires settings in which indirect effects are negligible. Second, collecting information on the size and duration of exposure to migrants is a hard task that may require many hours of work. Third, migration inflows are not random, as many economic factors can affect locational choices (Ravenstein 1885. Hence, one needs a source of exogenous variation in the migrants' final location. The same requirement applies to the case of asylum seekers (Hangartner et al. 2019; Neumayer 2005).

Our setting enables us to deal with these challenges. We focus on the dismantling of the Calais "Jungle", an encampment in the North of France. In October 2016, this illegal camp reached 6,400 inhabitants (Le Monde 2016), shortly before the government closed it and relocated the migrants in other areas of the country. Between October 2015 and 2016, the government relocated the migrants to more than 300 migrant centers called *Centres d'Accueil et d'Orientation (CAOs)*.

This setting presents important advantages that we exploit in the analysis below. First, it is unlikely that the relocation affected the local economy. CAOs hosted the migrants for a short period (typically less than three months), during which they did not have the right to work. Besides, the central government paid the cost of the relocation. These conditions enable us to study the effect of direct contact between migrants and natives while excluding potential indirect effects. Consistent with this claim, in the analysis below, we show that migrants' did not affect the local economic activity. Second, we collected information about CAOs' location through a systematic analysis of local newspapers

(Factiva) and combined them with a dataset that was publicly released by CIMADE (the main association helping migrants) on October 24^{th} 2016. We also collected precise information on CAOs' size. Among the municipalities that hosted a CAO, we find that, on average, these centers could host 31 migrants at the same time, which means 16 migrants per 1000 inhabitants.[1]

Third, this framework enables us to link municipality-level variation in exposure to migrants to electoral outcomes. Specifically, we exploit the fact that the 2017 French presidential election was held after dismantling the Calais "Jungle". We use the change in the FN municipal-level vote shares from the 2012 and 2017 presidential elections as the main outcome in our analysis and as a proxy for anti-immigrant sentiment. During the campaign, the rhetoric of the FN was anti-immigrant, referring continuously to the migrant crisis. The FN diffused this anti-immigrant stance through general and social media, public gatherings, and the party's election manifesto.[2]

Finally, this setting allows us to deal with the potential endogeneity of CAOs' location. More in detail, we estimate the effect of CAOs on electoral outcomes in two ways. First, we run a difference-in-differences model that exploits the fact that we do not observe different parallel trends in the elections before the dismantling between municipalities with and without a CAO. However, given the potential involvement of local governments in the allocation process, we cannot consider the assignment of CAOs as random.

Indeed, the French government could have chosen the location of CAO centers exploiting information unobservable to us. We rely on an instrumental variable (IV) approach based on pre-existing (i.e., built before the dismantling of the Calais Jungle) buildings that can accommodate groups of people to deal with this challenge. Specifically, we use two types of buildings that we combine in one instrument. First, we consider the number of "Holiday Villages" ("Village Vacances" in French) located in a municipality.[3] We expect

[1] These numbers (16 migrants per 1000 inhabitants or 0.016 migrants per capita) give an idea of how small was the inflow of migrants generated by CAOs. For example, in the context studied by Hangartner et al. (2019) and Dinas et al. (2019), the Greeks islands were receiving between 1 and 4 asylum seekers per capita, with a few of them receiving more than 4 per capita.

[2] See La Croix (2017), BBC (2017), and Le Monde (2017a) amongst others.

[3] "Holiday Villages" are structures owned by a public company managed by the state to be used by their employees to go on holiday. Since those structures were mostly empty during the dismantling, the central government used them to host migrants. In some cases, these structures were not used, but they

a positive correlation between the presence of a CAO and a holiday village because one of the criteria used for choosing CAOs' location was potential additional space in those holiday villages. Specifically, given that the "Jungle" was shut down mostly in October 2016, the holiday villages would be unoccupied at that time and could thus be used as temporary shelters for migrants. Besides, holiday villages were built mainly in the 1970s, much before the current migrant surge that led to the creation of the CAOs, and certainly not to host migrants.

Second, in line with the recent literature (Gamalerio et al. 2021; Steinmayr 2020), we consider the number of buildings such as homes for the elderly, disabled, drug addicts, and orphans that can accommodate groups of people. The idea behind using these buildings in constructing the instrument is that the government considered several venues, with a strong emphasis on buildings that could commodate groups of people. Besides, these buildings were built before the dismantling of the Calais Jungle and for reasons different than hosting migrants. Our instrument is equal to the sum of the number of these buildings plus holiday villages. Crucially, we show how the instrument correlates with electoral outcomes only for the presidential elections between 2012 and 2017 when the buildings could accommodate migrants due to the Calais Jungle dismantling. In contrast, we do not find any correlation between the instrument and electoral outcomes for the elections between 2007 and 2012. Thus, the exclusion restriction assumption (i.e., the assumption that the instrument must affect the dependent variable only through the endogenous treatment variable) appears plausible in this context. In addition, our regressions consider many potential covariates that control for municipal sociodemographic features and local politicians' characteristics.[4]

Depending on the approach considered, our main results show that the growth in FN's vote shares between the 2012 and 2017 presidential elections was between 4 and 12 percentage points lower in municipalities that hosted a CAO. Looking at the IV results,

were still kept as an alternative solution if collective houses or other empty flats did not prove sufficient.

[4]We also control for the overall tourism level. We think it is important to control for the overall tourism level because touristic municipalities may follow different electoral trends than non-touristic ones. This fact could generate doubts about alternative channels through which holiday villages may affect electoral outcomes besides CAOs.

as the average increase of FN's votes over this period corresponded to about 20%, they indicate that the increase of FN vote in municipalities with a CAO was 40% of the growth rate in municipalities without a CAO. Our interpretation of these findings is that citizens developed greater acceptance towards migrants. The fact that we observe an increase in the vote shares received by the far-left party *Front de Gauche*, which had a more open stance towards migrants, but a similar political platform to the FN on other issues, further confirms our interpretation of the results. In contrast, the evidence appears to rule out that mobilization of voters and changes in electoral turnout drive our results. Besides, we find spillover effects on neighboring municipalities.

Importantly, our analysis shows that municipalities with smaller CAOs drive the main results. Our difference-in-differences estimates suggest that the municipalities in which CAOs negatively affected the FN vote shares hosted approximately less than 47 migrants per 1,000 inhabitants. The IV estimates indicate a threshold of about 32 migrants per 1,000 inhabitants. Above these thresholds, CAO's estimated average effect on FN vote shares switches sign and eventually becomes both positive and statistically significant for very large CAOs. This finding is consistent with the evidence that large inflows contributed to the rise of right-wing parties in many western countries.

Our paper provides three main contributions. First, the event study analyzed led to proper direct contact between natives and migrants, not to a short and transient exposure. Since migrants were not allowed to work, and the government covered the costs, our setting allows us to estimate the effect of direct contact while ruling out potential indirect impacts. As outlined in the next section, we believe this setting meets some of the conditions described by contact theory (Allport 1954), such as authorities' role in supporting the contact between natives and immigrants. Therefore we expect a decrease in anti-immigrant sentiments.

