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Refugees' Transition from Welfare to Work: A Quasi-Experimental Approach of the Impact of the Neighbourhood Context

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Abstract

This study analyses the impact of the neighbourhood context on the likelihood that refugees move from social assistance to paid employment. It makes use of Dutch policy that resulted in an exogenous placement of refugees in their first regular housing. This natural quasi-experiment allows us to estimate intent-to-treat effects of initial neighbourhood characteristics on the likelihood of transitioning from welfare to work. We consider the impact of the employment share and the median level of income among natives and co-ethnics, using Dutch longitudinal administrative data and discrete time event-history modelling. Our findings indicate that refugees are more likely to enter the labour market when the neighbourhood's employment share among natives is higher. A similar effect for employment among co-ethnics is not found. There is also no evidence that the placement of refugees in an area with a higher median income among co-ethnics or natives facilitates the transition from welfare to work.

Introduction

Immigration and asylum seeker flows have become a major concern in the public debate of many European countries. These discussions revolve around the possibility and desirability of regulating entry, but also around the weak labour market integration of newcomers and processes of residential segregation by ethnic origin.

Refugees tend to end up in a particularly vulnerable state, compared to both the native population and labour migrants (Engbersen *et al.*, 2015; Bakker, Dagevos and Engbersen, 2017).[1] While the labour market position of refugees improves over time (Bakker, Dagevos and Engbersen, 2017), they usually do not catch up completely (Wooden, 1991; Zwysen, 2019). For

instance, Engbersen *et al.* (2015) find that among the refugees arriving in the Netherlands between 1995 and 1999, about 50 per cent had a job after 10 years of residency, up from about 22 per cent after 2 years of residency. From a policy perspective, this makes it pertinent to understand the factors affecting refugees' labour market outcomes.

The social context refugees encounter upon arrival in their host country may be particularly important in shaping their future labour market outcomes. Refugees initially do not have an extensive personal network, and are more likely to form new social ties compared to other types of immigrants (Andersson, Musterd and Galster, 2019). Under the implicit assumption that refugees mainly form social relations with people from the same ethnic background, the characteristics of co-ethnics in the receiving local area have been argued to either promote or inhibit refugees' labour market outcomes. Refugees are supposed to fare better if they are surrounded by employed co-ethnics, as these will be able to provide information about job leads. Larger ethnic groups may even offer employment opportunities within an 'ethnic' economy (Wilson and Portes, 1980). However, having co-ethnics in the neighbourhood may also serve as a disincentive for acquiring relevant host-country skills (especially language) because it lowers contact with natives (Lazear, 1999). This can in turn harm the labour market chances of newly arrived refugees.

Clear scientific evidence on the relative strength of these arguments, however, is difficult to obtain. It is hard to separate the influence of (i) self-selection into certain areas, from (ii) the causal influence of the composition of the neighbourhood on an individual. Previous studies provide conflicting evidence on the influence of the presence of co-ethnics on immigrants' labour market outcomes. Some studies show a negative relationship between co-ethnic clustering and immigrants' labour market outcomes (Galster, Metzger and Waite, 1999; Borjas, 2000; Clark and Drinkwater, 2002; Logan, Zhang and Alba, 2002), while others have found more mixed or nuanced evidence (Sanders and Nee, 1987; Zhou and Logan, 1989; Xie and Gough, 2011; Andersson, Musterd and Galster, 2014). One strategy to deal with selection bias is to identify natural quasi-experiments, in which sorting is independent of observed and unobserved individual characteristics. Along this line, a more recent stream of research has drawn on refugees' initial place of residence that—due to the settlement policy in some countries—is independent of refugees' characteristics (Edin, Fredriksson and Åslund, 2003; Åslund and Fredriksson, 2009; Damm, 2009, 2014; Beaman, 2012). The outcomes of these studies are, however, not clear-cut either.

