Does Community Social Capital Affect Refugees Integration?

Vera Rocha[1], Paolo Santini[2]

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Abstract

The integration of marginalized groups into the labour market is a significant policy objective in all developed countries. Refugees, with their forced migration stories, are among the most vulnerable groups in the population, and their integration has proven particularly challenging. In this paper, we study the role that local social conditions have on the integration of refugees in Denmark. Leveraging the spatial dispersal policy in place in the country between 1999 and 2016, we find a positive impact of the presence of social groups on the educational attainment of refugees. We do not find, however, any clear effect on their labour market integration, except for a reduction in self-employment in the short run. We conclude that local social capital might be beneficial for the cultural and social integration of refugees, but less so for their economic one.

[1] Department of Strategy and Innovation, Copenhagen Business School. Email: vr.si@cbs.dk.
[2] Department of Strategy and Innovation, Copenhagen Business School. Email: pasa.si@cbs.dk. We acknowledge the financial support of the Carlsberg Foundation via the grant CF21-0156.

1. Introduction

Wars, climate change, and civil rights violations have greatly increased the number of refugees in recent years. Worldwide, their number has more than doubled since 2013, passing from around 15 to almost 40 million individuals (World Development Report, 2023). Similarly, Europe has passed from 1 to 2.5 million refugees in the short time window 2014-2018 (Frattini et al., 2022). Given the scale of the phenomenon, and its likely aggravation in the future, governments are under pressure to implement effective integration policies to fulfill their protection duties towards refugees and maintain a peaceful society. This is however a challenging task as refugees are in several ways a disadvantaged group even compared to other migrants: they have suffered forced displacement and waited long time before being granted the asylum protection; they are typically less positively selected on skills than other migrants; they come, for the most part, from countries with very different cultural norms than the ones of the host country. Indeed, even after several years of residence, their labor market attachment is particularly low and lower than that of any other group (Brell et al., 2020; Fasani et al., 2021).

To address these issues, a large literature in social science has emerged trying to underpin the factors that could hamper or foster refugees' integration, with a special focus on the labor market. One obvious candidate is the characteristics of the area in which refugees settle upon arrival, but few of these characteristics have made the object of careful study. An overlooked factor is the social capital, i.e. the web of relationship and connections within a community, that is met by refugees upon arrival. Social capital has been found to have a positive impact on many desirable outcomes in different aspects of human life. For instance, it has been shown to increase political accountability (Nannicini et al., 2013) and reduce property crimes (Buonanno et al., 2009). It has also been shown to increase the capacity of communities to deal with adverse shocks such as COVID-19 (Makridis & Wu, 2021). More broadly, it is found to be positively correlated with innovation and growth (Akçomak & ter Weel, 2009) and to be a driver of income mobility for underprivileged groups (Chetty et al., 2022). Concerning refugees, places with more social capital could be favoring their integration as engaged citizens might be more sensible to refugees' needs and more able to help them through their larger networks. Some researchers, however, have emphasized that social capital could also have some detrimental consequences (Portes, 2014). A tight net of connections between members of a community and a closed group participation could in fact create a barrier for outsiders to retrieve information and catch opportunities (Portes & Sensenbrenner, 1993; Waldinger, 1995). Indeed, social groups are often participated only by the local population and might thus become an additional source of exclusion for refugees both from a social and an economic point of view. In this paper, we provide evidence regarding the impact of a widely investigated form of social capital, civic engagement, on the socio-economic integration of refugees in Denmark. Following the literature (see Engbers et al., 2017 for a review), we proxy the level of civic engagement in a locality as the number of non-profit organizations active in that location at the time of the refugees' settlement.

One key challenge that we need to overcome to study the impact of local conditions on refugees' outcomes is the potential endogenous sorting of refugees across locations (Damm, 2009a; Edin et al., 2003). If more skilled refugees self-select in locations that better promotes integration, estimating the effect of location characteristics on refugees' labor market status will give a biased estimate of their true effect. To address sorting, we exploit the spatial dispersal policy in place in the country between 1999 and 2016 (Azlor et al., 2020). Over this period, newly arrived refugees were assigned across municipalities according to a quota system aimed at evenly distributing non-western migrants across all the Danish territory. Refugees could ask to be assigned in specific locations, but only exceptionally they could get their preferred choice if the demanded place had filled its annual quota. To alleviate concerns on endogeneity and sorting, we restrict our analysis to the labor market integration of those refugees that were granted asylum when a substantial share of municipalities had met their annual quota (Azlor et al., 2020). Another feature of the policy introduced in 1999 strengthen our strategy: by making economic transfers conditional on remaining in the assigned municipality, very few refugees moved while still receiving the transfers (i.e. during the first 3 years) thus complying with the assignment rule. An additional advantage of our setting is that Danish registry data identifies refugees without measurement error as of 1997, increasing precision in the estimation.

We find that an increase in the number of social organizations does not affect employment nor earnings of refugees in the first 4 years upon arrival, while it reduces self-employment. It does increase though the number of individuals enrolled in education: a 10% increase in the number of social organizations leads to an increase of being in education of 0.11 or 57% of a standard deviation. Overall, then, it seems that communities with a higher social capital are able to redirect refugees away from self-employment towards education, possibly hoping to upgrade their labor market prospects. An analysis of long-term outcomes, however, does not reveal any significant gain from this choice. The rest of the paper is organized as follows: section 2 presents the literature on the effects of local conditions on refugees' integration. Section 3 introduces the institutional context and the empirical challenges; section 4 describes the data; section 5 presents the empirical strategy. In section 6 we present and discuss our results. Finally, section 7 concludes.

2. Literature Review

Beyond social capital, other local conditions have been shown to matter for the integration process of refugees. These are essentially two: the share of co-ethnics in the area and the tightness of the local labor market. Concerning the first one, early studies found that being placed in a community with a high share of co-ethnic or other non-western immigrants increased the employment probability and the earnings of the refugees settling there (Damm, 2009b; Edin et al., 2003; Martén et al., 2019). More recent studies, focusing on longer-run outcomes, however, have challenged this view finding roughly zero impact of ethnic enclaves on employment or earnings (Battisti et al., 2022; Foged, Hasager, & Peri, 2022). One possible explanation for this divergence is that ethnic enclaves slow down the acquisition of the host country language. Indeed, Damm et al., 2022 show for Denmark that a higher share of co-

ethnic reduces the host country formal language training. Since language proficiency has been found to increase the labor market prospects of all foreigners, especially in the long run (Foged, Hasager, Peri, et al., 2022; Foged & van der Werf, 2023; Lochmann et al., 2019), the initial positive effect of ethnic enclaves seems to be balanced out by a negative one later. Another strand of papers has investigated the impact of a more or less thriving local labor market. Damm, 2014 studied the impact of being placed in a more deprived neighborhood finding no statistically significant effect on employment or earnings. On the contrary, higher unemployment rates, especially among individuals belonging to similar groups of the incoming refugees, have been shown to be detrimental for the labor market outcomes of refugees (Aksoy et al., 2023; Åslund & Rooth, 2007; Azlor et al., 2020; Damm & Rosholm, 2010). Our paper is mostly related to an emerging literature that investigate the effects that the local populations' attitudes towards refugees have on their social and economic integration. In this case, two forces might be playing simultaneously: on the one hand, a more negative attitude should enhance the occurrences of discrimination and thus lower employment; on the other hand, a fear of retaliation might push refugees to find a job and conform to local norms faster. Perhaps for this reason, different studies reach different conclusions: some studies show that a stronger anti-migrant sentiment has a null or negative labor market impact on refugees (Aksoy et al., 2023; Jaschke et al., 2022) but others have found a positive effect instead (Müller et al., 2023). Similarly, a negative attitude towards refugees fosters cultural integration in Germany according to Jaschke et al., 2022 while it reduces it in Aksoy et al., 2023. One possible confounding factor thus far overlooked is the presence of non-profit organizations: using a regression discontinuity design, Pulejo, 2023 has shown that, in Italy, when a right-wing major is elected, the adverse part increases its civic engagement in NGOs for helping migrants. By analyzing the direct impact that non-profit organizations have on the integration of refugees in Denmark our paper aims to shed more light on the non-economic conditions that might affect refugees' integration both culturally and economically completing the above studies on attitudes.