Second, our analysis reveals that the negative effect can potentially become positive in municipalities with many migrants. This evidence suggests that natives may perceive the inflow of new immigrants as a threat to their social, cultural, and economic hegemony when the number of migrants received overcomes a certain threshold. As suggested by

"realistic group conflict theories" (Blalock 1967; Blumer 1958; Bobo 1983; Campbell 1965; Lahav 2004; Quillian 1995; Sidanius and Pratto 1999; Taylor 1998), this perceived threat can potentially determine a rise in prejudice and anti-immigrant sentiment. However, given that migrants could not work, we do not think that the perceived threat generated by big CAOs should be due to economic concerns related to the potential competition in the labor market (Bobo and Hutchings 1996; Mayda 2006; Scheve and Slaughter 2001). In the context studied, it is more likely that natives perceive the opening of too big CAOs as a threat to their identity and cultural dominance (Golder 2003). In addition, large CAOs may have made fostering contact between natives and immigrants more complicated, leaving the natives affected only by a pure exposure effect not counterbalanced by contact, which can explain the rise in anti-immigrant sentiments (Dinas et al. 2019; Hangartner et al. 2019). Third, the evidence provided in this paper has a clear and direct policy implication. It suggests that governments should develop a more proportional relocation mechanism (Bansak et al. 2017), redistributing refugees in a more homogeneous and diffuse way.

2 Conceptual framework

This section summarizes the main theories that drive our empirical analysis on the effect of the contact between immigrants and natives. It also briefly describes the predictions that originate from these theories and how they apply to our context. For more detailed reviews on these theories see the works of Paluck and Green (2009), Hainmueller and Hopkins (2014), Hangartner et al. (2019), and Dustmann et al. (2019).

We refer to two theories. The first is contact theory (Allport 1954), which describes how the direct contact between immigrants and natives can reduce anti-immigrant sentiments when the following four conditions are met: equal status between the two groups, common goals, intergroup cooperation, and the support of authorities. However, as suggested by the literature (Hangartner et al. 2019), it is difficult to find natural experiments and event studies in which all these conditions are simultaneously met. Besides, the lit-

erature has shown how direct contact can potentially reduce prejudice, even when only a subset of these conditions is met (Paluck et al. 2019; Pettigrew and Tropp 2006). Specifically, some scholars have suggested and provided evidence that contact between migrants and natives can increase knowledge about the outgroup, leading potentially to a reduction in prejudice (Barlow et al. 2012; Pettigrew and Tropp 2008).

The second stream of theories is the one that Campbell (1965) labeled "realistic group conflict theories" (Blalock 1967; Blumer 1958; Bobo 1983; Sidanius and Pratto 1999). According to this theoretical framework, natives can potentially perceive the inflow of a sufficiently big group of immigrants as a threat to their social, cultural, and economic dominance. This threat can then lead to an increase in prejudice against the outside group and a rise in anti-immigrant sentiment. Consistent with these intuitions, Taylor (1998) suggests that an increase in the outside group's size can lead to a rise in prejudice. Besides, Quillian (1995) and Lahav (2004) indicate that the largest is the size of the outside group, the highest is the threat perceived by the members of the dominant group.

Besides these two main theories, recent evidence in the literature (Dinas et al. 2019; Hangartner et al. 2019) shows how the effect of exposure to migrants without contact can lead to an increase in prejudice and exacerbate anti-immigrant sentiments. For example, as documented by Hangartner et al. (2019) for the case of the Greek islands, exposure without contact can exacerbate anti-immigrant sentiments when the arrival of big numbers of migrants can generate disruptions in natives' everyday life. This disruption could be simply represented by authorities' inability to provide basic services such as waste collection and medical support while dealing with the inflow of migrants. Natives can then perceive this disruption of everyday life as a threat to social order, leading to an increase in prejudice.

Which predictions can we generate from these theories that can guide the empirical analysis in the context of the Calais "Jungle" dismantling? According to the original formulation of the contact theory (Allport 1954), the contact between natives and immigrants should lead to a decrease in anti-immigrant attitudes when the four conditions described above apply. However, more recent investigations of the theory suggest that

a subset of these conditions can lead to a reduction in anti-immigrant attitudes (Paluck et al. 2019; Pettigrew and Tropp 2006). In our setting, national and local governments had an essential role in managing the dismantling and the relocation of migrants. Hence, given the involvement of national and local authorities in supporting the contact between natives and immigrants, we can expect the opening of CAO centers to reduce the FN's vote shares. Besides, the small immigration inflows generated on average by the opening of CAO centers should have increased the likelihood of contact and intergroup cooperation. Finally, the contact between natives and a small group of migrants should have increased the knowledge about the outside group, potentially generating a prejudice reduction (Barlow et al. 2012; Pettigrew and Tropp 2008). Thus, based on the general features of the event studied in this paper, we expect the effect on the FN's vote shares to be negative.

On the other hand, we know that CAOs centers' size was heterogeneous across municipalities, with some receiving more migrants than others. Hence, following the intuitions of the "realistic group conflict theories" (Campbell 1965) and the evidence on the effect of exposure without contact (Dinas et al. 2019; Hangartner et al. 2019), we can expect the baseline effect of the opening of CAOs centers on FN's vote shares to be heterogeneous across the size. Specifically, we can expect this effect to become smaller and eventually become positive when the centers' size becomes sufficiently big. For example, natives may perceive the inflow of many migrants as an economic or cultural threat, or large inflows may make contact with migrants more complicated, leaving the natives to be affected only by a pure exposure effect. In conclusion, given the theoretical intuitions provided by the contact theory, the "realistic group conflict theories," and the recent literature on the pure effect of exposure, we expect the effect of the CAO centers on FN's vote shares to change with the inflow size.

3 Institutional Framework and Data

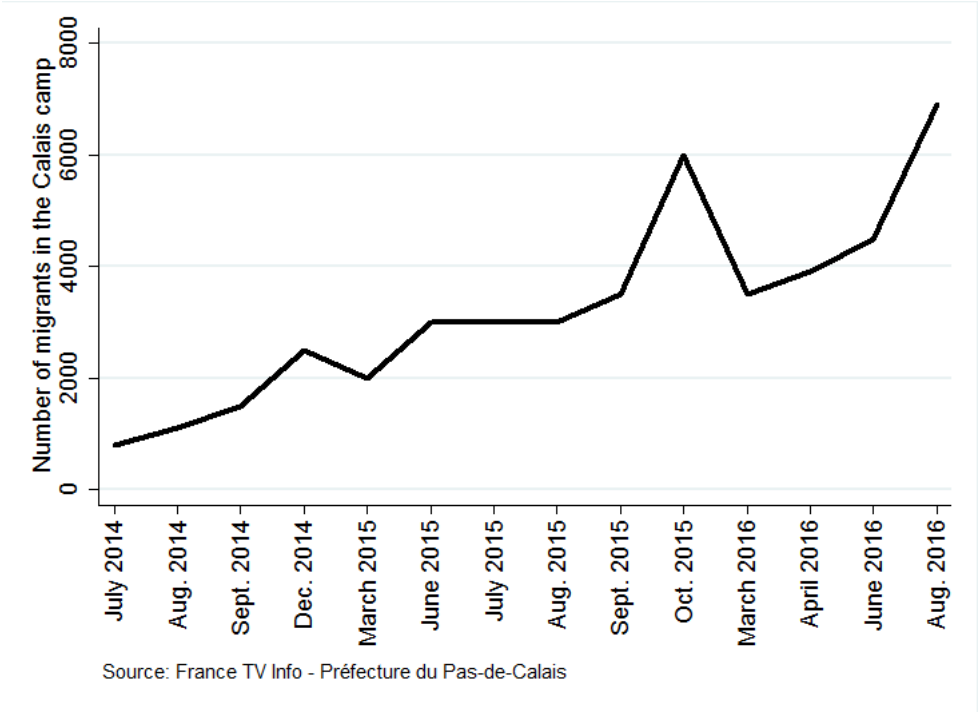
3.1 Migrants and the Calais "Jungle"