We draw on an exogenous placement procedure of Dutch refugees to deal with sorting bias. We add to related studies from Denmark, Sweden, and the United States by offering evidence from the Netherlands, which provides a different institutional context, as it combines traits of the Continental and Nordic welfare regimes (Powell, Yörük and Bargu, 2020). Second, we model the transition from welfare to work as a dynamic process. Although previous research has mainly looked at refugees' labour market outcomes 5 or 10 years after settlement, we make use of monthly, longitudinal data and employ event-history models. Third, we also consider characteristics of the native population in the area. Drawing on the ethnic enclave literature, past studies have mainly focused on the characteristics of co-ethnics. We presume that the native population can play an important role in shaping the labour market outcomes of refugees, especially if these end up in an area with relatively few co-ethnics.

We employ administrative data from the Netherlands to investigate the following research question: *To what extent do the presence and characteristics of co-ethnics and of the native population in the neighbourhood affect refugees' likelihood of transitioning from social assistance to work?* The data enable us to identify refugees—an asylum migrant with a granted residence status, their initial place of residence, and their subsequent labour market activity. We focus on refugees placed in regular housing between 1999 and 2009 and analyse to what extent they enter the labour market during the following 10 years.

Theory and Background

The Influence of co-Ethnics

Migration scholars have argued that living in ethnic enclaves—or ethnic concentration more generally—can either improve or hamper immigrants' labour market outcomes. Ethnic concentration may provide them with access to valuable 'ethnic' social capital. For example, Portes and colleagues (Wilson and Portes, 1980; Portes and Shafer, 2007) have argued that ethnic enclaves can foster ethnic niche economies that offer immigrants ample opportunity to find jobs. In a niche economy, co-ethnics—people with the same national-origin background—provide job leads and access to jobs, and thereby a higher likelihood of employment. It is debated to what extent immigrants' earnings are also positively affected (Sanders and Nee, 1987; Xie and Gough, 2011). While ethnic enclaves—in the sense of local labour markets dominated by one ethnic group—are rare

in the Netherlands, a more general version of the ethnic-enclave argument may be applicable. This states that co-ethnics may offer recently arrived immigrants a broad range of information, such as direct job leads, how to apply for jobs, or the best way to navigate the host country's labour market—and this is not necessarily confined to jobs within an ethnic niche economy.

Scholars have also pointed to the potentially detrimental influences of ethnic concentration. By lowering contact with natives, immigrants are discouraged to acquire human capital that is specific to the host country (Lazear, 1999), especially the host-country language. This makes it more difficult for immigrants to succeed on the host-country labour market (Chiswick and Miller, 2001; Kanas and van Tubergen, 2009; de Vroome and van Tubergen, 2010). This mechanism is argued to be stronger if the size of the immigrant group is larger (Lazear, 1999).

A slightly different argument posits that it is not the ethnic concentration and number of co-ethnics that matter most for immigrants' labour market outcomes. Rather, it is crucial to consider the 'quality' (Damm, 2014) or socioeconomic characteristics of co-ethnics. This fits within a social capital framework (Lin, 1999; McDonald *et al.*, 2013), in which social resources among co-ethnics determine to what extent co-ethnics can help immigrants integrate in the labour market. These resources include the extent of employment among co-ethnics and the type of jobs they have. For example, a higher employment rate among co-ethnics will increase the flow of host-country labour market information, which can facilitate a swift labour market integration for recently arrived immigrants. Moreover, co-ethnics in certain jobs may be able to influence the hiring process (den Butter, Masurel and Mosch, 2004). In the economic literature, this notion is in line with the 'ethnic capital' or the human externality argument (Borjas, 1995).

Empirical results so far do not offer conclusive evidence in favour of either argument. Some studies suggest a negative relationship, others a mixed or no relationship between the share of co-ethnics in the neighbourhood and immigrants' labour market outcomes. One reason for these diverging results is the difficulty in dealing with selection bias: How to separate the effect of the neighbourhood from the tendency of people with specific (unobservable) characteristics to cluster in a specific neighbourhood? Here, we briefly review research that is closest to our approach: Studies that draw on the exogenous placement of refugees. Methodologically, the initial exogenous placement of refugees upon granted asylum has mainly been used as an instrument to study

