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Labour market integration of refugees and the importance of the neighbourhood: Norwegian quasi-experimental evidence

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Abstract

This paper exploits a quasi-experimental feature of the Norwegian spatial dispersal policy for UNHCR quota refugees, which leads to nearly as-if random initial residential settlement of the refugees. In this framework, we study if there are positive long-run employment consequences of being assigned to neighbourhoods with higher residential labour force participation rates. Our results show a positive and statistically significant relationship between the initial neighbourhood participation rates and refugee labour market outcomes, but these overall effects are substantively small: A one standard deviation higher participation rate in the initial neighbourhood is associated with an 1.2%-point increase in the refugees' later employment probability. However, our subgroup analysis shows substantial effects around 2.6%-points for men older than 25 years at the time of entry to Norway. In comparison, the point estimates for women and persons younger than 25 years at the time of arrival are close to zero and statistically insignificant.

Keywords Refugee employment, Settlement policy, Neighbourhood, Quasi-experimental, Administrative data

JEL Classification J15, J18, R23

1 Background

Employment is pivotal for the successful integration of refugees. In many countries, including Norway, a dispersed settlement of refugees is an important component in the overall integration policy. Such a policy may have several potential advantages: First, it redistributes the financial and social costs between the local authorities. Second, it alleviates the housing demand in neighbourhoods near the capacity limit. And finally, on the individual level, it may increase the speed of acquiring host-country specific human capital, such as language skills and knowledge about the host country, through increased interaction with the majority population. The question we address in this paper is how individual refugees' long-term outcomes are affected by their initial local neighbourhood of residence in the host country.

On a general level, several authors have proposed mechanisms through which the neighbourhood of settlement may matter for individual outcomes; see, e.g. Bramoullž et al. (2020), Graham (2018) or Manski (1993). First, there may be geographical variation in the supply of high-quality public services and amenities (e.g. schools, doctors or infrastructure), regional labour market conditions, subnational social policies and so forth. Second, the characteristics and behaviours of one's neighbours influence own life outcomes. For example, if information and referrals from neighbours affect employment outcomes, then living in a disadvantaged neighbourhood makes acquiring a job more difficult; we will return to this briefly. We are interested in the combined effect of both causal pathways (hereafter defined as neighbourhood effects).

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From a policy perspective, if neighbourhood effects exist, it may be economically beneficial to have a public policy that directs families with unfavourable employment outlooks to neighbourhoods with a higher proportion of employed neighbours (compared to random or as-if random dispersal). Such a policy may also be used to alleviate the pressure upon immigrant dense neighbourhoods with low employment rates that otherwise could develop into ghettos.

The main challenges to measuring neighbourhood effects are homophily (e.g. McPherson et al. 2001), and simultaneity bias (see Manski 1993). First, in the non-refugee population, the neighbourhood where families live is a deliberate choice. Family characteristics—observed or not—may affect their choice of the neighbourhood of residence. This results in neighbourhoods of people with similar attributes (homophily). If the same characteristics also affect our outcome of interest, it leads to neighbourhoods with different average employment outcomes. In this case, the relationship between neighbourhood outcomes and individual outcomes is due to self-selection, not neighbourhood effects. Finally, all else being equal, if neighbours' behaviours affect an individual's behaviours (i.e. peer effects are present), this will violate the usual Stable Unit Treatment Value Assumption (SUTVA, see Holland 1986). In practice, this means that over time the presence of feedback loops caused by peer effects results in effect sizes that are too small or too large (Manski 1993). The remedy is to limit individuals' prior exposure to a neighbourhood. These two problems usually make it impossible to estimate the impact of the neighbourhood on later outcomes without access to a source of exogenous variation in the settlement location. Due to a series of pressures and constraints, Norway settles certain refugees across the country in a nearly as-if random fashion. The settlement policy also implicitly applies restrictions on where refugees can move, limiting the possibility of sorting based on the homophily principle or moving to labour market opportunities. In addition, refugees are new entrants to Norway, with no prior exposure to their future neighbours, which reduces simultaneity bias.

Our paper focuses on labour market outcomes, but other researchers have studied different outcomes using similar quasi-experimental designs. It should be noted that similar allocation policies exist in a several Western countries. A recent overview of related literature is found in Kosyakova and Kogan (2022). On Norwegian data, Bratsberg et al. (2020) studies how refugees later local election participation is shaped by the initial neighbours' tendency to participate in the elections. For Denmark, Damm and Dustmann (2014) have studied how neighbourhood crime impacts later criminal behaviour of youth. Both studies conclude that the initial neighbourhood has an impact on later individual outcomes.

In Sweden, a dispersed settlement policy for refugees was in effect from 1985 to 1991.[1] Edin et al. (2003) estimates the causal effect on labour market earnings of living in ethnic enclaves for refugees; an ethnic enclave is defined on the basis of the number of co-nationals residing in the municipality. After sorting into neighbourhoods is taken into account, earnings increase by 13 per cent for the low-skilled if the ethnic stock of the neighbourhood increase by one standard deviation. Similarly, on Danish data, Damm (2009), also analyse the effects of the size of the ethnic enclave on the labour market earnings of immigrants. She accounts for sorting using a danish dispersal policy in effect from 1986 to 98, and finds that a one standard deviation increase in ethnic enclave size increases earnings for the low-skilled by 18 per cent. Both studies focus on the effect of the current neighbourhood and not on the impact of the initial neighbourhood, and the neighbourhoods are defined at the municipality level with a median of 16,000 and 10,000 inhabitants for the Swedish and Danish studies, respectively.