The Calais "Jungle" was a migrant camp, which first took form in the late 1990s and grew following the European migrant crisis in 2014-2015, reaching a peak of more than 7,000 inhabitants in 2015 (Figure 1). Therefore, the government decided to dismantle the camp progressively starting from October 2015 by creating CAOs, temporary reception centers established to deal with this crisis. These centers aim at receiving migrants who have not yet started procedures to obtain refugee status. They receive bed and board but no separate financial assistance. The average cost to the government is 25 Euros a day (Ministère de l'Intérieur 2017). Migrants are meant to stay in CAOs only for a short period, usually three months, and then move to other reception centers.[5] Migrants who have started a procedure to obtain refugee status are redirected to the CADA (*Centres d'Accueil pour Demandeurs d'Asile*) while awaiting a decision. Between 2015 and 2017, the CADA places increased to 40,000 places (La Cimade 2017). Other structures were also created over time, such as the AT-SA (*Accueil Temporaire du Service de l'Asile* - 6,000 places), the HUDA (*Hebergement d'Urgence des Demandeurs d'Asile* - 15,000 places), the CPH (*Centre Provisoire d'Hebergement* - 2,300 places), and PRAHDA (*Programme d'Accueil et d'Hebergement des Demandeurs d'Asile* - 5,351 places) (La Cimade 2017).

The dismantling occurred between October 2015 and October 2016. The government reported having relocated 13,366 migrants of those more than 7,000 inhabitants in October 2016. This event received considerable media attention (Figure 2, shows mentions for "Jungle de Calais"). To the best of our knowledge, the French government did not provide official information on the location of the CAOs. The total number of CAOs is also uncertain, with different government sources citing different numbers (more details on request). To circumvent this issue, we combine the manual collection of information with a public database released by *CIMADE* in October 2016. Using Factiva, we systematically

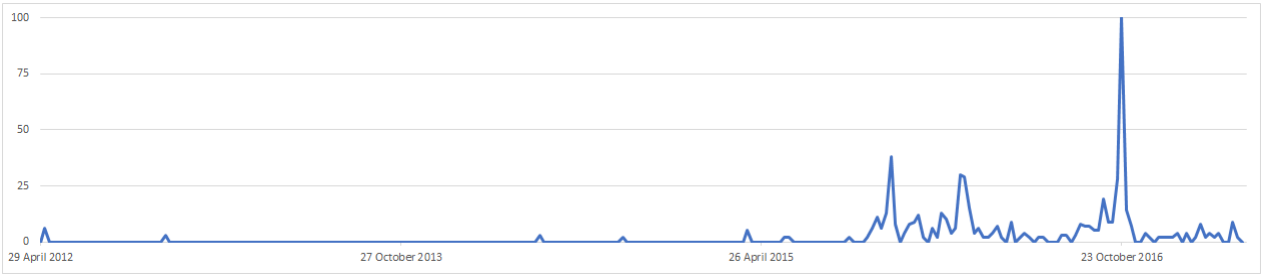
[5]The general rule was that migrants had to move to other receptions centers, and in particular CADA centers. However, we cannot exclude that some migrants remain to live in the municipality where the CAO was established in few specific cases. However, we do not have specific data on these cases.

Figure 1: Evolution of the number of migrants in the Calais camp



searched for articles mentioning the terms "CAO" for each French *département*. When available, we recorded the number of migrants. This procedure enabled us to recover 291 CAOs. We combined this information with a dataset provided by CIMADE, listing 210 centers and their capacity. The union of these two datasets results in 349 centers, close to the government's number in January 2017, namely 365 (Ministère de l'Intérieur 2017). Therefore, there should be only a few CAOs missing, if at all. Hence, since we probably assign some treated municipalities to the control group, we would slightly underestimate our treatment effect.

Figure 2: Google Trends for the expession "Jungle de Calais"



We also create a measure of CAO capacity through the following procedure. For CAOs recorded only in our manually collected dataset, we define a CAO's capacity as the maximum number of migrants ever recorded among all articles mentioning it. For CAOs belonging only to the CIMADE dataset or our manually collected dataset and the CIMADE dataset, the capacity is measured using the number of beds in the CIMADE dataset.[6] This measure of capacity cannot give information about the *total* number of migrants or the length of their stay. However, it informs about the *maximum* number of migrants that could be hosted at any point in time.

The second challenge is that the criteria of allocation of the CAOs have not been clearly defined, making the use of an instrumental variable approach important to validate the results. Even though the government announced that the allocation of CAOs across regions would be based on "socio-demographic criteria" (Ministère de l'Intérieur 2017), no comprehensive list of factors was provided, except for the fact that the Parisian agglomeration and Corsica would be excluded.

Finally, the last issue to consider is the extent to which the mayors were involved in the allocation process. Although many mayors were contacted to receive migrants (Association des Maires de France 2016; Le Monde 2015), during the final dismantling, the Minister of Interior, entrusted the final decision to the local representatives of the government i.e. the *préfets*.[7] The *préfets* would first identify suitable premises without prior consultation and then negotiate with the mayors. Even though mayors' compliance is not generally observed, we exploit additional information about a list of mayors who publicly declared their willingness to welcome migrants. We use this list of mayors as an additional control variable in the analysis below.

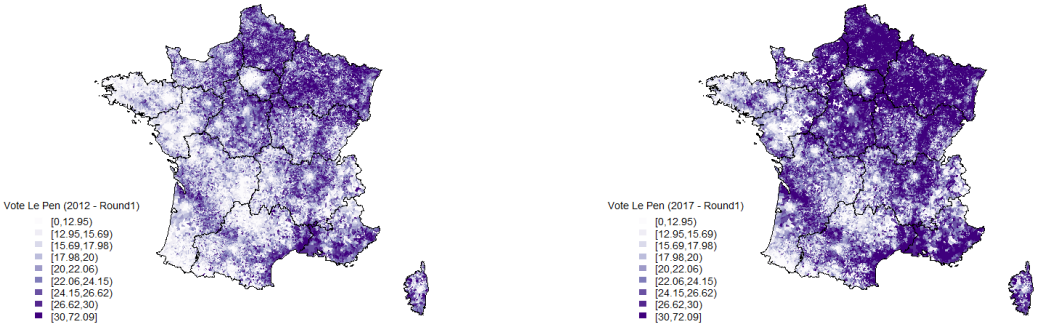
[6]Reassuringly, even though our capacity measure is not defined in the same way, its internal consistency seems warranted. To check it, we compare, among CAOs observed in both datasets, the maximum number of sheltered migrants observed in our manually collected dataset and the capacity registered in the CIMADE dataset. Excluding outliers for which the difference between the two measures is more than two standard deviations away from the mean in absolute value, i.e., less than 10% of cases, the correlation between the two measures is 88%. Therefore, our capacity measure is likely to indicate the number of migrants that were actually sheltered in CAOs.

[7]The *préfets* have authority at the level of the *département*.

3.2 French Presidential Elections

French presidential elections have been held every five years since 2002, using a two-round majoritarian system. If no candidate receives more than 50% of the expressed votes, a second-round is held between the two candidates with the largest shares. Our main outcome of interest is the share of votes received by the FN candidates in the first round of the presidential election. Figure 3 shows the geographic repartition of FN voters between 2012 and 2017. The FN's strongholds are the south-eastern and north-eastern parts of France. In those areas, more than 30% of the population voted for FN both in 2012 and 2017. One can also see that the FN vote increased substantially between 2012 and 2017 (by 20% on average).