the influence of the neighbourhood on labour market outcomes (Edin, Fredriksson and Åslund, 2003; Åslund and Fredriksson, 2009; Damm, 2009, 2014). Regarding the concentration of co-ethnics, the main result is that there is no effect on immigrants' welfare receipt (Åslund and Fredriksson, 2009) and employment (Damm, 2009, 2014). For earnings, the empirical results are more mixed. Swedish evidence suggests that a higher number of co-ethnics makes for higher earnings, but only for the lower educated (Edin, Fredriksson and Åslund, 2003). In Denmark, Damm (2009) finds that a higher number of co-ethnics positively affects earnings, but Damm (2014), controlling for the quality of the neighbourhood,[2] finds no impact of the number of co-ethnics in the neighbourhood. Separating the effects of annual inflows of exogenously placed refugees in the United States, Beaman (2012) finds that a recent higher inflow of co-ethnics makes for lower employment probabilities and lower earnings, whereas a higher inflow 3 years prior to settlement, in contrast, makes for higher employment probabilities and higher earnings. This suggests that the influence of the number of co-ethnics may depend on how much time has passed since the other co-ethnics arrived.

There is more evidence that the socioeconomic characteristics of the co-ethnics affect refugees' labour market outcomes. Damm (2009, 2014) finds that both the level of education and the level of income among co-ethnics positively affect earnings and employment. Similarly, Åslund and Fredrikson (2009) conclude that a higher share of welfare recipients among co-ethnics increases the likelihood of welfare receipt.

The results emanating from previous studies thus are somewhat mixed. Taken together, however, they point to a relationship between better labour market outcomes among co-ethnics and better individual labour market outcomes among refugees. As for the share of co-ethnics in the population, there does not seem to be any relationship with the refugees' labour market outcomes. Combining theory with these results, we formulate the following hypotheses:

H1a: A higher employment rate among co-ethnics makes refugees more likely to transition from social assistance to work.
H1b: A higher level of income among co-ethnics makes refugees more likely to transition from social assistance to work.

The Influence of Natives
Studies concerned with the impact of ethnic concentration have mainly investigated the role of co-ethnics. Little

attention has been paid to the potential role of natives for immigrants' labour market achievements. Implicitly, it is assumed that immigrants tend to form social ties with co-ethnics—i.e. ethnic homophily (McPherson, Smith-Lovin and Cook, 2001)—and that these are the most important for their labour market outcomes. It is true that immigrants—and people in general—tend to form social ties with people with a similar ethnic background (McPherson, Smith-Lovin and Cook, 2001; Schaeffer, 2013; van Tubergen, 2015). However, other studies show that ethnic minorities also maintain social contacts with natives and other ethnic minority groups (Vervoort, Flap and Dagevos, 2011; Martinović, 2013).

Moreover, social ties with natives and other ethnic groups may be relatively more important for refugees' labour market outcomes. As argued by Putnam (2000), it is particularly bridging social capital, which spans socioeconomic or ethnic boundaries that offers access to unique information. Along this line, a qualitative study among Romanian people in London points to the important role social ties with natives can play in the acquisition of cultural resources needed to navigate the formal labour market in the host country (Moroşanu, 2016). Similarly, Gericke et al. (2018) offer qualitative evidence on Syrian refugees in Germany indicating that particularly social contacts with a different ethnic background were helpful in gaining access to both low- and high-skilled jobs. Social contacts with co-ethnics tended to only yield access to low-skilled jobs. This is partially mirrored in quantitative research, where some studies suggest that social contact with natives is associated with better labour market outcomes, while the evidence regarding contact with co-ethnics is more mixed (Kanas, van Tubergen and van der Lippe, 2011; Heizmann and Böhnke, 2016; Lancee, 2016).