3. Institutional context & empirical challenge

People seeking asylum in Denmark need to register to the police authority upon arrival and be interviewed by the Danish Immigration Service (DIS) (Azlor et al., 2020; Hvidtfeldt et al., 2018). If deemed worthy of protection, DIS grants the applicant the refugee's status and proceed with its allocation to an available accommodation throughout Denmark. Since the mid-80s, the Danish government feared the creation of too large concentrations of non-western migrants in specific areas resulting in the possible formation of ghettos. For this reason, Denmark was among the first countries to introduce a spatial dispersal policy for refugees. The goal of the Danish dispersal policy is to scatter refugees all over the country such that in each location the proportion of non-western migrants to the Danes population is roughly similar. In practice, DIS forecasts the number of refugees that are supposed to enter in the country the next year[3] and decides how they should be redistributed across

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[3] Until 2005 it did forecast for 3 years but having to continuously update the forecasts they moved to a year-to-year prediction. Since the forecast can still be wrong, it can be reviewed during the year if the arrival of refugees was too largely

municipalities[4] in agreement with the local authorities. When the refugees start to arrive, DIS refers every refugee to a municipality that has not yet met its predetermined annual quota. In this paper, we leverage the version of the dispersal policy that took place from 1999 to 2016. The main change with respect to the pre-1999 version of the policy is that refugees are asked during the interview with a DIS fonctionnaire their preferred location. Based on interviews with DIS, Azlor et al., 2020 mention pursuing education, health reasons, or closeness to relatives[5] as possible reasons for wishing to be assigned to a specific municipality. DIS tries to accommodate these requests, but only in exceptional cases a refugee can be assigned to a municipality with a filled quota. This means that refugees being granted asylum at the beginning of the year will have a wider choice[6] than those being granted asylum in the middle or at the end of the same calendar year. Crucially, the date of application does not predict the date at which the individual is granted asylum, so that refugees can hardly strategize their application timing to reach a preferred location. This is so because of a series of administrative delays that are outside refugees' control: first, depending on the capacity of DIS at the time of the refugee application, the time between the registration day and the first interview may vary. Second, the time for the verdict is very heterogeneous. Hvidtfeldt et al., 2018 calculate that, over the period 1997–2011, the application process took on average 15 months but with large differences between cases. In particular, they provide evidence that applications from the same sending country are treated in butches so that refugees arrived at very different moments can receive the asylum status in the same day, creating substantial differences in waiting time. Until DIS has a verdict on the asylum application, asylum seekers are assigned to asylum centers. Up to 2013, refugees were not allowed to work while in these centers and lived relatively secluded from the rest of the Danish society (Dustmann et al., 2017). After 2013, they have been granted permission to work but bureaucratic obstacles, as well as the remoteness of most of these centers, make it very hard for them to work.

Once they are granted asylum, refugees are assigned to a municipality, and they officially enter the country. We observe them in our data from the year they settle in this first municipality of residence. As part of the 1999 reform, refugees receive means-tested subsidies[7] for 36 months and are offered a

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over- or under- estimated. While a revision of the initial quotas has occurred in more than one year of our analysis, we restrain ourselves to the prediction that was valid up to the 31st of December of the previous year.

[4] The decision is based on the share of population of each commune and its share of non-western migrants in the population. The formula used did not change between 1999 and 2016. A more careful description of the formula to compute municipal refugees' quotas is given in Azlor et al., 2020, Appendix A.

[5] Here relatives are to be understood as relatives that are not the close family, who is always located in the same municipality in which the first member arrived settled.

[6] This does not mean they can freely choose where to go as many municipalities are assigned a zero quota. This is typically the case of large cities, such as Copenhagen, where many non-western migrants already live. Again, only in exceptional cases refugees are allowed to move in a municipality with a zero quota.

[7] The amount of these subsidies has substantially varied during the period of analysis. The generosity of the subsidy has been shown to have an impact on the probability of (male) refugees finding a job. In this study we abstract from this channel by introducing year of migration fixed effects and thus de facto comparing individuals with identical level of subsidies.

language course of the same length[8]. These subsidies are conditioned on staying in the municipality of assignment unless a new municipality agrees in taking charge of the refugee. In practice, few demands are made and almost none accepted (Nielsen & Jensen, 2006). Importantly, the reform was introduced to curb mobility to guarantee equal distribution beyond the first year of assignment. Nielsen and Jensen, 2006 show that in this sense it was a success: between 1999 and 2005 very few refugees moved compared to the period 1997-1999 resulting in a much more homogenous distribution across municipalities. Figure 1 plots the annual share of movers (plain line) and the cumulative share of stayers (dotted line) for refugees entering in the 2001–2011-time window. As for the policy requirement, very few refugees moved out from the municipality of first assignment within the first 3 years. There is a spike in years 4 and 5, but it is relatively short lived. In general, mobility is quite low: up to 7 years after entry, around 60% of refugees still reside in their first municipality of assignment.

Figure 1: Refugees annual moving rate and cumulative share of stayers
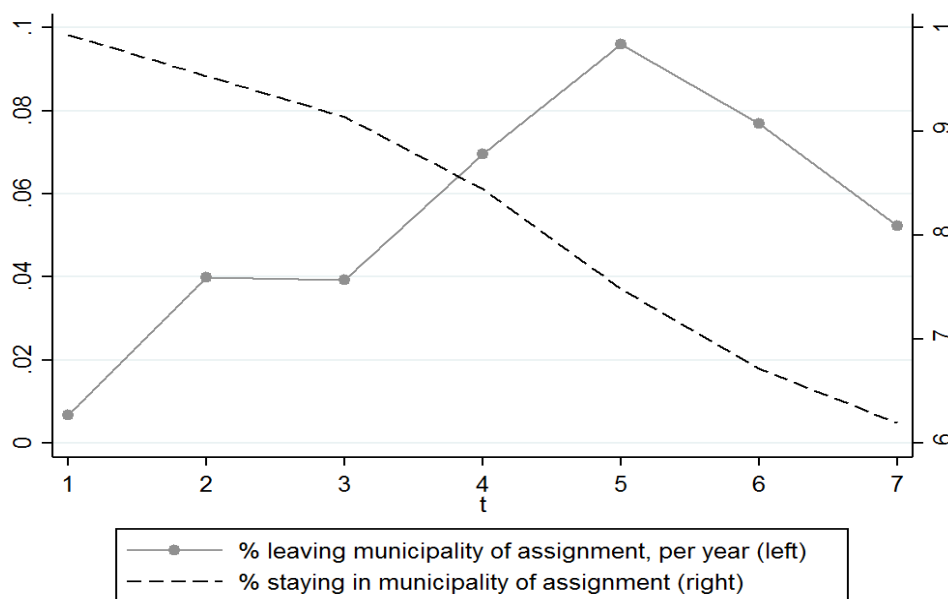
A necessary condition to analyze the effect of local conditions on refugees' integration is that refugees are sorted randomly across municipalities. If randomness is guaranteed, in expectations, similar individuals will be allocated to places with different conditions. Estimating the impact of these conditions on their subsequent integration would thus truly recover their causal impact on the variable of interest. Given the system with which refugees are allocated to municipalities through the period of our analysis though, a primarily concern is that the allocation is not random. Since refugees can express a wish on where they would like to go, it is possible that they would endogenously sort towards the

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[8] Instead of 18 months, as per the pre-reform.

places in which they would be more likely to succeed. In particular, we do worry about the possibility that more able individuals would be also better at choosing the best destinations (for instance, the one with a more thriving labour market), and thus the estimates of the local level of employment would be upwardly biased by the ability sorting occurring in the first settlement. Previous research has shown that, indeed, some endogenous sorting does occur. For instance, Edin et al., 2003 and Damm, 2009a have shown that negatively selected individuals move more frequently to ethnic enclaves and that neglecting it results in substantial negative bias of the effect of ethnic enclaves on refugees' integration. To alleviate these concerns, we follow the method firstly proposed by Azlor et al., 2020. This method consists in leveraging the time dimension of refugees' asylum permission. As aforementioned, individuals being granted asylum at the beginning of the year will have a wider choice of locations as, excluding those with a zero quota, all other municipalities will likely still have available slots. As the months pass, however, municipality quotas get filled and newly arrived refugees can only choose among the remaining municipalities. By restricting the sample to refugees granted asylum late enough in the year, we should thus drastically reduce the possibility of endogenous sorting. Importantly, this assumption can be partially tested by regressing the education level of refugees on several locations' characteristics. When the level of education is not correlated with any of the location characteristics, it means that sorting based on education is close to zero, and hence negligible. As long as education is highly correlated with unobserved ability, this test provides good evidence of absence of sorting by ability. In practice, this amounts to select a date that gives the largest estimation sample possible and that at the same time satisfies the above condition, and discard all refugees arrived earlier. In the next section, we provide this evidence while discussing sample selection. We deal with the second issue, namely selection in subsequent move, by focusing on a relative short time window (4 years after arrival) and studying the characteristics of initial placement. Note, however, that the conditionality of the means-tested transfers, and the low-mobility that it induced, already largely insure us against this threat to identification.