Figure 3: FN vote shares in the first round of 2012 and 2017 presidential elections



(a) FN vote share - 2012

(b) FN vote share - 2017

4 Empirical Specifications

4.1 Difference-in-differences Approach

The first approach that we use is the following difference-in-differences model:

$$log(FN)_{i,t} = \gamma_0 + \gamma_1 CAO_i + \gamma_2 2017_t + \gamma_3 CAO_i \cdot 2017_t + \gamma_k X_{k,i,t} + \xi_{i,t} \quad (1)$$

Where $log(FN)_{i,t}$ is the log of FN votes shares in municipality i and presidential

election at time t, with t = 2017, 2012; CAO_i is a dummy equal to 1 if the municipality i has a CAO, and $2017_t = 1$ for the 2017 presidential election. In the vector $X_{k,i,t}$, we find k control variables for municipality i and year t, outlined in the data described in the online Appendix. Specifically, we use all the socio-economic controls, political and administrative characteristics, and mayors' demographics. We cluster the standard errors at the municipality level. The coefficient of interest is γ_3, which captures the effect of CAO on the dependent variable. Then we modify equation 1 in the following version with municipal and year of election fixed effects:

$$log(FN)_{i,t} = \beta_0 + \beta_1 CAO_i \cdot 2017_t + \beta_k X_{k,i,t} + \delta_i + \lambda_t + \xi_{i,t} \tag{2}$$

Where the municipal FE δ_i control for all the municipal characteristics that do not change over time, and the year of election FE λ_t control for temporal shocks that hit all the municipalities at the same time. The coefficient of interest in equation is 2 is β_1. Given the logarithmic form used for the dependent variable, we interpret the coefficients as the percentage change in electoral outcomes between the 2017-2012 presidential elections. Finally, we describe the main assumptions of the difference-in-differences model in the Appendix.

4.2 Instrumental Variable Approach

As explained in the Appendix, OLS and diff-in-diff models may underestimate the effect of CAOs. To circumvent these potential biases, we propose to instrument CAOs location with the presence at the municipal level of pre-existing (i.e., built before the dismantling of the Calais Jungle) buildings that can accomodate groups of individuals. We use two different types of buildings, which we combine in one instrument. First, we collect data on the number of holiday villages. Second, in line with the recent literature (Gamalerio et al. 2021; Steinmayr 2020), we collect data on the number of group accommodation buildings such as homes for elderly, disabled, drug addicts, and orphans. The idea behind the construction of the instrument is that the government considered several types of

venues, with a strong emphasis on buildings that could commodate groups of people. We start the IV analysis with the following first stage regression:

$$CAO_i = \gamma_0 + \gamma_1 GroupBuildings_i + \gamma_k X_{k,i} + \epsilon_i \tag{3}$$

where $GroupBuildings_i$ is equal to the sum of the number of group accommodation buildings plus holiday villages located in municipaity i. Then, we run the following second stage regression:

$$\Delta FN \equiv log(FN_{2017})_i - log(FN_{2012})_i = \beta_0 + \beta_1 \hat{CAO}_i + \beta_k X_{k,i} + \eta_i \tag{4}$$

Where $log(FN_{2017})_i - log(FN_{2012})_i$ is the difference of log voting shares for the FN between 2017 and 2012 elections; $\hat{CAO}_i$ is the predicted value of CAO_i obtained from the first stage regression, while $X_{k,i}$ are k control variables for municipality i, described in the Appendix. Specifically, we use all the socio-economic controls and their evolutions, log distance to the closest permanent migrant center, the evolution in the number of CADA places between 2012 and 2016, log hotel rooms, political and administrative characteristics, and mayors' demographics. The standard errors are clustered at the *département* level. Finally, this IV approach relies on two main assumptions, described in the Appendix.

5 Empirical Results

5.1 Baseline results

Table 1 reports the baseline results of our analysis. In Panel A, we report the results of the difference-in-differences analysis. Across the five columns, we add covariates, year of election FE, and municipal FE. The variable of interest is the interaction term between CAO and the dummy variable for the 2017 presidential elections. The coefficients are stable across the different specifications. They indicate that CAOs reduce by 4 percentage points the change of FN vote shares between the 2017 and 2012 elections. Considering

spillover effects in column 5, we can see that localities in a five or ten-km radius experienced a negative impact on the FN vote, but not as strong as the municipalities with a CAO.

Moving to the instrumental variables approach in Panel B, the reduced form coefficient in column 1 indicates that our instrument negatively correlates with the growth in FN votes shares between 2017 and 2012. Crucially, the same thing does not happen if we look at the change between the 2012 and 2007 elections, as shown in the pre-trends analysis in the Appendix. In column 2, we get a first-stage regression with F-statistics that are above the customary values indicated by the weak instrument guidelines given in Stock and Yogo (2005). Looking at the IV coefficients in columns 4-5, we get an even more negative and highly significant effect. As we previously discussed, not instrumenting the allocation of CAOs could bias our estimates towards zero. When we run the IV strategy controlling only for our proxy for overall tourism (i.e., the log of the number of rooms iin hotels and the dummy variable for municipalities on the coast), a CAO's presence decreases the growth rate of FN votes by 8.6 percentage points (Column 4). The coefficient does not change much when adding the other controls (Column 5). As we can see from Column 5, a CAO's presence decreases FN votes' growth rate by 12 percentage points. Since the FN vote increased by 20% on average in French municipalities between 2012 and 2017 (corresponding to a change of 5.1 points if we look at shares as outcome variables rather than logs), this estimation suggests that the growth rate of FN vote in municipalities with a CAO was 40% of the growth rate in municipalities without a CAO.

In Table 2, we investigate what impact the relocation of migrants had on votes for the extreme left-wing party *Front de Gauche* and electoral turnout. In columns 1-2, we provide the results of the diff-in-diff analysis. In columns 3-4, the results of the IV analysis. The results are very clear for what concerns the vote shares of the FG (columns 1 and 3).[8] Both diff-in-diff and IV estimates indicate a positive effect of CAOs on FG votes shares. Conversely, the results are contradictory for the electoral turnout, with the

[8]We do not carry out a separate analysis for electoral outcomes for the center-left and center-right parties because the candidacy of Emmanuel Macron, an ex-socialist minister and centrist, makes it difficult to compare those votes with the election in 2012.

Table 1: Main Results on the impact of migrants on the Front National Vote

	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences estimates					
Dependent variables	Log(FN)	Log(FN)	Log(FN)	Log(FN)	Log(FN)
Covariates	No	Yes	No	Yes	Yes
Time FE	No	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes	Yes
CAO x 2017	-0.042***	-0.043***	-0.036***	-0.037***	-0.041***
	(0.010)	(0.009)	(0.012)	(0.012)	(0.012)
CAO	-0.205***	-0.116***			
	(0.018)	(0.015)			
2017	0.208***	0.206***			
	(0.001)	(0.002)			
ring5_CAO x 2017					-0.016**
					(0.008)
ring10_CAO x 2017					-0.012**
					(0.006)
ring15_CAO x 2017					-0.006
					(0.004)
Observations	58,066	58,066	58,066	58,066	58,066
Panel B: IV estimates					
Dependent variables	Δ_{FN}	CAO	Δ_{FN}	Δ_{FN}	Δ_{FN}
Covariates	Yes	Yes	Yes	No	Yes
Regions FE	Yes	Yes	Yes	Yes	Yes
Model	Reduced Form	First Stage	OLS	IV	IV
Group buildings	-0.001***	0.005***			
	(0.000)	(0.001)			
CAO			0.001	-0.086***	-0.120***
			(0.008)	(0.031)	(0.039)
Coastal				-0.041***	-0.030***
				(0.013)	(0.011)
Log hotel rooms				-0.005***	-0.004***
				(0.001)	(0.001)
Observations	26,888	26,888	26,888	26,888	26,888
F-statistic	-	41.05	-	65.11	41.05

Difference-in-differences estimates in Panel A, Instrumental variables estimates in Panel B. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; 2017 = 1 for 2017 presidential election; the rings (ring5_CAO, ring10_CAO, ring15_CAO) in Panel A denote municipalities within the 5, 10 and 15 km radius respectively; Group buildings = number of village vacances and group accommodation buildings in the municipality; Coastal = 1 for municipalities on the coast; Log hotel rooms = log of the total number of hotels in the municipality. Control variables in Panel A: municipality sociodemographic characteristics, the mayor's party, and personal characteristics. Control variables in Panel B: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. In column 4 of Panel B, the only control variables in regressions are the coastal dummy variable, the log of the number of hotel rooms and regions FE. Standard errors clustered at the municipality level in parentheses in Panel A. Standard errors clustered at the *département* level in parentheses in Panel B. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

diff-in-diff analysis indicating a negative effect and the IV one a positive impact. Hence, while it is clear that the votes lost by the FN due to CAOs went to the major left-wing pro-immigrant party, it is not clear which exactly is the role of electoral turnout and voters' mobilization. However, in the Appendix, we provide evidence that rules out that our results are due to a mobilization story or changes in electoral turnout.