To our knowledge, studies that have investigated this for refugees are few. Markussen and Røed (2015) juxtapose the influence of co-ethnics in the neighbourhood with the influence of other (non-Western) immigrants and natives, and find that it is particularly the co-ethnics that affect individual social insurance receipt. Their study, however, does not focus solely on refugee groups but also includes non-Western immigrants who arrived for other reasons. Damm (2014) investigates refugees exclusively, and suggests that a high employment rate among non-Western immigrants from a different country of origin in the area improves immigrants' employment probability but does not affect earnings, although co-ethnics seem to be more influential. Yet it remains unclear how the natives in the neighbourhood affect refugees' labour market outcomes. Based on the theoretical considerations and the outcomes of previous research discussed above, we presume that:

H2a: A higher employment rate among natives makes refugees more likely to transition from social assistance to work.
H2b: A higher level of income among natives makes refugees more likely to transition from social assistance to work.

However, a high employment rate and high level of income of the local native population do not only indicate a favourable social environment upon settlement, but also a favourable economic environment. If there is high demand for labour and more jobs are available, refugees are more likely to swiftly integrate in the labour market. We will use two strategies to disentangle the influence of the economic structures facing refugees from the social resources argument discussed above. First, it is likely that they operate on different geographical scales. Building social contacts depends strongly on meeting opportunities, e.g. via school, church, or sport clubs in the neighbourhood or municipality. Gambaro, Neidhöfer and Spiess (2020) show that female refugees with children in early childhood education and care are better integrated in German society, which they partly attribute to the social contacts ensuing from these provisions. For jobs, however, many people travel outside their municipality. We will therefore take the economic opportunities in the region into account.

Second, we formulate hypotheses on interaction effects. Especially for relatively small refugee groups, social contact with natives may represent a valuable source of information. This argument follows Lazear (1999), who posits that small ethnic minorities have a stronger incentive to form social ties with the ethnic majority and thus are better integrated in the host society. We argue that refugees residing in areas with fewer co-ethnics will be influenced more strongly by natives in the area due to limited contact opportunities with co-ethnics. Such an interaction is not to be expected if the employment rate among natives solely indicates economic opportunities:

H3a: The influence of natives' employment rate on refugees' likelihood of transitioning from social assistance to work weakens as the share of co-ethnics in the area increases.
H3b: The influence of natives' level of income on refugees' likelihood of transitioning from social assistance to work weakens as the share of co-ethnics in the area increases.

The Dutch Asylum Procedure

This section provides a brief overview of the Dutch settlement policy as well as the application procedure preceding refugees' settlement, focusing on the policies effectuated between 1999 and 2009. During this period, the policies remained fairly unchanged.

Upon entry to the Netherlands, an asylum request is first processed at an application centre ('*aanmeldcentrum*') for about 1 week. The aim is to quickly reject invalid claims, in particular regarding persons from countries that are considered safe. If their claim passes this initial check, people are moved to an asylum seeker reception centre[3] ('AZC') to await further screening. These are housing units administered by the Central Agency for the Reception of Asylum Seekers ('COA'), a governmental institute in charge of housing asylum seekers. COA assigns them to an AZC without considering their own preferences (Arnoldus, Dukes and Musterd, 2003). AZC processing time may take 6 months or longer, depending on the amount of asylum claims under review by the Dutch immigration authorities. Especially between 1995 and 2001, this resulted in long waiting periods.[4] Once a claim is approved, the refugee is granted a temporary or permanent residence permit.[5]

An asylum seeker may under specific conditions work while awaiting a decision on the asylum claim. The most important condition is that the asylum processing time has exceeded 6 months. Additionally, the work has to meet the normal working conditions, including wage, for that type of work. The employer must obtain a special permit ('*tewerkstellingsvergunning*') prior to hiring. If these conditions are met, the asylum seeker may work up to 14 weeks—in 2008 extended to 24 weeks—during a 52-week period. A certain amount of the income is subtracted and transferred to COA to cover housing costs. In 2008, the asylum seeker could keep 25 per cent of the income up to a maximum of 185 euros per month and the rest was paid to COA. After being granted a residence permit, these legal restrictions no longer apply.[6]