4. **Data and Sample**

To carry on the analysis, we exploit the rich Danish registry data for the years between 2001 and 2011. We combine detailed information at the individual level with local economic and social indicators built from population datasets. The backbone of our study is constituted by the OPIGHIN registry data. This registry records the flow of migrants that enters the country in each year, the precise date at which they are granted entry permission and classify the legal status of the permission given according to Danish law. One big advantage to use this dataset is thus that we measure the refugee status as well as the date of entry without any measurement error contrary to studies employing data before 1997. We combine this information with several demographic characteristics, such as gender, age, marital status, and, most importantly, residence location, obtained by the population registry BEF. We consider the municipality of residence observed in the year of migration or the subsequent year in BEF as the

municipality of assignment[9]. Finally, we add information on education (UDDA registry) and labour market attachment (IDAP registry). To build measures of local labor market composition we merge the FIRM data, containing the universe of registered firms in the country, with IDAS, containing information on the establishments with employed individuals. We compute our main independent variables regarding the local labor market composition using firms' characteristics (such as size, age, or sector) but averaging them at the local level using the establishments' locations. The unit of aggregation for the local characteristics is the commune. Denmark went through an important administrative reform starting from 2003 and culminated in 2007 with the reduction of the number of municipalities from 275 to 98 (LGDK, 2008). For consistency, we decided to conduct the analysis at the level of the new municipalities. In the same setting, Azlor et al., 2020 made the same choice. All variables, including refugees' quotas, are hence computed at the new municipality level. Except for 13 municipalities, all pre-reform municipalities translate one-to-one to new municipalities. In those cases where a municipality is split into 2 (in one case 3), we assign the full pre-reform population to the post-reform municipality to which most of the pre-reform population belong post-reform[10].

To conduct the analysis, we construct two samples of refugees. The main sample is composed by the refugees that enters Denmark over the period 2001 – 2011. To study how quotas got filled during the year we keep only one member per household using the number of individuals in the household to account for the total number of people settling in the commune. Figure A1, in Appendix A displays the share of municipalities with full quotas by May and June in all years of our study. On average, 17% (22%) of municipalities were completely full in May (June) respectively. To carry on the analysis, we further restrict our sample according to several criteria. First, we only focus on adults aged 18-59 who entered the country for the first time. Second, we identify household heads (first person of a nuclear family to reach the country) and keep only them and their spouses if they reached the country at the same time. We make this choice because according to Danish immigration laws close families are never separated. Thus, if a member of the family was already in the country, the incoming members are assigned to the same location as the present member even if the location's quota is full or if the first resident has moved out of his originally assigned location. Finally, we keep a balanced sample of individuals that remains in the country for at least 4 years after arrival, and we mainly restrict our analysis time on short-run outcomes, i.e., within the first 4 years of settlement. This first sample is composed by 10 452 refugees.

As mentioned in the previous section, if individuals were completely randomly distributed across municipalities upon entry, there would be no correlation between their characteristics and those of the municipality of assignment. The main concern regards the possibility that more able individuals would

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[9] This should not be a major cause of measurement error given the low initial mobility of refugees documented in Figure 1. Azlor et al. 2020, using quarterly data, documents that mobility is as low as 1.7% in the first three months and 3.9% after 9 months over the period 2008-2010.

[10] This choice is expected to have little influence as only 2% of the total population and 3% of refugees lives in those municipalities (Azlor et al., 2020).

instead sort in location with most favorable conditions. While we cannot test this directly, we can test if educational attainment is correlated with certain municipalities' characteristics of interest and a set of other characteristics shown to matter for employment by previous literature. The test should be a good proxy for sorting if educational attainment is highly correlated with ability, controlling for individual demographic characteristics such as age, indicators for being male, marital status, having under-aged children, year and month of asylum, and, most importantly, country of origin. We present the result from OLS regressions when we regress three educational categories (0-9 years (reference); 10-12 year; >12 years) on our measures of social organizations in Table A1 and on other important local characteristics in Table A2. Results are displayed in Appendix A. Each panel within the tables represents a specific set of refugees that have reached after a certain number of months since the beginning o the year. In the first panel, we report the results for the total sub-sample with all refugees included. We then subtract arrivals for the first months one by one until June. More educated individuals seem to sort in or out municipalities depending on some of the characteristics tested when we look at the whole sample. For instance, they are significantly more likely to be reaching places with a higher share of non-western migrants, and less likely to reach places with a higher share of social organizations. As we exclude the months, however, these effects tend to vanish. By June, none is significant anymore and point estimates are smaller. Our second sample, the one that will be used in the main analysis, is thus constituted by the refugees that have being granted asylum after May in each year. This comprises 6906 individuals that match our sample restriction criteria reaching 97 different municipalities[11]. In appendix A, Tables A3-A6, we present descriptive statistics on the full sample and the main sample for the analysis for refugees' and municipalities' characteristics. In terms of refugees' characteristics, we observe that they are more often men, low educated, and they come primarily from places experiencing conflict in those years. In some years/months, notably 2001, there are more new refugees than in others. A reasonable share does arrive though in all years and in all months. If we compare the characteristics of refugees settled before and after June, we find that refugees settling later are younger, less married and, importantly, less educated. Turning to locations, refugees are, on average, placed in communes with 4% of other non-western immigrants and a 3.8% unemployment rate. Interestingly, we do see differences in the characteristics of the places by early and late incomers that are likely revealing of what refugees do praise or dislike. Refugees with fewer choices in fact settle in smaller places with fewer immigrants from non-western countries and substantially less co-nationals, and with a small, but significant, higher share of votes for anti-immigrants' parties. Surprisingly, unemployment rates are the same between the two groups, perhaps suggesting that social conditions matter more for the well-being of refugees than economic ones. Finally, advocacy groups' presence is smaller in the municipalities of settlement of refugees reaching after June.

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[11] To avoid having our estimates biased by Copenhagen we exclude any individual that appears there as, over the whole period of analysis, Copenhagen had null quota of refugees.