Table 2: Effect of migrant Relocation on Extreme-left wing votes and Turnout

	(1)	(2)	(3)	(4)
	Log(FG)	Log(Turnout)	Δ_{FG}	$\Delta_{Turnout}$
CAO x 2017	0.035***	-0.009***		
	(0.013)	(0.003)		
CAO			0.166***	0.026***
			(0.062)	(0.009)
Model	DiD	DiD	IV	IV
Covariates	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	No	No
Municipal FE	Yes	Yes	No	No
Regions FE	No	No	Yes	Yes
F-statistic	-	-	41.05	41.05
Observations	58,047	58,068	26,878	26,888

Difference-in-differences estimates in columns 1-2, Instrumental variables estimates in columns 3-4. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; 2017 = 1 for 2017 presidential election. Control variables in columns 1-2: municipality sociodemographic characteristics, the mayor's party, and personal characteristics. Control variables in columns 3-4: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Standard errors clustered at the municipality level in parentheses in columns 1-2. Standard errors clustered at the *département* level in parentheses in columns 3-4. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Heterogeneous Effects of Migrant Relocation

We conduct regressions showing heterogeneous effects in Table 3. We focus on the size of the immigration inflow generated by the opening of CAOs, for which we can find the descriptive statistics about the distribution in the bottom panel of Table 3. We run

the heterogeneity analysis for the diff-in-diff (columns 1-2), and the IV (columns 3-4) approaches. In the IV analysis, we instrument the interaction term with the interaction between the instrument and the inflow size. We measure the size of the inflow as the capacity of the CAO per 1,000 inhabitants. We standardize this variable so that it takes mean 0 and standard deviation 1.[9]

Interestingly, the FN's vote share's negative effect is reduced in places where more migrants were allocated. The analysis of the intensive margin yields important results for the understanding of electoral reaction to migrant inflows. We find that FN's negative effect is stronger in municipalities with fewer migrants per inhabitant hosted in the CAOs. Looking at the diff-in-diff results, we estimate that municipalities that decreased their FN vote upon receiving migrants were those that, on average, hosted less than 47 migrants per 1,000 inhabitants, which corresponds to 12 standard deviations in the distribution of CAOs capacity (a standard deviation being equal to 3.96). In the IV results, the estimated threshold is approximately 32 migrants per 1,000 inhabitants (i.e., 8 standard deviations). Above these thresholds, CAO's estimated average effect on FN vote switches sign and eventually becomes positive and statistically significant for very large CAOs. This result is in line with the literature on the impacts of immigrants' large inflows on political outcomes. This evidence indicates that, while small immigration inflows can reduce prejudice, inflows above a certain threshold can produce the opposite effect, suggesting a potential "Tipping point".

5.3 Contact theory vs. Realistic Group Conflict theories vs. Pure exposure effect

We think that the negative baseline effect in Table 1 is in line with contact theory (Allport 1954), which suggests that the contact between natives and immigrants should lead to a decrease in anti-immigrant attitudes when certain conditions apply. The anecdotal

[9]We could not find information on the capacity for a small number of municipalities with a CAO. These municipalities appear to be small towns for which it is more complicated to recover information. Hence, we replace the missing information with the smaller value taken by the distribution of the capacity of CAOs. If we repeat the analysis dropping these municipalities with missing information, the results do not change. Results can be made available upon request.

Table 3: Heterogeneous Effects of the impact of migrants on the Front National Vote

	(1)	(2)	(3)	(4)
	Log(FN)	Log(FN)	Δ_{FN}	Δ_{FN}
CAO x 2017	-0.037***	-0.057***		
	(0.012)	(0.012)		
CAO $\times \frac{CAO-migrants}{Population}$ x 2017		0.005*		
		(0.003)		
CAO			-0.120***	-0.113***
			(0.039)	(0.037)
CAO $\times \frac{CAO-migrants}{Population}$				0.015***
				(0.005)
Model	DiD	DiD	IV	IV
Covariates	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	No	No
Municipal FE	Yes	Yes	No	No
Regions FE	No	No	Yes	Yes
F-statistic	-	-	41.05	13.14
Observations	58,066	58,066	26,888	26,888
Distribution of the size of CAOs				
	Mean	Standard deviation	Min	Max
$\frac{CAO-migrants}{Population}$	0.18	3.96	0	253.81
$\frac{CAO-migrants}{Population}$ if $CAO = 1$	15.97	33.99	0.035	253.81

Difference-in-differences estimates in columns 1-2, Instrumental variables estimates in columns 3-4. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; 2017 = 1 for 2017 presidential election; $\frac{CAO-migrants}{Population}$ = number of migrants in CAO every 1000 inhabitants. We standardize the variable so that it takes mean 0 and standard deviation 1. Control variables in columns 1-2: municipality sociodemographic characteristics, the mayor's party, and personal characteristics. Control variables in columns 3-4: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Standard errors clustered at the municipality level in parentheses in columns 1-2. Standard errors clustered at the *département* level in parentheses in columns 3-4. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The bottom panel report the descriptive statistics on the distributiion of the size of CAOs.

evidence on the CAOs' experience seems to confirm this idea. When collecting our data on the location of the CAOs, we came across many examples that suggest that the interactions between migrants and local populations were generally successful. While at the onset of the dismantling process, protests seemed to be widespread (La Depeche 2016), and sometimes violent (La Croix 2016), several articles mention that local populations regret migrants have to leave after only a few months, even within municipalities where protests took place initially (Charente Libre 2018; Liberation 2017). Many forms of interactions emerged, through charity dinners (La Nouvelle Republique du Centre Ouest 2017), car-pooling (Liberation 2017), or football games. Officials of small municipalities argued that the arrival of migrants revitalized football teams in rural areas, which lacked players to compete in amateur leagues (20 Minutes 2016). Although we cannot systematically analyze those events, they do not seem rare and repeatedly appear in newspapers. A map released by Le Monde (2017b) shows that initiatives helping migrants being integrated were far from scarce.