Using the same quasi-random neighbourhood assignment of refugees, Damm (2014) investigate how living in a socially deprived neighbourhood affects labour market outcomes. Their sample of interest were male refugees, including asylum-seekers, aged 18–59 years. She defines a socially deprived neighbourhood as one where the employment rate is at most 60 per cent. As in Edin et al. (2003) and Damm (2009), the focus is on the impact of the *current* neighbourhood on current outcome measures, and identification strategy uses the *initial* neighbourhood as an instrument. For this analysis, a neighbourhood is defined in terms of 2296 grid-squares with an average size of 2343 inhabitants of which 119 are deprived in 2004. The instrumental variable results show no statistically significant effects on refugee men. Their labour market outcomes are also not affected by the current neighbourhood's overall employment rate and average skill levels. However, an increase in the employment rate among non-western immigrant men living in the current neighbourhood does significantly raise the current earnings. The author concludes that this provides evidence that residence-based job information networks are ethnically stratified.

[1] Edin et al. (2004) studies the overall effects of a change in the Swedish immigration policy. The policy dispersed refugee immigrants across Sweden, but it also placed immigrants on introductory support for the first 18 months, which switched focus away from immediate labour market integration. The overall effect on immigrants' long-run earnings was negative, but the effect primarily arose because of the shift in labour market focus and not because of the dispersed settlement.

Germany has similar allocation policy for recently arrived refuges as the one found in Norway, and this policy has been utilised for studying causal issues related to the integration of migrants (Kanas et al. 2022). Using a representative large household survey of migrants in Germany, Gërxhani and Kosyakova (2022) examine the impact of social networks for migrants' first integration into labour markets. They also study the initial settlement of refugees and other immigrants, focusing on transition rates into their first jobs and the quality of their first job. By and large they find no causal evidence of social networks themselves to have any impact on measured labour market integration. Using the same type of natural experiment in Germany, Kanas et al. (2022) study whether the influence of being residentially allocated to a linguistic enclave have any negative effect on improving language skills. Contrary to the previous literature, they find that residing in linguistic enclaves does not impede the learning of the native language.

Finally, on Norwegian data, Godøy (2017) identifies the effect of being initially settled in a labour market regions where other non-OECD immigrants do well and find that it increases the refugees' annual earnings up to 6 years after immigration. In total, there are 46 labour market regions in Norway, and Godøy focuses on 7394 refugees, aged 18–55 years, in the year of settlement (1993–2007). To proxy the employment prospects in the labour market region, the author uses local employment rates, defined as the share of residents in a region aged 25–55 with earnings above a certain minimum value.

The question we address in this paper is whether the place of initial settlement affects refugee labour market integration. For illustrative reasons, we chose a proxy for neighbourhood labour force participation as the marker of neighbourhood quality. We do not make a direct causal link between neighbourhood participation rates and individual outcomes: common causes such as regional labour markets and social programs can affect both. From a policy perspective focused on optimising dispersal rules, it is not pragmatically relevant to separate these effects. Contrary to e.g. Godøy (2017), we focus on small geographical areas. Our approach is similar to Bratsberg et al. (2020), who also study the effect of small neighbourhoods. Their focus is on refugees' political participation, and unlike Bratsberg et al., we do not claim direct causal effects (i.e. no common causes) between neighbourhood labour force participation rates and refugee labour market outcomes; the neighbourhood rate of participation is very likely correlated with, say, the number of jobs within commuting distance.

The layout of the rest of paper is as follows: Below, we discuss the data available, including our treatment and outcome variables, as well as our empirical estimation

Table 1 Characteristics for in-sample refugee neighbourhoods in 2008

	With settlement		Total	
	Mean	**SD**	**Mean**	**Diff.**
LFP rate (16–74 years), %	81.38	6.02	81.33	− 0.04
Non-western residents, %	8.98	9.48	5.01	− 3.97***
Higher education, %	30.66	12.85	26.17	− 4.49***
SA-recipients (18–59 years), %	4.89	4.55	3.71	− 1.18***
Avg. wage inc. (16–74 years), t.NOK	318.01	57.36	309.19	− 8.82***
Avg. population	639.01	492.24	339.02	− 299.98***
Observations	3209		13,820	

See Appendix Table 10 for an extended version. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

strategy. In the subsequent section, we discuss the quasi-experiment design using the Norwegian settlement policy for refugees and how we exploit this policy for identification purposes. Finally, we present the empirical results and discuss its implications for policy and the wider academic field.

2 Empirical strategy

From the Norwegian statistical bureau, *SSB*, we have access to individual-level administrative panel-data for every citizen with residence in Norway, as well as immigrants and refugees with a legal residence. Each individual is uniquely identified via an anonymised number, through which different administrative registers can be linked. Most of our data is available in the period from 1990 until the end of 2019, and thus we are able to follow-up on the entire population, at the individual-level, for a long time period.

We study quota refugees entering Norway between 1990 and 2012. Before entry into Norway, quota refugees are assigned to a municipality, and the municipality finds accommodation in a local neighbourhood. In this analysis, we define a neighbourhood as a 'grunnkrets', which is a small spatial unit within a municipality. The neighbourhoods are defined by SSB.[2] The country is divided into more than 420 municipalities and some 14,000 neighbourhoods that are stable in our period of analysis and had an average population of about 339 persons in 2008.