5. Empirical Strategy

As discussed in the previous section extensively, we address the potential endogenous placement of refugees by focusing only on refugees that are granted asylum after May and that hence have a less wide option of municipalities among which they can choose. Using this sub-sample, we estimate the effect of the social engagement in a municipality on the economic and social integration of refugees. We measure the local social engagement as the number of organizations in specific sectors such as advocacy groups (parties, unions, churches, others), recreational groups (sports clubs, cultural associations or libraries), and other social associations (foundations, NGOs, asylum centers, etc.). We first estimate the cumulative effect of the three and then disentangle the effect of each separately. Alternative we measure the social capital as the whole non-profit sector excluding utilities and rent owners associations. The main specification investigates the effect of these characteristics, measured at the time of entry, on the economic and cultural integration of migrants over the first 4 years in the country. This means that our parameter of interest should be interpreted as an intention to threat (ITT). The advantage of using this model is that ensures that subsequent selection in movers does not affect the results. We thus estimate through OLS:

$$Y_{ijcmts} = \alpha_1 \, LSE_{j^*t=t_0} + \beta_1 X_{it=t_0} + \boldsymbol{\gamma}\boldsymbol{v}_{j^*t=t_0} + \delta_c + \delta_t + \delta_m + \delta_s + \epsilon_{ijcmts} \quad (1)$$

Where the parameter of interest $\hat{\alpha}_1$ is a consistent and unbiased estimate of the local social engagement at arrival on the labour market or cultural integration of refugees in the first 4 years in the country. In addition to the random allocation hypothesis, no omitted correlated need to affect the results directly. This is true if initial local conditions do not vary too much over the short time period of analysis. Since DIS knows some of the refugee characteristics and might use them to determine the refugee allocation, we control for age and its squared, education upon arrival, being married and having under-aged kids in the vector $X_{it=0}$. Additionally, previous research has shown how other local conditions, namely local labour market status and population composition, might matter for integration, especially in the first years. We thus include the population share, the share of non-western immigrants and the unemployment rate in the vector $v_{j^*t=0}$. In all specification we also always control for the normalized vote share of far-right parties in the last election. We normalize the vote share to make comparison across elections meaningful. The vote share for far-right parties was however quite constant over this period of time in Denmark. Results are presented also for this regressor separately as it might in itself be relevant for integration. Finally, we control for a series of fixed effects that should ensure comparability among different refugees. These are the country-of-origin δ_c, the month of permit δ_m, and the year of permit δ_t fixed effects. Since we pool all first years together, we also control for δ_s that is the time since arrival. Results are similar if we run the same model year-by-year without including this control. Since the treatment varies at the municipality level, we cluster the standard errors at this level.

To quantify the potential bias from selection, we re-estimate the same model as in equation 1 for the full sample.

Since the most recent papers in the literature has underlined the potential reversal of certain effects in the long run (Battisti et al., 2022; Foged, Hasager, & Peri, 2022), we also look at longer time window. We run a similar model as above but groups together outcomes from periods 5 to 7, instead of 1 to 4.

6. Results

6.1 Short term effects:

In this section, we present our results using employment and employment type in November over the first 4 years of residence in the country as dependent variables. In table 1 we report the analysis from equation (1) using as main explanatory variables the log of the total number of social organizations and its decompositions as well as an alternative measure of social connectedness and the normalized share of far-right votes. In Table B1 in Appendix B we report the full estimation table using the log of the total number of social organizations to check that all controls do have the expected sign. This is indeed the case: better educated people do better on the labor market, men work substantially more than women, unemployment lowers the labor market outcome of refugees while marginally increasing their schooling, the presence of non-western migrants reduces substantially mobility without affecting the labor market much.

Table 1: effects of social engagement on mobility, in education and employment outcomes

	Moved	In education	Employed	Employee	Experience	Independent
Total Social org.	0.0126	0.0112**	-0.0157	-0.0084	-19.7051	-0.0074**
	(1.496)	(2.140)	(-1.479)	(-0.808)	(-0.938)	(-2.420)
Observations	34453	34453	33941	33941	33941	33941
Adjusted R^2	0.111	0.064	0.214	0.200	0.278	0.027
Advocacy groups	0.0184***	0.0025	-0.0059	-0.0001	7.1453	-0.0059**
	(2.785)	(0.603)	(-0.659)	(-0.007)	(0.393)	(-2.465)
Observations	34453	34453	33941	33941	33941	33941
Adjusted R^2	0.112	0.064	0.214	0.200	0.278	0.027
Social activities	0.0098	0.0110**	-0.0200*	-0.0123	-26.5829	-0.0076**
	(1.191)	(2.052)	(-1.760)	(-1.129)	(-1.276)	(-2.307)
Observations	34453	34453	33941	33941	33941	33941

Adjusted R^2	0.111	0.064	0.214	0.200	0.278	0.027
Other Social org.	0.0024	0.0097***	-0.0031	-0.0013	-7.1583	-0.0020
	(0.476)	(3.149)	(-0.394)	(-0.165)	(-0.462)	(-1.320)
Observations	34418	34418	33908	33908	33908	33908
Adjusted R^2	0.110	0.065	0.214	0.200	0.278	0.026
Total nonprofit	0.0106	0.0128***	-0.0136	-0.0057	-19.3486	-0.0080**
	(1.130)	(2.663)	(-1.236)	(-0.522)	(-0.903)	(-2.486)
Observations	34453	34453	33941	33941	33941	33941
Adjusted R^2	0.111	0.064	0.214	0.200	0.278	0.027
far right (norm.)	0.0040	-0.0030	0.0083	0.0106*	23.5885*	-0.0023
	(1.176)	(-1.471)	(1.561)	(1.957)	(1.868)	(-1.554)
Observations	34453	34453	33941	33941	33941	33941
Adjusted R^2	0.111	0.064	0.214	0.200	0.278	0.026

Notes: The table displays the result of equation (1) run in each panel for a main dependent variable and in each column for a different independent variable. Observations for refugees 18-59, reaching Denmark between 2001 and 2011 and that remains in the country for at least 4 years are pooled together from the 1st to the 4th year after arrival in Denmark. All regressions include country of origin, month of permit, year of permit, and time since arrival fixed effect. Standard errors are clustered at the municipal level.

Turning to the main objects of interest, we do find that an increase in the total number of socially engaged organizations increases in education take-up and reduces self-employment. An increase in 10% of TS organizations increases in-education probability by 0.1 p.p. or 50% of a standard deviation. At the same time, TS organizations decreases self-employment of 0.075 p.p. or around 58% of a standard deviation. If we disentangle between the three components of social organizations, we uncover different patterns: the advocacy groups' presence does not impact education enrollment while it decreases self-employment and raises the probability of moving out. The other two groups, on the other hand, raises education of a similar magnitude, but not affect moving out. We conclude that there exist different effects for these groups that need to be considered. The presence of advocacy groups seems to play against refugees, that indeed tend to leave more from these places. On the contrary, other social groups help them integrating, at least by pushing them into some formal education. Reassuringly, in Panel E, our alternative measure of social organizations goes in exactly the same direction as the other ones. In the last panel we look at the impact of having a higher vote share going to the anti-immigrant parties in national elections. Interestingly, like Viarengo et al. 2022, we find that in places with more right-wing supporters refugees work significantly more (p=0.0503) and do so as employees. There

seems to be as well a negative effect on education, that is however not statistically significant at conventional levels.

In Appendix B, tables B2 and B3, we present results separated for men and women as descriptively the probability of working was sensibly higher for men with respect to women. Men, indeed, seems to react more on all margins, with larger coefficients that are also more statistically significantly different from zero. Social organizations though impact women education too, a potentially very important aspect for refugee women that are often even more marginalized than refugee men both culturally and economically. The impact of right-wing strength is instead entirely concentrated on men: refugee men reaching places with a higher support for anti-immigrant's parties are more employed, and particularly so as employees. Also, they are significantly less likely to be in education. This is not surprising as foreign men are often the main target of hostility and fear from the host population, but they are also much more actively involved in the labour market. Taken together, this evidence seems to point to refugees that to avoid discrimination decides to forego education and find an employment as fast as possible. Besides being or not in employment, the local conditions at arrival might influence the quality of the employment fount and more generally the quality of life. This is why, in Table 2, we investigate the impact of our variables of interest on salary, total income and disposable income. Perhaps not surprisingly, as they do not impact the employment probability, social organizations do not affect salaries or any other form of revenue in the short run. Refugees located in more right-wing communes, instead, enjoy a larger total and disposable income. This is not driven by better paying jobs though, but rather by the fact that they work more.