Conversely, the results in Table 3 indicate that CAO centers' negative effect on FN votes shares can turn positive when the centers' size reaches a certain threshold. We believe that this evidence indicates that natives can perceive the inflow of new immigrants as a threat to their social, cultural, and economic hegemony when their number is too large. This evidence is consistent with the "realistic group conflict theories". For this event study, we think that the potential threat generated by large CAOs should be due to cultural rather than economic concerns. In Table 4, we use data from Trendeo - Observatoire de l'investissement et de l'emploi (2017), which reports the number of job creations and destructions at the municipality level from January 2009 to June 2017, to test the potential economic consequences. As shown in Table 4, we do not find any significant relationship between the presence of a CAO and net job creation (columns 1 and 3). Besides, controlling for net job creation per inhabitant does not affect our diff-in-diff and IV estimates (columns 2 and 4). Therefore it is more likely that when entering in contact with a large group of outsiders, natives perceived the opening of big CAOs as a threat to their identity and cultural dominance. In addition, an alternative

or perhaps coexisting explanation to the "realistic group conflict theories" story is the one based on the recent evidence on the pure effect of exposure without contact (Dinas et al. 2019; Hangartner et al. 2019). Specifically, big CAO centers may have made contact between natives and migrants more complicated, leaving the natives to be affected by a pure exposure effect, potentially leading to an increase in prejudice.

Table 4: Effect of Migrant Relocation on Net job creation

	(1)	(2)	(3)	(4)
	NJC	Log(FN)	NJC	Δ_{FN}
			$Post-10/2016$	
CAO x 2017	2.335	-0.037***		
	(2.523)	(0.012)		
NJC		0.000		
		(0.000)		
CAO			-2.112	-0.120***
			(1.779)	(0.039)
$NJC_{Post-10/2016}$				0.000
				(0.000)
Model	DiD	DiD	IV	IV
Covariates	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	No	No
Municipal FE	Yes	Yes	No	No
Regions FE	No	No	Yes	Yes
F-statistic	-	-	41.05	41.05
Observations	58,066	58,066	26,888	26,888

Difference-in-differences estimates in columns 1-2, Instrumental variables estimates in columns 3-4. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; 2017 = 1 for 2017 presidential election; NJC = net job creation rate per 1,000 inhabitants; $NJC_{Post-10/2016}$ = net job creation rate per 1,000 inhabitants after October 2016. Control variables in columns 1-2: municipality sociodemographic characteristics, the mayor's party, and personal characteristics. Control variables in columns 3-4: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Standard errors clustered at the municipality level in parentheses in columns 1-2. Standard errors clustered at the *département* level in parentheses in columns 3-4. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

We have tried to answer important questions regarding the electoral impact of migrants' relocation after the dismantling of the Calais "Jungle". We find a negative effect on the FN's vote shares, consistent with the contact hypothesis. We provide some anecdotal evidence that supports this claim. We also provide empirical evidence on the heterogeneity behind the baseline effect. We show that the effect can potentially turn positive for municipalities that received a larger number of migrants, which is consistent with "realistic group conflict theories". Given that CAO centers did not have any local economic impact, we think that large reception centers' positive effect on FN votes shares is likely due to cultural and identitarian rather than economic concerns. In conclusion, this paper gives some indication also on the allocation mechanisms of migrants. Small numbers seem to decrease prejudice against them. Overall, our results suggest that there is a difference in perceived immigration through the media compared to real immigration. The electoral reaction to actual migration seems to depend crucially on the size of the inflow.

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A1 Appendix: Data Description

Presidential election results for 2007, 2012, and 2017 at the municipality level come from the Ministry of Interior. We also use a dataset from Trendeo - Observatoire de l'investissement et de l'emploi (2017), which reports job destructions and creations at the municipal level in France between January 2009 and June 2017. This dataset provides a measure of local employment dynamics at the municipal level with high frequency. We use these variables as dependent variables in the empirical analysis.

We collected municipalities' characteristics from 2006, 2011, and 2013 French Censuses. We collected data on the total population, the share of individuals aged between 0 and 14 and over 60, and the share of individuals belonging to each of the eight official socio-professional categories (farmers, independent, white collars, intermediary professions, employees, blue collars, retired and inactive). Similarly, we consider the share of unemployment among the population aged between 15 and 64. Besides, we collected data on migrants' share of the total population, where migrants are defined as foreign-born individuals. We also collected the median disposable income by consumption unit (available only for municipalities of more than 50 inhabitants). To control for mayors' characteristics, we use the *Repertoire National des Elus*. This dataset provides information on the mayor's occupation, i.e., if she is a private employee or a civil servant, a teacher, a farmer, or an individual working in an industrial or liberal occupation. It also indicates the mayor's age, gender, and political orientation (e.g., whether the mayor is right-wing or not). We use these variables observed in the three years available in the data as time-varying covariates in the difference-in-differences analysis.[1]

We collected the number of hotel rooms in the municipality from INSEE. Data on municipalities located on the coast comes from the webpage Comersis. To control for the compliance of French mayors in implementing the CAOs, we use a list of mayors who declared to be willing to welcome migrants. This dataset, taken from the National French Television (France Télévision 2015), is neither official nor exhaustive but contains

[1]In practice, for every electoral year, we control for municipal and mayoral characteristics taken from the latest Census year that we were able to collect.

367 municipalities. From the *CIMADE*, we also collected information on the presence of other types of migrant centers, including CADA, HUDA, AT-SA, CPH, and PRAHDA. The data is most detailed for the CADA, for which we observe the number of places between 2012 and 2016. We computed the evolution of the number of places in CADAs at the municipality level with this information. Combining all this information with a GIS dataset of French municipalities (provided by the French National Geographic Institute (IGN)), we computed each municipality's distance to each of these centers.[2] We use the values of these variables in 2013 and, when available, their change between 2006 and 2013 as controls in our IV regressions to capture municipalities' current socio-economic and political conditions and their evolution after the 2008 financial crisis.

The location, number, and size of holiday villages are taken from the 2014 survey on tourism carried out by the French national statistical institute (INSEE). We collected data on group accommodation buildings such as homes for the elderly, disabled, drug addicts, and orphans from the National archive of health and social establishments (FINESS) for the year 2014. We use this information to build our instrument, which, importantly for the credibility of the instrument, is based on data observed before the dismantling of the Calais Jungle. Finally, we keep the municipalities without missing information in all these control variables in the empirical analysis.[3]

A2 Appendix: Main assumptions of Empirical Specifications

A2.1 Appendix: Difference-in-differences Approach

The main assumption of the difference-in-differences approach is that municipalities with and without a CAO should have been following the same electoral trends in the pre-

[2]Furthermore, we also computed the distance to the closest CAO for each municipality. We use this information in the difference-in-differences analysis to provide evidence about spillover effects in neighboring municipalities.

[3]The results do not qualitatively change if we keep these observations by replacing the missing values with the average value of the variable in the sample. Results can be made available upon request.

treatment period. We test this assumption in the Appendix A3, using data from the 2007 and 2012 presidential elections and the 2014 European and 2015 regional elections. Finally, an important factor behind the idea of this difference-in-differences model is that the *préfets* took the final decision about the location of CAO and not local politicians. Hence, it may be that the *préfets* decided the location of CAOs without being influenced by electoral trends. However, we cannot completely exclude the possibility that some mayors participated in the allocation process, generating doubts about the exogeneity of our treatment. For example, municipalities that volunteered to receive migrants and those with historically lower FN votes were more likely to host a CAO. We deal with this possibility and the fact that the assignment of the CAOs was not random by repeating the analysis using the instrumental variable approach described in the paper and in the next section.

A2.2 Appendix: Instrumental Variable Approach

OLS and diff-in-diff models may underestimate the effect of CAOs for two main reasons. First, as described in section 3.1 in the paper, we were not able to recover information on all the existing CAOs. This misinformation represents a measurement error that is likely to lead to an attenuation bias toward zero. Second, many municipal governments likely opposed the opening of CAOs for electoral reasons. For example, mayors from centrist and moderate parties may have done it to attract (or not lose) the votes of extreme and anti-immigrant voters. This potential movement of voters could lead to a negative correlation between CAOs and vote shares of mainstream parties. Thus, since we do not observe the bargaining process between municipalities and the government, simple OLS estimates may be biased towards zero. As described in the paper, to circumvent these potential biases, we propose to instrument CAOs location with the presence at the municipal level of pre-existing (i.e., built before the dismantling of the Calais Jungle) buildings that can accomodate groups of individuals.