Table 1 shows that neighbourhoods where the sample refugees initially settle are more urban: they have larger populations, more non-western residents, higher

[2] See Statistics Norway's definition of a neighbourhood (a 'grunnkrets'), as well as the historical background in Byfuglien and Langen (1983).

proportions of residents with higher education, and higher proportions of social assistance recipients compared to the overall average. Thus, the initial neighbourhoods are not representative of the average Norwegian neighbourhood. This is expected since municipalities settle refugees into areas with available social housing or privately rented accommodation.[3]

In our analysis, the outcome of interest is whether the individual refugee finds employment, which we measure from 2010 until 2019. Employment is measured as a binary indicator equal to one, if a person is linked to an employer in a given year in the administrative tax registers, and otherwise zero. Due to the nature of our outcome variable, we only sample individuals in their prime working ages (25–59 years) in a given year the outcome is measured. In 2019, the average refugee in our sample had been in Norway for 20 years.

The treatment variable is the labour force participation (abreviated LFP or participation) rate among the working-age neighbours in the refugee's initial settlement neighbourhood. The LFP rate is measured in the year of arrival. Our hypothesis is that a higher participation rate among neighbours will exhibit a positive relationship with refugees' later employment outcomes. In practice, we proxy the participation rate for the neighbourhood, r, by the number of individuals aged 16–74, who have a positive annual wage income, divided by the number of individuals in this age-range.[4] The advantage of this neighbourhood 'quality' index, is that it is stable over time compared to other potential measures (e.g. unemployment), and, on average in the refugee initial settlement neighbourhoods, it increases only slightly from around 76 percent in the early 1990's to around 80 percent from the early 2000's. A more volatile neighbourhood index would allow the 'quality' of a neighbourhood to seemingly drop in periods of economic recession, which, we argue, in reality does not necessarily reflect the truth about a neighbourhood, its residents, and their network etc.: a temporary economic downturn does arguably not suddenly deteriorate the human or social capital embedded with the residents in a neighbourhood, although this may happen over time due to sorting in the housing market etc. In Fig. 1, we compare our the labour force participation of the in-sample initial settlement neighbourhoods with the national official representative

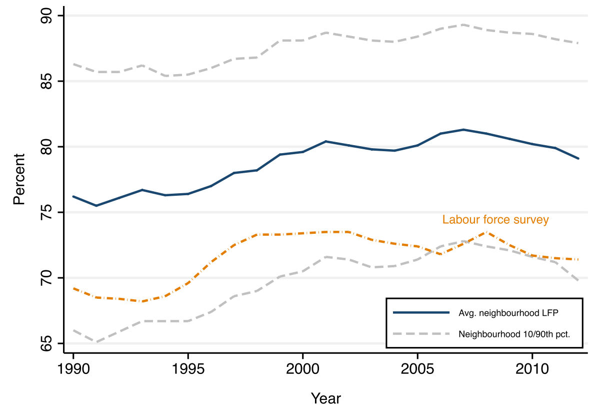

Fig. 1 This illustration shows the average labour force participation (LPF) of initial settlement municipalities

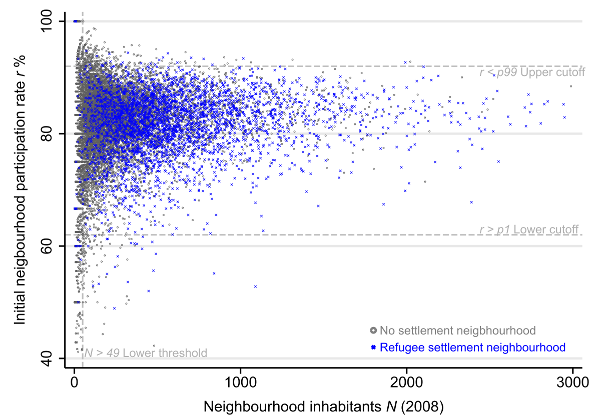

Fig. 2 This figure shows the variance of the LPF in relation to the neighbourhood size

survey-based labour force participation. As note above, the refugee-settlement neighbourhoods are not representative of the typical neighbourhood.

The 10th. and 90th. percentile, in Fig. 1, nevertheless reveals a substantial variation between the participation rates of the initial settlement neighbourhoods. To explore this variation further, we have plotted the neighbourhood participation rates for all neighbourhoods against the number of inhabitants. This is shown in the funnel-like plot in Fig. 2, where the blue crosses indicate a neighbourhood with refugee-settlement. As would be expected, the variance of the participation rate depends on the number of inhabitants in the neighbourhood. As we have sufficient data, have chosen to limit the potential impact of outliers and cut our sample based on the following criteria: the initial neighbourhood must have 50 or more inhabitants, and, the participation rate must be below the 99th percentile and above the 1st percentile in overall

[3] Table 1 summarises characteristics for 3209 neighbourhoods that were used for initial settlement from 1990 to 2012 for our sample of refugees, and compares with the total of 13,820 neighbourhoods that existed in 2008. Hence, the observation count differs slightly from that of our main analysis. To allow for comparison, the neighbourhood statistics in Table 1 are measured in 2008-values, but for our analysis, the neighbourhood statistics vary by the time of arrival.

[4] The SSB wage definition is available at their website. See also Epland and Kirkeberg (2001).