Upon receiving a residence permit, the refugee must leave the asylum seeker reception centre. Regular accommodation is assigned to the refugee, mainly in the form of rented social housing. All municipalities are required to provide accommodation for a certain number of refugees, depending on their number of inhabitants. COA is responsible for matching refugees to houses, and generally does not take their preferences into account. The agency may apply some 'objective' criteria, such as (i) whether the refugee has a job, (ii) if family members (most notably partner, parent, children, or siblings) reside in the Netherlands, or (iii) whether the refugee requires (or undergoes) medical treatment only offered at a specific hospital. In these instances, housing is sought in proximity to the workplace, family members, or hospital. It is also known that refugees occasionally reject to move to small municipalities, although they are formally not allowed to do so (Dagevos, 2007).[7]

To ensure that our sample of refugees has been placed in regular accommodation exogenously—that is: independent of unobserved characteristics—we exclude some groups from our analytical sample. First, we omit individuals who worked while residing in an asylum-seeker centre. This ensures that current or past employment will not affect the initial placement. Second, we exclude asylum seekers who have stayed in a housing arrangement specifically designed for asylum seekers with close relatives in the Netherlands. In doing so, we minimize the risk that family already present in the Netherlands affects the initial placement in regular housing. We cannot identify the few people who reject a housing offer. This is likely related to the refugees' household situation; for instance, single refugees might be more inclined to reject offers from small municipalities. This will be accounted for by including detailed variables for the household composition in the initial place of residence in our statistical models. It should be noted that refugees' knowledge of the Netherlands is typically limited. We therefore assume that they will not reject housing offers based on their perception of local employment opportunities, and that rejections will not systematically bias our results. We also cannot identify refugees with a serious illness who are placed in the neighbourhood of a hospital. However, since hospitals are more likely placed in cities (where there are likely more co-ethnics and relatively good employment opportunities) and these people are not likely to find employment soon, this works against our hypotheses.

Data and Sample Description

We employ Dutch administrative data[8] that contain longitudinal information on individuals' residence history, household composition, migration history, major source of income, and several socioeconomic and demographic characteristics from 1999 to 2017. These data cover the entire population and allow for the identification of refugees. To zoom in on our group of interest, we apply two main criteria. (i) We select those people who are registered with asylum as their main motivation for migrating to the Netherlands. This information is taken

from an administrative dataset on immigrants with a non-Dutch nationality and their reason for immigrating, which is collected by the Dutch Immigration and Naturalization Service and was linked to the administrative data. (ii) Subsequently, we select refugees that we observe in an AZC. This is made possible by data on the exact address and opening (and closing) year and month of all COA-administered housing.[9] There are two reasons for this selection. By selecting persons observed in an AZC reception centre, we ensure that they have undergone the regular asylum procedure, and we exclude refugees whose first regular housing is influenced by having close relatives in the Netherlands, as these people typically are placed in other types of reception centres. Furthermore, in this way, we can observe the exact timing of people's exit from COA-administered housing and thus pinpoint the first address after having left the reception centre.

In addition to these two selections, we impose some additional sample restrictions. We focus on refugees who are placed in regular accommodation during the period 1999–2009. The upper bound was chosen due to major changes in the asylum application procedure from 2010 and onwards, which affected the placement of refugees. In addition, we confine the analysis to people originating from the 12 largest refugee-sending countries during this period, to ensure sufficient numbers of co-ethnics.[10] These were in descending order: Afghanistan; Iraq; the former Soviet-Union; former Yugoslavia; Somalia; Angola; Iran; Sierra Leone; Sudan; China; Syria; and Turkey. Finally, we focus on people of prime working age, who were aged 25–55 years when they moved out of the AZC. This ensures that we can follow the oldest refugees for up to 10 years before they reach retirement age.

We arrived at our analytical sample in the following way. In our data, 174,194 people had asylum as main reason for immigration from the 12 countries of origin. Of these, 81,964 people immigrated during the period 1996–2009. After accounting for having ever stayed on an address administered by COA and deselecting those residing in a housing arrangement for asylum seekers with close relatives in the Netherlands, 61,592 people remain. When we disregard people that receive a resident permit and leave a COA-address prior to 1999—implying we cannot track their labour market status—51,830 are left. After selecting for working age, 24,166 refugees remain. Of these, 15,811 are observed in an AZC reception centre—meaning they follow a regular application procedure involving exogenous placement in regular housing; 11,936 are left once removing those who move to

regular accommodation after 2009. Finally, we exclude refugees who worked prior to moving to regular housing, who at the time of moving to regular housing were ineligible for social assistance (explained below), or who did not receive social assistance within the first 6 months. This resulted in an analytical sample consisting of 5,483 refugees. Obviously, this is only a small fraction of the entire population of refugees arriving to the Netherlands within this period; but we confine ourselves to this group to ensure that the refugees in question have been exogenously placed.