The IV approach used in the paper relies on two main assumptions. The first assumption is that we need a first-stage regression in which $GroupBuildings_i$ correlates with the

presence of CAOs at the municipal level. In the empirical analysis, we formally test for this first assumption. The second assumption is the exclusion restriction one, which we can think of as being composed of two parts. The first part states that conditional on covariates, the instrument can be seen as good as randomly assigned (i.e., conditional on covariates, the instrument is independent of potential outcomes and treatment assignments). The second part requires that the instrument affects the outcome variable ΔFN only through the treatment CAO_i.[4] Relative to the first part, various concerns arise. For example, group accommodation buildings may have been built to accommodate the migrants redistributed after the dismantling of the Calais Jungle. For this reason, we measure the number of group accommodation buildings and holiday villages in 2014, before the beginning of the dismantling of the Calais Jungle. In addition, most of these buildings were built in the past and certainly not to host migrants.

An additional concern is that municipalities with group accommodation buildings and holiday villages may have different characteristics compared to municipalities without these buildings. For example, municipalities with group accommodation buildings may have a larger population or higher income per capita or may elect mayors with different political orientations. These differential characteristics may lead these municipalities to follow different electoral trends and experience a different probability of hosting migrants. For this reason, in the analysis, we control for a rich set of various municipal socio-economic and political attributes and trends that are likely to correlate with electoral trends and with the probability of hosting migrants. In addition, for what concerns holiday villages, we control for proxies for overall tourism (i.e., the log of the number of rooms in hotels and a dummy variable equal to 1 for municipalities on the coast), which is a factor that could correlate with electoral trends.

Relative to the second part, we show that our instrument correlates with the change in FN vote shares only between the 2017 and 2012 presidential elections, not between the 2012 and 2007 elections (see Table A1 in Appendix A3). This evidence suggests that

[4]For a discussion on how we can think of the exclusion restriction as being composed of two parts, see Angrist and Pischke 2009, chapter 4, page 117. Also, for a discussion of IV identification in constant-effect vs. heterogeneous-effect models and a detailed description of the independence and exclusion restriction assumptions, see Angrist and Pischke 2009, chapter 4, pages 150-153.

our instrument started to correlate with changes in electoral outcomes only when the buildings considered could be effectively be used to host migrants and not before that. In addition, we run a falsification test using Corsica's case: while this region has several holiday villages, it did not receive any CAOs. We do not find that municipalities in Corsica with a holiday village and group accommodation buildings had different voting trends for the FN between 2012 and 2017. Thus, while we cannot treat all this evidence as a formal test, these results suggest that the instrument started to correlate with the change in FN votes shares only during the dismantling of the Calais Jungle.

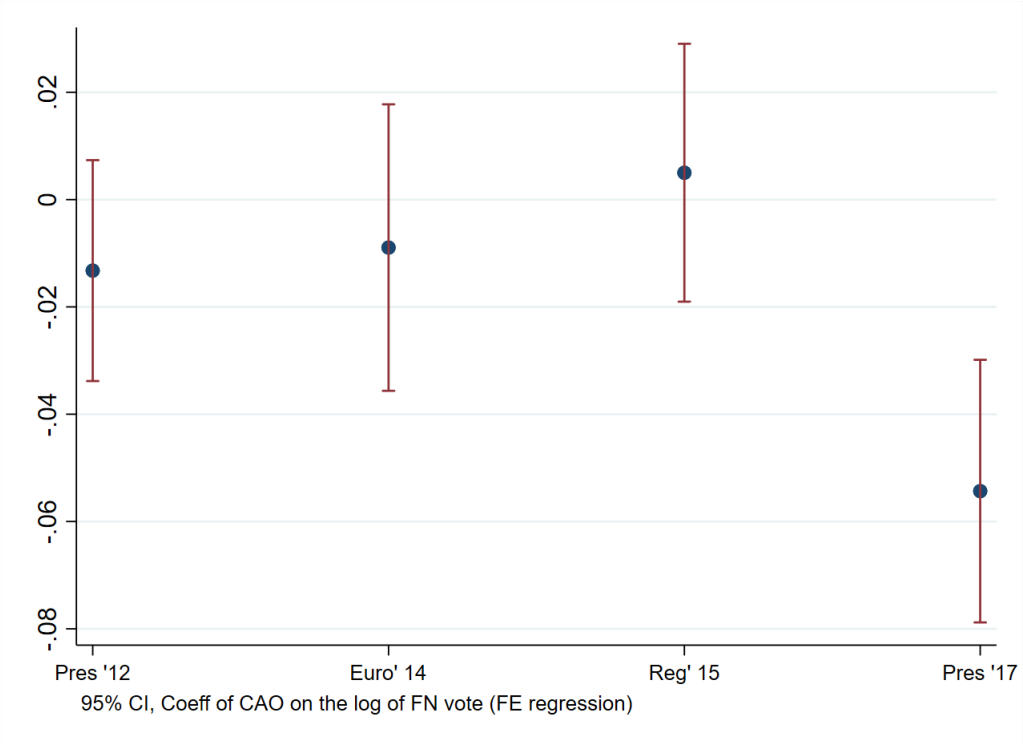
We argue that, since we control for a rich battery of municipal and mayoral characteristics, conditional on covariates, the instrument can be considered as good as randomly assigned. In addition, the evidence that the instrument correlates with the change in FN vote shares only between the 2017 and 2012 presidential elections suggests that it is plausible to think that group accommodation buildings and holiday villages affected the outcome variable ΔFN only through the treatment CAO_i. In conclusion, we think that the exclusion restriction assumption appears to be plausible in the context studied in this paper.

A3 Appendix: falsification and robustness checks

First, we consider whether we might be picking up pre-treatment electoral trends. We do this in two ways. First, we run a panel regression with municipal and year of election FE, where we evaluate the effect of CAO presence on various elections since 2012 (i.e., the Presidential elections of 2012, the European elections of 2014, the Regional elections of 2015, and the Presidential election of 2017). In Figure A1, where the effect of CAO in the Presidential elections of 2007 is normalized to be zero, the coefficient on CAO is never statistically different from zero except for the 2017 Presidential elections. This evidence shows that treated municipalities were not on different political trends before the election.

Second, in column 1 of Table A1, we run our diff-in-diff model using the 2007 and 2012 presidential elections, and we use the interaction between CAOs and the dummy for

Figure A1: Absence of Pretrends



the 2012 election as the main treatment. We do not find any effect. In column 2, we show that our instrument does not correlate (p-value equal to 0.950) with the log of the change in FN vote shares between the 2007 and 2012 elections. Finally, in column 3, we repeat the IV analysis using the log of the change in the FN vote shares as a dependent variable between the 2007 and 2012 elections. Also, in this case, we do not find any effect.