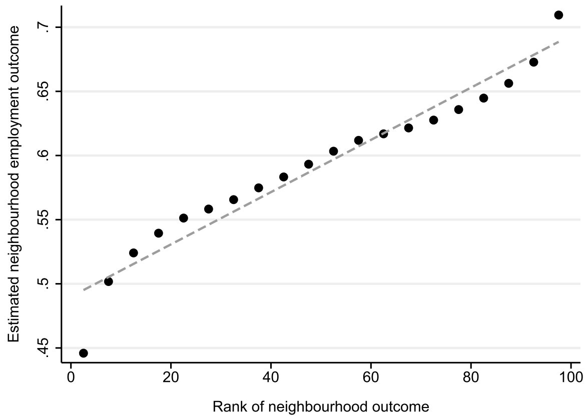

Fig. 3 This figure illustrates the predicted employment probability by the rank of the initial neighbourhood

Table 2 Initial individual characteristics. Arrival before 2010 and 2019

Category	2010	2019	Category	2010	2019
Gender: Man	53.8	51.9	*Family*: single	26.9	22.9
Arrival: 1990–1994	45.2	36.0	- parent	12.0	15.0
1995-1999	26.4	25.9	couple	5.0	4.0
2000-2004	18.3	19.2	- w/child	55.0	57.2
2005-2009	10.1	11.5	other	1.0	1.0
2010-2012	0.0	7.4	*Origin*: Iran or Iraq	23.2	23.9
Age entry: 0–6	0.3	10.1	Europe	46.4	39.0
7-15	13.4	22.8	Africa	10.0	14.4
16-29	42.5	42.5	Asia	20.3	22.7
30-44	39.9	23.4			
45-	3.8	1.2			
Education: basic	33.0	47.6			
secondary	27.5	26.5			
- upper	23.4	15.7			
higher	16.1	10.2			
			Observations	21,228	25,601

distribution of the settlement neighbourhoods in order to be included.

The next question we address, is the distributional differences between the initial settlement neighbourhoods and the later refugee employment outcomes. Figure 3 illustrates the association between the predicted probability of later refugee employment and the rank of the refugee's initial neighbourhood. Specifically, the leftmost dot indicates that the refugees who were initially settled in one of the lowest performing neighbourhoods had a 45 per cent chance of being employed in 2019 (after adjusting for the refugees' initial individual characteristics and the year of arrival). In contrast, the rightmost dot indicates an employment probability of 71 per cent among the refugees who were settled in the best performing neighbourhoods. Each dot in the diagram represents 5 per cent of the neighbourhoods, which is about 156 units. If we compare neighbourhoods ranking around the 20th and the 80th percentile, then the predicted employment rates differ by about 10 percentage points, which is about the size of the gap in employment rates among natives and immigrants in Norway,[5] and thus indicating that the initial neighbourhood might be an important predictor of refugee labour market participation.

Our main results are based on regressions of the binary refugee employment outcomes, Y_{in}, on the initial standardised neighbourhood labour force participation rate, r_n, refugee initial characteristics, X_i, and the initial neighbourhood characteristics, V_n, where i indexes individuals and n indexes neighbourhoods. Note that all right hand side variables in equation (1) are measured at the time of arrival, although the mathematical notation only indexes i and n. However, in a typical observational context, people would self-select into neighbourhoods, which would make r an endogenous choice. In the next section, we discuss our identification strategy, but before we turn to that, we will briefly summarise the characteristics of the individual refugees in our samples.

$$Y_{in} = \alpha + \theta r_n + \beta \mathbf{X}'_i + \chi \mathbf{V}'_n + \epsilon_{in}, \tag{1}$$

Our main results focuses on individual employment outcomes in 2019. This sample has 25,601 individual observations in the ages between 25 and 59 years in 2019. To investigate the trend of the results, we run the same specification separately year-by-year for each of the years 2010–19. Each sample is restricted to only the mentioned age interval, and thus the sample sizes vary. Table 2 summarises sample for the first year, 2010, and the last year, 2019, in our outcome window. Note that the 2010-sample includes only refugees, who arrived before 2010. The majority of refugees arriving in the early 1990's were from the ex-Yugoslavian republic, while large cohorts arriving in the late 1990's and early 2000's were from Iran or Iraq. The typical UNHCR-refugee was relatively young, had little or no education beyond basic schooling, and arrived with his or her family. Please refer to appendix Table 6 for selected summary statistics by the year of arrival.

3 Identification

Our identification strategy follows Bratsberg et al. (2020) (who studies election turnout among refugees), and exploits the as-if-random nature of the Norwegian dispersal settlement policy for United Nations High

[5] See SSB Table09837.

Commissioner for Refugees (UNHCR) quota refugees. As mentioned before, this type of research design has been used by other researchers. In Norway, once a person has been given refugee status, they are allocated a municipality to settle in. The allocation of settlement areas for refugees is not intentionally random; however, the allocation process for quota refugees results in nearly random allocations due to two factors: limited information on refugees prior to settlement and an overriding policy focus on quick settlements.

Statistics Norway (see Tønnessen and Andersen 2019) conducted a thorough investigations into the Norwegian dispersal system and the extent to which it is random which we now summarise. The majority of refugees in Norway are former asylum seekers who have travelled to Norway to apply for refugee status within the country (ibid.). However, a significant minority apply for refugee status from outside Norway with the assistance of the UNHCR. For these refugees, the UNHCR create their refugee applications and caseworkers from various Norwegian agencies travel to a third country to interview the refugees. Upon a successful application, a specialised settlement team must assign refugees to a settlement municipality prior to their arrival in Norway. The settlement decision team have limited information about refugees collected by caseworkers who interviewed the refugees. In addition, within municipalities, the local government has to make informed decisions about housing based on limited information about individual refugees. Whilst it is policy to try to accommodate refugees' wishes and backgrounds, the biggest priority has always been to ensure a quick settlement decision (ibid., section 2). This is particularly true during refugee crises when there is high housing demand. Testing for random allocation, Tønnessen and Andersen (2019) conclude that whilst allocation is not truly random, the correlation between confounders and municipality characteristics is extremely weak (see ibid., Appendix F and table 6.13).