To get a sense of the size of the endogeneity we report in Appendix B, Table B4, the results for the full sample of analysis. For all variables, but the far-right regressions, the results are quite similar. The evidence for a positive effect of right-wing communes is also similar but weaker, both in magnitude and significance level. This is despite the test for endogenous sorting seemed to not be correlated with hidden ability measures from the very first months. One possibility is that more politically engaged individuals across all educational groups avoided high-right wing places more. Indeed, the share of right-wing vote is higher in municipalities reached by refugees after May. If more politically engaged individuals are more sensible to political hostility, they might react to it even if not directly exposed in their own commune. They would thus be already responding to a hypothetical high level of right-wing share, irrespective of the one in their specific location. On the contrary, less informed individuals might only react when exposed. As a confirmation of this hypothesis, all effects become larger and more significant each additional month subtracted: this is true also passing from May to June as selected threshold, but then it stabilizes. We conclude that this is additional evidence that after mid-year, self-sorting was likely to be difficult also on the political margin.

6.2 Long-term effects:

Recently, some scholars (Battisti et al., 2022; Foged, Hasager, & Peri, 2022) have underlined the importance of focusing also on the effects of the treatments beyond the first years. Indeed, there is a possibility that short and long run effects are not aligned. For instance, Foged et. al 2022 found that characteristics such as the share of co-ethnics, previously found to substantially matter in the short run, becomes irrelevant as years since settlement pass. On the other hand, other characteristics, such as the language skill level, takes time to translates into labour market rewards, but are then very persistent. With the same idea, we test if our local characteristics of interest change their effect in a slightly longer time horizon. We so run the same model as above but focusing on the 5 to 7 years after settlement. Obviously, the correlation between the values of our measures of social engagement at arrival and after several years will be smaller, opening the door for new confounders. Still, it seems important to check whether there is any sign of reversal or continuation. Since we find that the presence of social organizations leads to more education in the short run, and we know that education is beneficial on the labour market, we wonder if this translates in subsequent gains for the refugees. Similarly, starting to work early on might bring refugees on a better path that will be persistent even years later. The results of the analysis are presented in Table 3. None of the expected beneficial effects materializes in the mid-run: refugees located in more anti-migrant municipalities do not enjoy anymore an employment nor an income premium. The only coefficient that turns out positive is the cumulated experience, indicating that indeed, in the past, they had an advantage. Turning to the social organizations we even find a negative impact on the probability of being employed. This is entirely driven by the presence of advocacy groups that significantly lowers employment both as employees and self-employed (both coefficients are not statistically significant, but on the bord to be). This is in line with the short-term results where refugees moved out more from locations with a higher share of advocacy groups and were less likely to be self-employed, but not more to be in education. Overall, it thus seems that advocacy group advocate exclusively in the interests of the local population. Importantly, this is indeed what they are supposed to do, as refugee lack an interest group defending their interests and solely rely on other associational forms such as voluntary associations.

7 Conclusions:

Local conditions at arrival are supposed to matter substantially for the integration of refugees. Western countries should know which conditions is more beneficial for refugees so to design settlement policies that maximize their integration chances. Moreover, municipalities can also adjust some of these conditions to smoother refugee integration and guarantee local social mixing. Yet only a handful of conditions has made the object of a careful study. In this paper, we studied the effect that the social capital, measured as the presence of different types of social organizations, has on the cultural and economic integration of refugees. Social interactions are possible important channels through which information is diffused and negative shocks absorbed. Refugees can thus greatly benefit from being

placed in a community with more chances to interact and that is more engaged socially. On the other side, social groups are predominantly formed by the local population, and they can thus become a tool for exclusion if refugees face a barrier to entry. We test which effect is stronger in the case of refugee integration in Denmark. We find that refugee assigned to municipalities with a greater presence of social organizations tend to be more enrolled in schools, and less in self-employment. No positive effect is however found on the labour market, neither in the short, nor in the long run. We document instead a positive effect of anti-migrant vote on the refugee employment attachment in the short run. This effect is entirely concentrated on men, typically object of the largest resentment and fear towards refugees. This is in line with the effect found by Müller et al., 2023in Switzerland, but contrasts with similar research in Germany (Aksoy et al., 2023; Jaschke et al., 2022). The positive effect, though, is short lived: 5 to 7 years after arrival, refugees placed in more right-wing communes are as likely than others to be employed. We conclude that facing anti-migrant sentiment might push refugees to look faster for a job, partly at the expenses of education, but that this does not translate in a future advancement and might instead have negative consequences on their well-being not captured by the present study.

Table 2: Income

	Ln Disposable income	Ln Total income	Ln Salary
Total Social org.	0.0009	0.0036	-0.0546
	(0.067)	(0.243)	(-0.855)
Observations	34116	34128	10855
Adjusted R^2	0.523	0.501	0.124
Advocacy groups	-0.0105	-0.0071	0.0087
	(-0.929)	(-0.601)	(0.191)
Observations	34116	34128	10855
Adjusted R^2	0.523	0.501	0.124
Social activities	0.0093	0.0122	-0.0598
	(0.677)	(0.822)	(-0.924)
Observations	34116	34128	10855
Adjusted R^2	0.523	0.501	0.124
Other Social org.	-0.0017	-0.0041	-0.0387
	(-0.157)	(-0.375)	(-0.939)

Observations	34083	34095	10845
Adjusted R^2	0.523	0.501	0.124
Total nonprofit	-0.0025	-0.0009	-0.0644
	(-0.195)	(-0.065)	(-0.998)
Observations	34116	34128	10855
Adjusted R^2	0.523	0.501	0.124
far right (norm.)	0.0117*	0.0167**	0.0292
	(1.944)	(2.472)	(0.875)
Observations	34116	34128	10855
Adjusted R^2	0.523	0.501	0.124

Notes: The table displays the result of equation (1) run in each panel for a main dependent variable and in each column for a different independent variable. Observations for refugees 18-59, reaching Denmark between 2001 and 2011 and that remains in the country for at least 4 years are pooled together from the1st to the 4th year after arrival in Denmark. All regressions include country of origin, month of permit, year of permit, and time since arrival fixed effect. Standard errors are clustered at the municipal level.

Table 3: Middle-run effects of local social characteristics

	Employed	Employee	Experience	Indep.	Ln Disp. income	Ln Total income	Ln Salary
Total Social org.	-0.0319*	-0.0260	-45.0891	-0.0054	-0.0135	-0.0163	-0.0114
	(-1.855)	(-1.653)	(-0.724)	(-0.717)	(-0.815)	(-0.836)	(-0.123)
Obs	20116	20116	20116	20116	20098	20136	11082
Adjusted R^2	0.145	0.118	0.277	0.060	0.060	0.057	0.034
Advocacy groups	-0.0363**	-0.0255	-0.5617	-0.0101	-0.0088	-0.0100	0.0050
	(-2.023)	(-1.525)	(-0.011)	(-1.626)	(-0.664)	(-0.637)	(0.047)
Obs	20116	20116	20116	20116	20098	20136	11082
Adjusted R^2	0.145	0.118	0.277	0.060	0.060	0.057	0.034
Social activities	-0.0181	-0.0148	-32.3851	-0.0027	-0.0113	-0.0111	0.0355
	(-1.093)	(-0.944)	(-0.522)	(-0.359)	(-0.681)	(-0.568)	(0.426)
Obs	20116	20116	20116	20116	20098	20136	11082

Adjusted R^2	0.144	0.118	0.277	0.060	0.060	0.057	0.034

Other Social org.							
	-0.0159	-0.0151	-34.4228	-0.0010	0.0030	-0.0018	-0.0359
	(-1.344)	(-1.379)	(-0.707)	(-0.208)	(0.254)	(-0.123)	(-0.560)
Obs	20095	20095	20095	20095	20077	20115	11072
Adjusted R^2	0.144	0.118	0.277	0.060	0.061	0.057	0.034

Total nonprofit							
	-0.0247	-0.0205	-23.6007	-0.0038	-0.0182	-0.0162	0.0146
	(-1.420)	(-1.300)	(-0.384)	(-0.470)	(-1.008)	(-0.776)	(0.166)
Obs	20116	20116	20116	20116	20098	20136	11082
Adjusted R^2	0.145	0.118	0.277	0.060	0.061	0.057	0.034

far right (norm.)							
	-0.0060	-0.0039	37.1654	-0.0025	-0.0014	0.0013	-0.0371
	(-0.928)	(-0.723)	(1.215)	(-0.641)	(-0.197)	(0.151)	(-1.390)
Obs	20116	20116	20116	20116	20098	20136	11082
Adjusted R^2	0.144	0.118	0.277	0.060	0.060	0.057	0.034

Notes: The table displays the result of equation (1) run in each panel for a main dependent variable and in each column for a different independent variable. Observations for refugees 18-59, reaching Denmark between 2001 and 2011 and that remains in the country for at least 4 years are pooled together from the 5th to the 7th year after arrival in Denmark. All regressions include country of origin, month of permit, year of permit, and time since arrival fixed effect. Standard errors are clustered at the municipal level.