To assess to what extent the placement actually is exogenous in terms of observed characteristics, we ran several regression analyses. Specifically, we modelled whether there are any systematic differences in the neighbourhood or municipality characteristics by household position, ethnic-origin groups and age, respectively, across the years of placement. The results support our assumption of exogenous placement,[11] as we find nearly no systematic relation of observed personal characteristics with the neighbourhood and municipality characteristics. The differences we do find are mainly attributable to a very low number of refugees for certain subgroups. Note, however, that it is impossible to properly test whether refugees have been completely exogenously placed in regular housing as we assume that the placement is exogenous to *un*observed characteristics—unobservable in the data.

Operationalizations

Response Variable

We use monthly information about peoples' major source of income to identify the transition from social assistance receipt to employment. We define the transition as the moment that an individual's major source of income shifts from social assistance to labour. Because we are interested in the transition from welfare to work, we censor individuals who stop receiving social assistance for other reasons. In practice, this involves individuals who (i) live in a household in which another household member starts working, or (ii) who start receiving retirement benefits. Both (i) and (ii) imply that the household is no longer eligible for social assistance. Social assistance is a means-tested benefit provided to households whose income and assets fall below the Dutch statutory social minimum. As a rule, refugees are automatically enrolled in the social assistance scheme upon being placed in regular housing. Among refugees with granted residence in 2014, 90 per cent received social assistance after 1 year (Statistics Netherlands,

2017). Exceptions to this are refugees who form a household with relatives or other individuals who receive an income above the social minimum. We include only individuals who receive social assistance benefits in the first month they are at risk. Additionally and to allow for administrative delays, we include individuals who are initially registered as 'others no income', if they (i) start receiving social assistance benefits within the first 6 months since moving to regular housing, and (ii) there are no employed household members during this period.

Explanatory Variables

The main explanatory variables are the characteristics of co-ethnics and natives in the first neighbourhood in which refugees live after leaving the AZC, as well as the share of co-ethnics in the neighbourhood. The neighbourhood is measured at two different levels: The four-digit zip code for the characteristics of natives, and the municipality for those of co-ethnics. We base this choice on the assumption that refugees have social contacts with native Dutch people in their immediate surrounding (neighbourhood as defined by four-digit zip code), whereas they are more willing to travel throughout the municipality to meet co-ethnics. Additionally, we need to ensure an adequate number of co-ethnics; an initial check showed this is very low if measured at the level of four-digit zip codes.

We focus on two variables, namely (i) the proportion of employed people defined as receiving their major source of income from the labour market, and (ii) the level of income. For both characteristics, we consider the people in the neighbourhood or municipality aged between 18 and 65 years, excluding any early retirees. People's ethnic origin is based on the country of origin as defined by Statistics Netherlands (including both first- and second-generation immigrants). We calculate the measures for each year on 1 January for the period 1999–2009, and match these with the refugees' first address in regular housing. If a refugee leaves a COA-administered address and moves to regular housing, in e.g. June 1999, the neighbourhood or municipality characteristics as of 1 January 1999 are used. Refugees without any co-ethnics in the municipality are coded at the mean of the employment share in the municipality and included in a separate dummy variable. Thus, we avoid having to drop these cases from the analytical sample. The share of co-ethnics is similarly constructed at the municipality level and measures the proportion of co-ethnics among the 18–65-year-old population.