Given the above constraints and pressures, the municipality will settle the family conditional on the limited information they have on the family beforehand and depending on available housing at the current time. For quota refugees, municipalities will likely place the family according to whatever suitable public or private housing is available at the time of arrival. Therefore, we assume that the actual neighbourhood where the refugee and their family are settled is as-if random, conditional on the time of arrival. We investigate this assumption below using falsification tests.

Because refugees are in principle free to relocate anywhere in Norway—but they have strong incentives to stay in their initially allocated settlement municipality[6]—we are estimating the intention-to-treat (ITT) effect of neighbourhoods.

Table 3 F-tests of the relationship between the initial individual characteristics and the neighbourhood LFP rate (2019 sample)

	F-test	p-value
Full set of variables	7.242	0.000***
All variables except year of arrival	1.270	0.207
Year of arrival	10.540	0.000***
Education	0.635	0.592
Gender	2.174	0.140
Family type	2.060	0.083*
Country of origin	1.267	0.281
Age	0.936	0.442

25,601 individual refugees and 3118 initial neighbourhoods

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

This argument implies that the refugee's pre-arrival characteristics should not have any predictive power in relation to the participation rate of the neighbours in the initial neighbourhood, conditional on the year of arrival:

$$r_n = \alpha + \boldsymbol{\beta}\mathbf{X}_i' + \gamma\, t_i + \epsilon_n, \tag{2}$$

where r_n is the participation rate in the initial neighbourhood n (measured in the time of arrival), $\mathbf{X}$ is a vector of refugee characteristics also measured upon arrival—gender, age, education, origin, and the family type—and t is the year of arrival in Norway.

Table 3 shows F-test statistics after a linear regression similar to equation (2) of the relationship between the initial individual characteristics and the neighbourhood participation rate. A model with the full set of variables—including both year of arrival, t, and the vector of individual characteristics, $\mathbf{X}$—has predictive power, but this is predominantly because of a strong correlation between the year of arrival and the neighbourhood participation rate, and not because of the individual characteristics, $\mathbf{X}$.

For education, gender, country of origin, and age, we cannot reject the null hypothesis, but family type is statistically significant at a 10 percent level. Our 2019-sample has 25,601 individuals, and even *qualitatively* small differences may produce *statistically* significant differences. Indeed, the mean initial neighbourhood participation is 77.5 per cent for couples with children, 77.3 for singles,

[6] Since September 2004, new refugees aged between 18-55 have an obligation and duty to participate in a full-integration scheme called the Norwegian Introduction Programme (NIP). Aside from their obligations, refugees are entitled to a special benefit for each hour of participation in NIP. This benefit is, however, conditional on the refugees staying within their allocated settlement municipality. See e.g. Djuve et al. (2017).

Table 4 Main results: 2019-employment change (%-points) of a 1SD increase in the initial neighbourhood LFP rate

	(1)	(2)	(3)
LFP rate	1.811***	1.710***	1.227**
	[0.453]	[0.446]	[0.538]
Year of arrival	Yes	Yes	Yes
Individual controls		Yes	Yes
Neighbourhood controls			Yes

25,601 individuals and 3118 neighbourhoods. Standard errors are clustered on neighbourhoods. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and 77.5 for single parents. However, a selection into neighbourhoods based on the type of family intuitively makes sense, because the family type partially determines the housing needs: a cohabiting couple with children is presumably more likely to be placed in a house in a residential neighbourhood, as opposed to a single person household without children, who are perhaps more likely to be in an apartment. In our main results, we control for all the initial individual characteristics listed in Table 3, as well as the initial neighbourhood characteristics summarised in Table 1.

4 Results

Here we present our main results from a linear probability model (LPM) similar to equation (1), which is estimated in a three-step procedure. The three-step estimation procedure implies that we obtain the residuals from a regression of (*a*) the binary outcome indicator on the control variables, and (*b*) the treatment variable—i.e. the standardised initial neighbourhood participation (LFP) rate—on the control variables, after which, the final results are obtained by regressing the residuals from (*a*) on the residuals from (*b*).

The estimates represent the average effect—measured in percentage points—on the 2019 refugee employment probability of a one standard deviation increase in the initial neighbourhood's participation rate. All models control for the refugee's year of arrival, but model (2) also controls for the observed individual characteristics (listed in Table 2), and, in addition, model (3) controls for the characteristics of the initial neighbourhood (see Table 1) measured in the time of arrival.

The estimates of the initial neighbourhood's effect are statistically significant ($p < 0.01$) in the first two specifications in Table 4, but the standard error increases in model (3) and leaves the estimate significant only at the 5 per cent level. According to model (3), a one standard deviation change in the participation rate of the initial neighbourhood (about 7%-points) leads to a change in the refugees' 2019-employment probability of approximately 1.2%-points. To give a sense of scale: the estimate corresponds to about 1/7 of the predicted employment probability difference between a neighbourhood ranked 20th and 80th (see Fig. 3).