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Appendix A

Figure A1: Share of communes with a filled quota by year

a)   Filled by the end of May   b)   Filled by the end of June
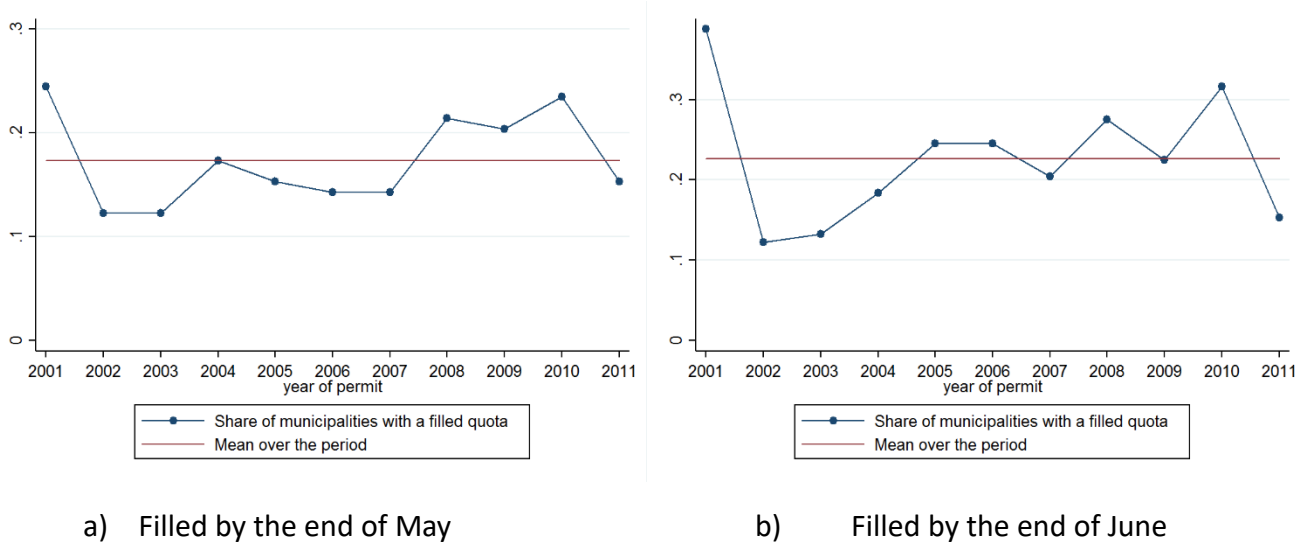
Table A1: Testing the selection of refugees across municipalities by education: social organizations

	(Ln) Advocacy groups	(Ln) Broad social groups	(Ln) Other groups	(Ln) Total non-profit	(Ln) Total non-profit 2
Panel A: Balanced panel					
10-12 years completed	-0.0202*	-0.0220**	-0.0137	-0.0189**	-0.0184**
	(-1.791)	(-2.193)	(-1.040)	(-2.011)	(-2.210)
above 12 years completed	-0.0249*	-0.0254**	-0.0134	-0.0203*	-0.0244**
	(-1.888)	(-2.289)	(-0.650)	(-1.809)	(-2.089)
Observations	10292	10292	10292	10292	10292
Adjusted R^2	0.605	0.807	0.642	0.762	0.762
Panel B: Arrived after 1 month(s)					
10-12 years completed					
	-0.0152	-0.0208*	-0.0103	-0.0156	-0.0157*
	(-1.266)	(-1.941)	(-0.700)	(-1.531)	(-1.714)
above 12 years completed	-0.0208	-0.0228**	-0.0073	-0.0164	-0.0199*
	(-1.522)	(-1.987)	(-0.344)	(-1.419)	(-1.696)
Observations	9663	9663	9663	9663	9663
Adjusted R^2	0.604	0.806	0.639	0.761	0.761

Panel C: Arrived after 2 month(s)

10-12 years completed	-0.0118	-0.0196*	-0.0055	-0.0131	-0.0146
	(-0.918)	(-1.717)	(-0.367)	(-1.208)	(-1.467)
above 12 years completed	-0.0176	-0.0185	-0.0102	-0.0138	-0.0166
	(-1.206)	(-1.523)	(-0.457)	(-1.141)	(-1.374)
Observations	8883	8883	8883	8883	8883
Adjusted R^2	0.602	0.805	0.639	0.760	0.760

Panel D: Arrived after 3 month(s)

10-12 years completed	-0.0136	-0.0219*	-0.0062	-0.0150	-0.0162
	(-0.988)	(-1.788)	(-0.389)	(-1.289)	(-1.504)
above 12 years completed	-0.0147	-0.0156	-0.0093	-0.0117	-0.0140
	(-1.017)	(-1.289)	(-0.441)	(-0.974)	(-1.133)
Observations	8236	8236	8236	8236	8236
Adjusted R^2	0.599	0.801	0.638	0.757	0.757

Panel E: Arrived after 4 month(s)

10-12 years completed	-0.0136	-0.0213*	-0.0045	-0.0143	-0.0158
	(-0.949)	(-1.717)	(-0.276)	(-1.222)	(-1.442)
above 12 years completed	-0.0195	-0.0175	-0.0121	-0.0135	-0.0135
	(-1.292)	(-1.367)	(-0.518)	(-1.066)	(-1.016)
Observations	7543	7543	7543	7543	7543
Adjusted R^2	0.597	0.801	0.641	0.757	0.757

Panel F: Arrived after 5 month(s)

10-12 years completed	-0.0153	-0.0195	0.0073	-0.0129	-0.0150
	(-0.973)	(-1.399)	(0.406)	(-0.990)	(-1.251)
above 12 years completed	-0.0231	-0.0177	-0.0096	-0.0140	-0.0150
	(-1.543)	(-1.394)	(-0.411)	(-1.112)	(-1.139)
Observations	6741	6741	6741	6741	6741
Adjusted R^2	0.592	0.801	0.643	0.757	0.758

Table A2: Testing the selection of refugees across municipalities by education: other local conditions

	Unemployment rate	Non-Western migrant share	Population share	Far right vote (norm.)
Panel A: Balanced panel				
10-12 years completed	-0.0000	0.0006**	-0.0002	0.0108
	(-0.141)	(2.067)	(-1.130)	(0.415)
above 12 years completed	-0.0005	0.0007**	0.0001	-0.0007
	(-1.645)	(2.137)	(0.401)	(-0.026)
Observations	10292	10292	10292	10292
Adjusted R^2	0.398	0.346	0.283	0.200
Panel B: Arrived after 1 month(s)				
10-12 years completed	0.0001	0.0007**	-0.0001	0.0139
	(0.181)	(2.163)	(-0.947)	(0.498)
above 12 years completed	-0.0005	0.0007**	0.0001	-0.0138
	(-1.423)	(2.075)	(0.349)	(-0.509)
Observations	9663	9663	9663	9663
Adjusted R^2	0.398	0.350	0.287	0.198
Panel C: Arrived after 2 month(s)				
10-12 years completed	0.0001	0.0006*	-0.0002	0.0086
	(0.225)	(1.871)	(-1.292)	(0.305)
above 12 years completed	-0.0004	0.0007*	0.0001	-0.0208
	(-1.031)	(1.945)	(0.356)	(-0.704)
Observations	8883	8883	8883	8883
Adjusted R^2	0.399	0.357	0.293	0.193
Panel D: Arrived after 3 month(s)				
10-12 years completed	-0.0002	0.0006*	-0.0002	0.0043
	(-0.429)	(1.876)	(-1.266)	(0.141)
above 12 years completed	-0.0004	0.0008*	0.0001	-0.0154
	(-0.914)	(1.985)	(0.362)	(-0.534)
Observations	8236	8236	8236	8236
Adjusted R^2	0.403	0.355	0.293	0.188