In Table A2, we consider Corsica, which represents an indirect test of our exclusion restriction. No migrants were relocated to Corsica, although it contains many holiday villages. Here, we regress our instrument on the change in voting outcomes for the FN in the French Presidential elections. Table A2 shows that no coefficient is significant. In addition, we report the p-value of the hypothesis test on the equality between the coefficient in front of the instrument in the reduced-form regression in the main sample (i.e., Table 1, Panel B, column 1) and the coefficient in Table A2, column 2. The p-value confirms that we reject the null hypothesis of equality between the two coefficients. Even tough this placebo test is run with a smaller number of obsevations compared to the main

Table A1: Pre-Trends: CAO Coefficients on Past Presidential Elections

	(1)	(2)	(3)
	Log(FG)	$\Delta FN_{2007-2012}$	$\Delta FN_{2007-2012}$
CAO x 2012	0.000		
	(0.013)		
Group buildings		0.000	
		(0.000)	
CAO			0.003
			(0.040)
Model	DiD	Reduced form	IV
Covariates	Yes	Yes	Yes
Time FE	Yes	No	No
Municipal FE	Yes	No	No
Regions FE	No	Yes	Yes
F-statistic	-	-	41.05
Observations	56,195	26,884	26,884

Difference-in-differences estimates in column 1, Reduced form model in column 2, Instrumental variables estimates in columns 3. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; 2012 = 1 for 2012 presidential election; Group buildings = number of village vacances and group accommodation buildings in the municipality. Control variables in column 1: municipality sociodemographic characteristics, the mayor's party, and personal characteristics. Control variables in columns 2-3: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Standard errors clustered at the municipality level in parentheses in column 1. Standard errors clustered at the *département* level in parentheses in columns 2-3. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

sample (i.e., 200 vs. 26,888 observations), we think that these additional results underline the validity of our IV approach.

Table A2: No Link between Holiday Villages and FN trend in Corsica

	(1) Δ_{FN}	(2) Δ_{FN}
Group buildings	0.000	0.002
	(0.000)	(0.002)
P-value difference		0.026
Regression	OLS	OLS
Controls	No	Yes
Observations	352	200

Columns 1 to 2 report the results of OLS regressions of the variation of log FN votes between the presidential elections of 2012 and 2017 on the dummy for a holiday village and group accomodation buildings. Control variables in columns 2-3: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms. Standard errors clustered at the *département* level in parentheses.The p-value reported in the Table is the p-value of the hypothesis test of the equality between the coefficent in front of Group buildings in Table 1, Panel B, column 1 and the coefficent in this Table, column 2. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table A3, we rule out the possibility that changes in electoral turnout explain the effect of CAOs on FN and FG vote shares. Specifically, we split the sample between municipalities that experienced a negative change in electoral turnout and those that experienced a positive change. As we can see, the coefficients for both FN and FG vote shares are similar in magnitude for the two groups of municipalities. Besides, the coefficients are statistically different from zero only for municipalities that experienced a drop in electoral turnout. This evidence suggests that changes in electoral turnout and voters mobilization do not appear to be the main driver of our results.

Table A3: The role of Turnout and mobilization

	(1)	(2)	(3)	(4)
	Δ_{FN}	Δ_{FN}	Δ_{FG}	Δ_{FG}
Sample	$\Delta_{Turnout} < 0$	$\Delta_{Turnout} > 0$	$\Delta_{Turnout} < 0$	$\Delta_{Turnout} > 0$
CAO	-0.126***	-0.112	0.159***	0.167
	(0.038)	(0.085)	(0.060)	(0.109)
Observations	19,366	7,522	19,359	7,519
F-statistic	48.92	6.249	48.91	6.249
Model	IV	IV	IV	IV
Covariates	Yes	Yes	Yes	Yes
Regions FE	Yes	Yes	Yes	Yes

Instrumental variables estimates in all columns. Variables reported in the Table: CAO = 1 for a migrant center in the municipality. Control variables in all columns: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Standard errors clustered at the *département* level in parentheses in all columns. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table A4, we check that few outliers do not drive the first-stage relationship. As reported in the bottom panel of Table A4, the instrument takes values that go from 0 up to 274. Hence, one potential concern is that a few municipalities with big values of the instrument drive the first-stage relationship. To rule out this possibility, following the example of Steinmayr (2020), we rerun the first-stage regression dropping respectively the municipalities/observations with the 5, 10, and 30 biggest values of the instrument (i.e., municipalities with respectively more than 191, 132, and 85 group accommodation buildings). As we can see from columns 1-3 of Table A4, dropping these observations, we get first-stage regressions with even bigger F-statistics. In addition, in column 4 of Table A4, we have rerun the first stage regression using the instrument winsorized at the 99th percentile (i.e., considering the sample of municipalities with at least one group accommodation building, the 99th percentile corresponds to 43 group accommodation buildings). We get a first-stage relationship with a bigger F-statistic even in this case. This evidence suggests that few outliers do not drive the first-stage relationship.

Finally, in Figure A2, we use the methodology developed by Hainmueller et al. (2019)

Table A4: The role of outliers in the first-stage regression

	(1)	(2)	(3)	(4)
Dependent variable	CAO	CAO	CAO	CAO
Covariates	Yes	Yes	Yes	Yes
Regions FE	Yes	Yes	Yes	Yes
Sample	Drop 5	Drop 10	Drop 30	Winsorize
Group buildings	0.006***	0.007***	0.007***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	26,883	26,877	26,858	26,888
F-statistic	86.25	85.30	42.15	56.56
Distribution of Group buildings				
	Mean	Standard deviation	Min	Max
Group buildings	1.16	5.96	0	274
Group buildings if Group buildings > 0	4.09	10.65	1	274

First-stage regressions in all columns. Variables reported in the Table: CAO = 1 for a migrant center in the municipality; Group buildings = number of village vacances and group accommodation buildings in the municipality. Control variables in all columns: municipality sociodemographic characteristics (in 2013 and evolution between 2006 and 2013), coastal dummy variable, the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and personal characteristics. Samples used: 1) Drop 5 = we drop the 5 municipalities/observations with the highest values in Group buildings; 2) Drop 10 = we drop the 10 municipalities/observations with the highest values in Group buildings; 3) Drop 30 = we drop the 30 municipalities/observations with the highest values in Group buildings; 4) Winsorize = we winsorize Group buildings at the 99th percentile. Standard errors clustered at the *département* level in parentheses in all columns. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

to test for the plausibility of a linear interaction effect in the heterogeneity analysis described in Table 3. Figure A2 shows the relationship between the marginal effect of CAOs on FN vote shares and the moderator, which is the number of migrants in CAOs every 1000 inhabitants, standardized as to take mean 0 and standard deviation 1. We implement this analysis applying the Stata command interflex to model 1. This test compares the conditional effect estimates from a binning estimator with those from a standard multiplicative interaction model. We use different cutoffs to split the sample of municipalities with a CAO in the three bins required by the binning estimator. First, in the graph on the right, we split the sample between municipalities with a size of the inflow below 7 (i.e., 27 migrants per 1000 inhabitants) standard deviations, those between 7 and 12 (i.e., 47 migrants per 1000 inhabitants) standard deviations, and those above 12 standard deviations. We pick these two thresholds because they represent the cutoffs at which, according to the estimates in Table 3, the effect of CAOs on FN votes becomes first insignificant and then switches sign. Second, in the left graph, we use as thresholds 12 and 17 (i.e., 68 migrants per 1000 inhabitants) standard deviations. These thresholds represent the 90th and the 95th percentile of the distribution of the inflow size, respectively, if we consider only municipalities with a CAO. We use these thresholds so that to split in samples of a similar size those municipalities above the threshold at which, according to our estimates, the effect of CAOs on FN votes switches sign.[5] As we can see, the estimates from the binning estimator appear to sit on the estimated linear marginal effect line in both graphs. This evidence reinforces the plausibility of a linear interaction effect.

[5]We have also tried to use other combinations of thresholds, and we get similar results. For example, we have tried with 7 and 17 standard deviations, 7 and 59 (i.e., the threshold at which the effect becomes positive and statistically significant), and 12 and 59 standard deviations. Results can be made available upon request.

Figure A2: Robustness check interaction model