Figure 4 shows the equivalent of model (3) in Table 4, but re-estimated separately year-by-year from 2010 until 2019. The square indicates the point estimate, while the vertical lines are the 95% and 90% C.I.'s. The regression line is the prediction from a fractional polynomial regression of the point estimates on time. We note that the results are stable over time and, in most years, individually statistically significantly different from zero even at the 5 per cent level. In summary, there is a small positive relationship between the initial neighbourhood labour force participation rate and refugee outcomes. However, according to our results, settling a refugee in neighbourhood in the 20th to the 80th percentile would only imply modest improvements in the long-run employment outcome.

To ease comparison with other studies that focus on short or medium long-run outcomes measured a certain number of years after arrival, we have re-estimated the model separately from 3 years to 9 years after arrival (see Fig. 5 in the appendix). These results suggests relatively larger effects after 3 years than after 9 years, i.e. the impact of the initial neighbourhood exhibits a declining time profile. This time-profile is accordance with

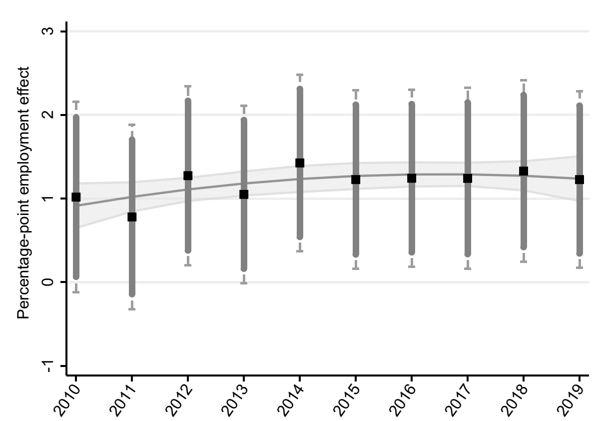

Fig. 4 This illustration demonstrates the point estimates for different outcome years

Table 5 Subgroup results: 2019-employment change (%-points) of a 1SD increase in the initial neighbourhood LFP rate, by gender and age at arrival

Subgroup	(1)	(2)	(3)
Men ≤25 (n = 8177)	1.442**	1.222*	0.727
Men >25 (n = 5118)	2.843***	2.876***	2.565***
Women ≤25 (n = 7731)	1.927***	1.657**	0.683
Women >25 (n = 4575)	0.640	0.786	0.939

See appendix Table 9 for details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

what has been reported by Godøy (2017), who study the impact of local labour market regions on later refugee earnings.

4.1 Heterogeneity

Similarly to the results in Table 4, we have done subgroup analysis, based on the specification in model (3), separately by age and gender. The subgroup results are summarised in Table 5.

The results clearly show that the effect of the initial neighbourhood is driven by men older than 25 years at the time of arrival to Norway, while the point estimates for other groups are smaller and statistically insignificant. The results for men older than 25 years at arrival shows that a one standard deviation increase in the initial neighbourhood participation rate will raise the later employment probability by about 2.6 percentage points.

5 Discussion

In our full sample results, there is little evidence to support a re-distribution policy for quota refugees into areas with higher participation rates. Our results do not suggest that re-distribution would substantially improve refugee outcomes. However, our subgroup results show that the small overall effect size is solely driven by women and those younger than 25 upon arrival.

We hypothesise that the individual's behavioural response to the neighbourhood participation signal may partly be determined by the level of signal exposure as well as the ability to respond. If women to a higher degree have the primary caring responsibility in the family then they are likely to be unable to offer their labour (i.e., a lower signal response). However, women older than 25 years at arrival have the lowest raw 2019-employment of around 49 per cent, although it is only slightly higher at 51 per cent for men older than 25 years upon arrival. For comparison the raw 2019-employment rates are about 63 and 66 per cent for younger women and men, respectively. Whilst interesting, distributing refugees by gender would probably be challenging from a practical policy perspective.

To our knowledge, the Norwegian Directorate of Integration and Diversity (IMDi), who oversees the resettlement process, already practises some targeted dispersal based on education and health requirement (i.e., individuals with the need for highly specialised hospital treatment). However, as mentioned, implementation is limited due to lack of information and the focus on quick resettlement as evidenced by Godøy (2017) and Tønnessen and Andersen (2019).

Under current Norwegian policy, municipalities are given a fixed block grant for each settled refugee (ibid.). This may give municipalities an incentive to quickly make refugees self-sufficient. Redirecting refugees towards areas with better labour market outcomes may not incur additional cost, and the level of grant excess could increase if refugees become self-sufficient sooner. In the light of potential individual and social benefits, a practical policy recommendation may be to increase focus on the quality of the settlement for better long-term integration prospects; see Bansak et al. (2018) for an interesting machine learning approach to improving refugee integration.

6 Limitations

The quasi-experimental feature of the Norwegian resettlement scheme for quota-refugee will, in principle, ensure an unbiased estimate of the neighbourhood. However, we make no causal claim about which *mechanism* might be directly responsible for the effects, but, as we discussed in the introduction, our interest is on the combined effect of the mechanisms at work.[7] Neighbourhood labour force participation is the neighbourhood quality signal that we study, but we do not claim that increasing neighbourhood participation would improve refugee outcomes (all else being equal), because this measure may be correlated with other measures. Isolating the effect

[7] See Galster (2012) for an overview of neighbourhood mechanisms.

of participation rates or similar neighbourhood characteristic would require more assumptions about random variation in characteristics across neighbourhoods that are not justified by our setting. This is a limitation shared with other similar quasi-experimental designs and actual experiments with limited control over treatment delivery across sites, including the 'Moving to Opportunities'-project (Ludwig et al. 2008).[8] We believe that the association between neighbourhood and individual employment outcomes is highly relevant for considering alternative resettlement policies.