Panel E: Arrived after 4 month(s)				
10-12 years completed	-0.0001	0.0005	-0.0002	-0.0103
	(-0.231)	(1.534)	(-1.154)	(-0.335)
above 12 years completed	-0.0003	0.0009*	-0.0001	-0.0320
	(-0.619)	(1.953)	(-0.285)	(-0.980)
Observations	7543	7543	7543	7543
Adjusted R^2	0.409	0.358	0.296	0.187

Panel F: Arrived after 5 month(s)				
10-12 years completed	-0.0001	0.0005	-0.0001	-0.0234
	(-0.243)	(1.371)	(-0.453)	(-0.714)
above 12 years completed	-0.0003	0.0006	0.0001	-0.0377
	(-0.670)	(1.313)	(0.258)	(-1.083)
Observations	6741	6741	6741	6741
Adjusted R^2	0.418	0.361	0.300	0.186

Table A3: Descriptive statistics

VARIABLES	mean	sd
Demographics		
male	0.667	0.471
age	31.82	8.963
married	0.588	0.492
kids	1.007	1.474
0-9 years	0.570	0.495
10-12 years	0.247	0.431
more than 12 years	0.183	0.387
Year of permit:		
2001	0.246	0.431
2002	0.134	0.341
2003	0.0920	0.289
2004	0.0597	0.237
2005	0.0468	0.211
2006	0.0488	0.215
2007	0.0507	0.219
2008	0.0584	0.234
2009	0.0585	0.235
2010	0.0969	0.296
2011	0.108	0.311

Table A3 cont.: descriptive statistics

VARIABLES	Mean	sd
Month of Permit:		
1	0.0603	0.238
2	0.0746	0.263
3	0.0619	0.241
4	0.0658	0.248
5	0.0766	0.266
6	0.118	0.323
7	0.107	0.309
8	0.0709	0.257
9	0.104	0.306
10	0.0927	0.290
11	0.0449	0.207
12	0.123	0.328
Country of origin:		
Irak	0.193	0.395
Afghanistan	0.166	0.372
Iran	0.0938	0.292
Myanmar	0.0712	0.257
Somalia	0.0670	0.250
Syrien	0.0495	0.217
BosniaHZ	0.0383	0.192
Russia	0.0357	0.186
Juguslavia	0.0284	0.166
Eritrea	0.0276	0.164
Others	0.229	0.420

Table A4: Differences in refugees' characteristics, by sample

	Before June	After June	Difference
Demographics:			
male	0.665	0.667	0.002
	(0.472)	(0.471)	(0.010)
age	32.282	31.588	-0.694***
	(9.032)	(8.918)	(0.185)
married	0.622	0.570	-0.052***
	(0.485)	(0.495)	(0.010)
kids	1.023	0.998	-0.024
	(1.470)	(1.477)	(0.030)
0-9 years	0.525	0.593	0.068***
	(0.499)	(0.491)	(0.010)
10-12 years	0.274	0.233	-0.042***
	(0.446)	(0.423)	(0.009)
more than 12 years	0.201	0.174	-0.027***
	(0.401)	(0.379)	(0.008)
Year of permit:			
2001	0.273	0.232	-0.041***
	(0.446)	(0.422)	(0.009)
2002	0.166	0.118	-0.048***
	(0.372)	(0.322)	(0.007)
2003	0.100	0.088	-0.013**
	(0.301)	(0.283)	(0.006)
2004	0.051	0.064	0.013***
	(0.220)	(0.245)	(0.005)
2005	0.040	0.050	0.010**
	(0.197)	(0.218)	(0.004)
2006	0.031	0.058	0.027***
	(0.173)	(0.234)	(0.004)
2007	0.032	0.060	0.029***
	(0.176)	(0.238)	(0.005)
2008	0.049	0.063	0.014***
	(0.215)	(0.243)	(0.005)
2009	0.037	0.070	0.033***
	(0.189)	(0.254)	(0.005)
2010	0.124	0.083	-0.041***
	(0.330)	(0.276)	(0.006)
2011	0.097	0.114	0.018***
	(0.296)	(0.318)	(0.006)

Table A4 cont.: Differences in refugees' characteristics, by sample

	Before June	After June	Difference
Country of origin:			
Irak	0.232	0.173	-0.060***
	(0.422)	(0.378)	(0.008)
Afghanistan	0.173	0.163	-0.010
	(0.378)	(0.369)	(0.008)
Iran	0.080	0.101	0.020***
	(0.272)	(0.301)	(0.006)
Myanmar	0.030	0.092	0.062***
	(0.170)	(0.290)	(0.005)
Somalia	0.054	0.073	0.019***
	(0.227)	(0.261)	(0.005)
Syrien	0.075	0.036	-0.038***
	(0.263)	(0.188)	(0.004)
BosniaHZ	0.054	0.030	-0.023***
	(0.225)	(0.172)	(0.004)
Russia	0.040	0.033	-0.007*
	(0.196)	(0.180)	(0.004)
Juguslavia	0.047	0.019	-0.029***
	(0.212)	(0.135)	(0.003)
Eritrea	0.036	0.023	-0.013***
	(0.187)	(0.150)	(0.003)
Others	0.178	0.256	0.077***
	(0.383)	(0.436)	(0.009)

Table A5: Descriptive statistics on municipalities of arrival

VARIABLES	mean	sd
Advocacy groups	96.80	60.91
Broad social groups	361.5	307.3
Other support groups	58.82	58.88
Total non-profit groups	404.5	347.3
Non-profit, alternative	480.3	419.3
Share population (%)	1.106	0.750
Share immigrants (%)	4.813	1.928
Share non-western (%)	3.497	1.438
Share co-nationals (%)	0.131	0.164
Unemployment rate (%)	3.815	1.394
Far right vote share (%)	13.37	2.544

Table A6: Differences in municipalities' characteristics, by sample

	Before June	After June	Difference
Advocacy groups	99.644	95.338	-4.306***
	(62.759)	(59.896)	(1.258)
Broad social groups	355.949	364.304	8.355
	(310.451)	(305.628)	(6.348)
Other support groups	59.974	58.230	-1.744
	(60.332)	(58.114)	(1.216)
Total non-profit groups	399.709	406.901	7.192
	(351.737)	(345.055)	(7.176)
Non-profit, alternative	479.123	480.900	1.777
	(429.271)	(414.121)	(8.663)
Share population (%)	1.153	1.082	-0.071***
	(0.774)	(0.736)	(0.015)
Share immigrants (%)	4.944	4.747	-0.197***
	(2.002)	(1.886)	(0.040)
Share non-western (%)	3.638	3.424	-0.214***
	(1.496)	(1.402)	(0.030)
Share co-nationals (%)	0.152	0.121	-0.032***
	(0.173)	(0.158)	(0.003)
Unemployment rate (%)	3.808	3.818	0.010
	(1.281)	(1.448)	(0.029)
Far right vote share (%)	13.189	13.463	0.274***
	(2.569)	(2.527)	(0.052)

Appendix B

Table B1: Effect of total number of social organizations on move, education, and the labor market