The as-if random settlement scheme is only used for quota refugees. About 1/5 of the refugees that enter Norway are quota refugees, while the remainder are asylum seekers (see Tønnessen and Andersen 2019, Table 3.2). For the stock of quota-refugees, the top-3 nationalities are Bosnia, Iran and Irak (48%), while it is Somalia, Eritrea and Iraq for asylum seekers (44%) (see Bratsberg et al. 2020, Table A2). Due to their size, evidence about asylum seekers is very policy-relevant. Future research will confirm if our results are externally valid to other refugee groups.

Finally, it is worth noting the intention-to-treat nature of our design, arising because quota-refugees are allowed to move to a neighbourhood of their own choice, should they not wish to remain in the initially assigned neighbourhood. In total, 1/3 of the refugees in our sample have moved to a different neighbourhood after 5 years in Norway.

7 Conclusion

Our results show that there are statistically significant effects on quota refugee's later employment probability of what neighbourhood they were initially placed in, when they arrive in Norway. We proxy the 'quality' of the first neighbourhood by the labour force participation rate of the inhabitants, and our neighbourhood's are defined as geographically small areas with a median size of about 310 persons. For identification, we exploit an as-if random dispersal of quota refugees. Although the main results are statistically significant, they are also quantitatively small: A one standard deviation higher participation rate in the initial residential neighbourhood implies about an 1.2 percentage point increase in the refugees long-run employment probability. These results suggest that the labour force participation rate in the initial neighbourhood matter little for the refugees labour market integration. However, subgroup analysis reveals that the small main effect sizes are driven by women and persons below the age of 25 years at the time of arrival. For men aged 25 years or older a one standard deviation increase in the initial neighbourhood participation rate will raise the later employment probability by around 2.6 percentage points.

Appendix

See Fig. 5 and Tables 6, 7, 8, 9, 10.

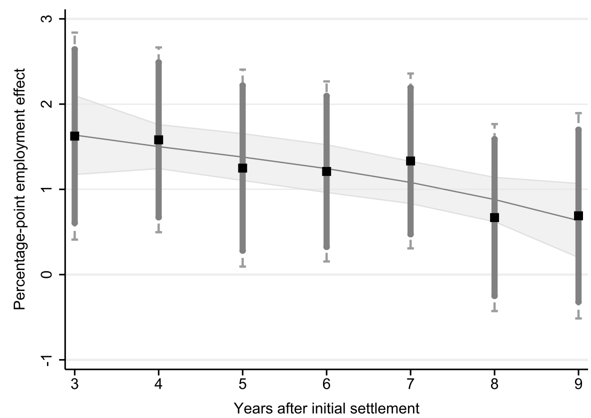

Fig. 5 This illustration demonstrates the point estimates for 3–9 years after arrival outcome years. Sample size varies between 17 and 20.000 individuals, as individual employment outcomes are only observed 1995–2019. Results are otherwise based specification (3) in Table 4

[8] See another example from Hotz et al. (2006).

Table 6 Selected summary statistics, by year of arrival

Year	Employment pct. (after five years)	Higher education	Men, pct.	Cohabitating couple w/child	Age	Origin, pct. Europe	Mid-east	Observations
1990	33.3	2.7	58.8	41.4	20.9	2.6	31.9	954
1991	43.1	5.8	61.2	47.7	21.3	1.4	46.0	1,169
1992	52.9	10.9	59.4	51.0	20.5	19.9	45.6	1,518
1993	62.7	16.9	50.7	62.2	21.3	87.5	8.1	5,393
1994	67.7	16.1	51.8	65.7	22.9	87.2	8.1	2,688
1995	57.2	7.7	52.4	69.4	22.5	76.2	17.0	1,379
1996	53.8	5.8	50.5	71.1	22.6	58.0	34.1	821
1997	46.2	8.3	59.0	66.8	21.8	15.0	76.6	913
1998	40.5	11.3	56.2	68.4	22.5	9.3	71.8	826
1999	22.0	12.8	53.0	63.0	23.0	73.6	14.0	3708
2000	46.3	7.9	53.4	64.2	24.0	30.5	27.3	1183
2001	56.5	12.3	53.1	59.4	25.2	13.2	41.7	1166
2002	58.8	19.1	52.6	52.2	25.7	7.7	41.3	976
2003	66.4	16.2	56.6	53.5	26.9	2.8	30.9	1227
2004	71.4	13.3	48.5	37.2	26.0	1.1	6.5	873
2005	70.9	17.8	53.9	60.4	28.0	1.5	1.4	518
2006	72.5	9.5	54.3	62.8	28.1	1.7	2.8	643
2007	65.2	11.6	53.9	52.3	28.8	0.0	8.4	751
2008	64.5	19.8	50.8	49.0	30.2	0.9	22.7	449
2009	54.6	8.8	41.9	46.8	31.0	0.3	25.2	797
2010	53.3	12.6	40.8	46.1	31.3	0.5	31.3	595
2011	57.4	17.9	36.7	37.6	30.9	0.7	23.4	559
2012	58.4	10.4	40.3	51.3	31.8	0.6	14.8	846

29,952 observations including everyone aged 25–59 during 2010–19

Table 7 Linear probability model coefficients from our main 2019-sample (part a)