	Moved	In education	Employed	Employee	Experience	Independent
Total Social org.	0.0126	0.0112**	-0.0157	-0.0084	-19.705	-0.0074**
	(1.496)	(2.140)	(-1.479)	(-0.808)	(-0.938)	(-2.420)
Non-Western Share	-0.9464***	0.2075	0.3853	0.2587	665.176	0.1167
	(-3.985)	(1.306)	(0.966)	(0.652)	(0.843)	(1.041)
Share population	-1.1659**	-1.2457***	2.0323**	1.5527*	2336.450	0.4963*
	(-2.048)	(-3.320)	(2.566)	(1.885)	(1.330)	(1.725)
age	-0.0036**	-0.0080***	0.0016	0.0016	13.754***	0.0000
	(-2.403)	(-5.855)	(0.691)	(0.673)	(3.190)	(0.057)
age squared	0.0000	0.0001***	-0.0001***	-0.0001***	-0.288***	-0.0000
	(1.285)	(5.046)	(-3.558)	(-3.240)	(-4.945)	(-1.023)
10-12 years completed	-0.0006	0.0059*	0.0197**	0.0185**	30.132*	0.0013
	(-0.114)	(1.878)	(2.464)	(2.259)	(1.961)	(0.494)
above 12 years completed	-0.0014	0.0017	0.0208**	0.0184**	26.386*	0.0024
	(-0.291)	(0.506)	(2.583)	(2.194)	(1.808)	(0.839)
married	-0.0341***	-0.0070***	-0.0441***	-0.0458***	-74.069***	0.0016
	(-6.861)	(-2.852)	(-6.150)	(-6.300)	(-5.004)	(0.654)
kids	-0.0076***	-0.0009	-0.0093***	-0.0093***	-15.544***	-0.0001
	(-5.060)	(-0.924)	(-4.155)	(-4.040)	(-3.776)	(-0.204)
male	0.0091**	0.0070**	0.1821***	0.1673***	271.233***	0.0149***
	(2.520)	(2.316)	(28.252)	(25.376)	(19.366)	(10.031)
unemployment rate	0.4069	0.2523*	-1.0308**	-1.0639**	-2687.931***	0.0347
	(1.450)	(1.669)	(-2.345)	(-2.300)	(-2.727)	(0.426)

far right (norm.)	0.0042	-0.0029	0.0081	0.0105[*]	23.3678[*]	-0.0024
	(1.231)	(-1.456)	(1.525)	(1.937)	(1.851)	(-1.656)
Constant	0.0629	0.0874[**]	0.1034	0.0730	-83.4835	0.0305
	(1.199)	(2.234)	(1.505)	(1.074)	(-0.666)	(1.358)
Observations	34453	34453	33941	33941	33941	33941
Adjusted R^2	0.111	0.064	0.214	0.200	0.278	0.027

t statistics in parentheses

All regressions include month of permit, year of permit, and country of origin fixed effects. Standard errors are clustered at the commune level

Table B2: Effects of social organizations on men move, education and labor market outcomes

	Moved	In education	Employed	Employee	Experience	Independent
Total Social org.	0.0166	0.0126**	-0.0218	-0.0119	-18.5987	-0.0098**
	(.)	(1.995)	(-1.565)	(-0.862)	(-0.627)	(-2.228)
Observations	22990	22990	22603	22603	22603	22603
Adjusted R^2	0.121	0.073	0.204	0.190	0.304	0.029
Advocacy groups	0.0219	0.0036	-0.0074	0.0003	22.4623	-0.0076**
	(.)	(0.693)	(-0.667)	(0.028)	(0.970)	(-2.306)
Observations	22990	22990	22603	22603	22603	22603
Adjusted R^2	0.122	0.072	0.203	0.190	0.304	0.029
Social activities	0.0135	0.0123*	-0.0267*	-0.0164	-30.6066	-0.0103**
	(.)	(1.954)	(-1.790)	(-1.133)	(-1.046)	(-2.167)
Observations	22990	22990	22603	22603	22603	22603
Adjusted R^2	0.121	0.073	0.204	0.190	0.305	0.029
Other Social org.	0.0059	0.0085**	-0.0049	-0.0024	-5.3843	-0.0025
	(.)	(2.308)	(-0.514)	(-0.239)	(-0.261)	(-1.102)
Observations	22975	22975	22588	22588	22588	22588
Adjusted R^2	0.120	0.073	0.203	0.190	0.304	0.029
Total nonprofit	0.0144	0.0143**	-0.0207	-0.0099	-20.7227	-0.0107**
	(.)	(2.488)	(-1.428)	(-0.684)	(-0.685)	(-2.265)
Observations	22990	22990	22603	22603	22603	22603
Adjusted R^2	0.121	0.073	0.204	0.190	0.304	0.029
far right (norm.)	0.0045	-0.0040*	0.0096	0.0130*	29.5882*	-0.0034
	(.)	(-1.682)	(1.457)	(1.937)	(1.743)	(-1.573)
Observations	22990	22990	22603	22603	22603	22603
Adjusted R^2	0.121	0.072	0.203	0.190	0.304	0.029

Table B3: Effects of social organizations on women move, education and labor market outcomes

	Moved	In education	Employed	Employee	Experience	Independent
Total Social org.	0.0076	0.0099*	-0.0071	-0.0033	-17.4674	-0.0043**
	(0.769)	(1.701)	(-0.686)	(-0.333)	(-1.200)	(-2.169)
Observations	11463	11463	11338	11338	11338	11338
Adjusted R^2	0.092	0.066	0.163	0.161	0.180	0.014
Advocacy groups	0.0124	0.0003	-0.0070	-0.0040	-19.5670	-0.0033*
	(1.614)	(0.061)	(-0.726)	(-0.420)	(-1.240)	(-1.912)
Observations	11463	11463	11338	11338	11338	11338
Adjusted R^2	0.093	0.065	0.163	0.161	0.180	0.013
Social activities	0.0049	0.0101*	-0.0095	-0.0052	-14.4647	-0.0044**
	(0.515)	(1.815)	(-0.879)	(-0.501)	(-0.934)	(-2.389)
Observations	11463	11463	11338	11338	11338	11338
Adjusted R^2	0.092	0.066	0.163	0.161	0.180	0.014
Other Social org.	-0.0023	0.0110**	-0.0008	-0.0005	-8.5079	-0.0012
	(-0.327)	(2.533)	(-0.093)	(-0.055)	(-0.707)	(-1.124)
Observations	11443	11443	11320	11320	11320	11320
Adjusted R^2	0.092	0.066	0.163	0.161	0.180	0.013
Total nonprofit	0.0050	0.0102*	-0.0030	0.0008	-12.4373	-0.0043**
	(0.471)	(1.791)	(-0.284)	(0.080)	(-0.854)	(-2.337)
Observations	11463	11463	11338	11338	11338	11338
Adjusted R^2	0.092	0.066	0.163	0.161	0.180	0.014
far right (norm.)	0.0017	-0.0016	0.0009	0.0019	4.0651	-0.0009
	(0.454)	(-0.502)	(0.170)	(0.346)	(0.468)	(-1.265)
Observations	11463	11463	11338	11338	11338	11338
Adjusted R^2	0.092	0.065	0.163	0.161	0.180	0.013

Table B4: Testing for impact of endogenous sorting using the full sample

	Moved	In education	Employed	Employee	Experience	Independent
Total Social org.	0.0133	0.0112***	-0.0114	-0.0034	-8.2145	-0.0081***
	(1.611)	(2.636)	(-1.043)	(-0.315)	(-0.397)	(-3.317)
Observations	52147	52147	51567	51567	51567	51567
Advocacy groups	0.0183***	0.0045	-0.0052	0.0019	9.9786	-0.0073***
	(2.728)	(1.172)	(-0.554)	(0.202)	(0.541)	(-3.475)
Observations	52147	52147	51567	51567	51567	51567
Social activities	0.0107	0.0111***	-0.0130	-0.0057	-13.5839	-0.0073***
	(1.374)	(2.636)	(-1.166)	(-0.517)	(-0.644)	(-2.693)
Observations	52147	52147	51567	51567	51567	51567
Other Social org.	0.0035	0.0063**	-0.0034	-0.0005	-1.8209	-0.0031**
	(0.730)	(2.514)	(-0.420)	(-0.060)	(-0.125)	(-2.082)
Observations	52112	52112	51534	51534	51534	51534
Total nonprofit	0.0108	0.0129***	-0.0093	-0.0011	-9.4852	-0.0084***
	(1.217)	(3.226)	(-0.856)	(-0.103)	(-0.446)	(-3.246)
Observations	52147	52147	51567	51567	51567	51567
far right (norm.)	0.0033	-0.0016	0.0071	0.0090*	19.4320	-0.0019
	(1.088)	(-0.870)	(1.370)	(1.730)	(1.569)	(-1.580)
Observations	52147	52147	51567	51567	51567	51567