	Left hand side			
	r_n		Y_{in}	
Year of entry: 1990				
1991	0.023	[0.038]	− 0.959	[2.268]
1992	− 0.140***	[0.036]	2.136	[2.149]
1993	− 0.180***	[0.034]	8.437***	[2.020]
1994	0.100***	[0.036]	10.733***	[2.168]
1995	− 0.062	[0.039]	5.702**	[2.348]
1996	− 0.101**	[0.044]	1.421	[2.594]
1997	− 0.161***	[0.042]	− 0.954	[2.475]
1998	− 0.165***	[0.043]	0.516	[2.556]
1999	0.072**	[0.036]	− 25.846***	[2.126]
2000	0.136***	[0.040]	0.591	[2.407]
2001	0.254***	[0.041]	3.599	[2.468]
2002	0.171**	[0.043]	3.547	[2.562]
2003	0.077*	[0.043]	10.514***	[2.538]
2004	0.013	[0.046]	11.340 ***	[2.749]
2005	− 0.100*	[0.052]	16.371***	[3.096]
2006	0.065	[0.051]	21.708***	[3.036]
2007	0.050	[0.052]	17.149***	[3.097]
2008	− 0.013	[0.058]	17.890***	[3.437]
2009	− 0.094*	[0.053]	11.533***	[3.185]
2010	− 0.209***	[0.058]	13.110***	[3.444]
2011	− 0.238***	[0.060]	9.518***	[3.575]
2012	− 0.333***	[0.058]	10.631***	[3.478]
Observations	25601		25601	
R^2	0.398		0.120	

Standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 Linear probability model coefficients from our main 2019-sample (part b)

	Left hand side			
	r_n		Y_{in}	
Gender: woman				
Man	− 0.015	[0.010]	4.172***	[0.602]
Age entry: 0-6				
7–15	0.005	[0.019]	− 4.242***	[1.118]
16–29	0.026	[0.021]	− 17.875***	[1.251]
30–44	0.019	[0.023]	− 30.589***	[1.347]
45 +	0.081	[0.050]	− 60.154***	[3.009]
Education: basic				
Secondary	− 0.014	[0.016]	1.699*	[0.938]
Upper	0.008	[0.018]	5.515***	[1.066]
Higher	− 0.007	[0.020]	8.972***	[1.199]
Origin: Iran or Iraq				
Europe	− 0.070***	[0.017]	− 0.814	[1.000]
Americas	− 0.115	[0.096]	5.865	[5.745]
Africa	− 0.022	[0.019]	8.975***	[1.105]
Asia	− 0.003	[0.016]	10.514***	[0.946]
Family: single				
Parent	− 0.032*	[0.018]	0.987	[1.053]
Couple	0.022	[0.027]	8.120***	[1.595]
W/child	0.004	[0.014]	5.578***	[0.817]
Other	0.210***	[0.051]	− 3.918	[3.050]
Neighbourhood: pop.	0.000***	[0.000]	0.000	[0.000]
Mean wage income, t.NOK	0.003***	[0.000]	0.028**	[0.012]
Share SA recipients	− 5.856***	[0.119]	− 2.866	[7.094]
Share high educ.	1.661***	[0.060]	− 2.659	[3.565]
Share immigrants	− 2.165***	[0.060]	− 14.984***	[3.565]
Constant	− 0.331***	[0.046]	56.919***	[2.728]
Observations	25601		25601	
R^2	0.398		0.120	

Standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9 Subgroup results: 2019-employment change (%-points) of a 1SD increase in the initial neighbourhood LFP rate, by gender and age at arrival

Subgroup	(1)	(2)	(3)
Men ≤25 (n=8,177)	1.442**	1.222*	0.727
	[0.639]	[0.628]	[0.778]
Men >25 (n=5,118)	2.843***	2.876***	2.565***
	[0.672]	[0.658]	[0.871]
Women ≤25 (n=7,731)	1.927***	1.657**	0.683
	[0.710]	[0.682]	[0.809]
Women >25 (n=4,575)	0.640	0.786	0.939
	[0.822]	[0.791]	[0.998]

Standard errors are clustered on neighbourhoods

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

Table 10 Characteristics for 2008 refugee neighbourhoods

	In-sample neighbourhoods with settlement							All	
	Mean	SD	p10	p25	p50	p75	p90	Mean	Difference
LFP rate (16–74 years), %	81.38	6.02	73.86	78.38	82.19	85.45	87.96	81.33	− 0.04
Non-western residents, %	8.98	9.48	1.87	3.56	6.23	10.77	18.28	5.01	− 3.97***
Higher education, %	30.66	12.85	16.54	21.26	28.18	37.65	49.43	26.17	− 4.49***
SA-recipients (18–59 years), %	4.89	4.55	1.30	2.27	3.75	6.17	9.61	3.71	− 1.18***
Avg. wage inc. (16–74 years), t.NOK	318.01	57.36	261.81	286.16	312.52	344.67	386.08	309.19	− 8.82***
Avg. population	639.01	492.24	184.00	317.00	513.00	822.00	1225.00	339.02	− 299.98***
Observations	3209							13,820	

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

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Author contributions
HLA has had the main responsibility in the writing of the overall paper. This involves the design of the analytical strategy, putting together data, analysing and interpreting the results. He is hence, listed as a first author. HLA has worked together with MLZ and LO. MLZ main contributions are related to the identification strategy and related literature. LO contributions are related to working with the existing relevant literature, data collection and insight into the Norwegian system. The co-authors have contributed to critical revisions of the article. All authors read and approved the final manuscript.

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Availability of data and materials
The register data used in this study are at the individual level and by definition contains sensitive information. Researchers can apply to access register data from Statistics Norway.

Declarations

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Not applicable.

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The authors declare that they have no competing interests.

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