Level of income is measured as the median gross yearly income in Euros. Specifically, we draw on individuals' primary income that indicates earnings from waged employment and self-employment, thus excluding taxes, social security contributions, social transfers, and other income sources. The variable is aggregated to the neighbourhood or municipality level from individuals' income, excluding people who have no income according to this measure. Substantively, this implies that our measure captures the median yearly income for people active on the labour market. Additionally, some median income levels turn out negative for the co-ethnics, which is due to the combination of (i) very few co-ethnics who (ii) report net losses. These were coded as 0.1. For some refugees, it was not possible to calculate a median neighbourhood income, although some co-ethnics were employed; in this case, they were coded 0.01.[12] Finally, refugees who do not have any co-ethnics living in the municipality are given the average score on this variable and identified by a separate binary variable in the analyses. We include the logged values of the income variables in our analyses. Because data on individual income is only available from 2003 onwards, this variable is not available for the whole sample of refugees from 1999 to 2009. We return to this point in the analytical strategy.

Control Variables

The exogenous placement of refugees in municipalities ensures that individual characteristics that may affect their likelihood to find work, such as their level of education, are not associated with the characteristics of these municipalities. They therefore do not have to be included as control variables.

We account for the individual's position in and the configuration of the *household* because this may be related to the (small) likelihood to refuse placement in a certain municipality. This variable is time-invariant, reflecting the household type during initial housing. We capture the following configurations: (i) child in a household; (ii) single (reference category); (iii) living with partner without children; (iv) living with partner and youngest child below 4 years of age; (v) living with partner and youngest child 4–12 years; (vi) living with partner and youngest child older than 12 years; (vii) single parent with youngest child below 4 years of age; (viii) single parent with youngest child 4–12 years; (ix) single parent with youngest child older than 12 years; and (x) other household.

In addition, we include dummy variables for the *ethnic origin* groups and for the *year* the refugee was placed

in regular housing using 1999 as the reference category. These variables may be related to municipality characteristics, not because of selection of refugees into certain municipalities, but because ethnic groups enter the country in different years with divergent unemployment rates.

We take two characteristics into account that measure the economic opportunities for refugees in the region. These are the *regional unemployment level* and the share of the employed population working in the *primary and secondary sector* (per COROP subregion and year).[13]

Finally, we strengthen the explanatory power of the models by taking into account the individual's *age*. Age is fixed when the person first moves to regular accommodation and measured in number of years. Age has been specified as a time-invariant characteristic in order to avoid conflating it with time at risk (see analytical strategy below).[14]

Analytical Strategy

We employ multilevel linear probability discrete time event-history modelling with robust standard errors to analyse the data. Linear probability models are to be preferred over logistic models when testing interaction effects, and the coefficients can readily be compared across models (Mood, 2010). Specifically, we model the conditional likelihood of transitioning from social assistance to employment. Refugees are considered 'at risk' from the month they first move to regular housing. As we do not explicitly consider subsequent moving behaviour, the estimated influence of the neighbourhood context should be interpreted as intent-to-treat estimates.

To test our hypotheses, we draw on two analytical samples; a *main sample* of refugees placed in regular housing from 1999 to 2009, and an *income sample* of refugees placed in regular housing from 2003 to 2009, when information on income is available in our data. We take account of the fact that some refugees are placed in the same neighbourhood (four-digit postcode) in the same year. Neighbourhoods are nested in COROP regions. Accordingly, we use multilevel modelling with person-months nested in neighbourhood-years nested in COROP-years.[15] Time is measured as months since moving to regular accommodation divided by 12—meaning an increase of 1 equals 1 year at risk—and is modelled using splines. In the main sample, the cut-off points are set at 2, 4, and 7 years, meaning, we allow the conditional effect of time to change three times. For the income sample, we also employ splines but use one cut-off point at 4 years.[16]

As explained above, individuals are censored if they become ineligible for social assistance benefits. Additionally, we censor refugees who have not found work within 10 years after becoming at risk. We do this to ensure that we compare refugees for a relatively equal amount of time—although we can only observe the most recent refugees for 8 years at most, because our data run until 2017. The substantive conclusions regarding the hypotheses do not change if we follow refugees for as long as the data permit. To ease the interpretation of the results, we mean-centre all the continuous variables at their respective sample mean. All models were estimated using Stata 15.

Ideally, we would run separate models for men and women. However, because the number of observed transitions for women is very low, this turned out to be statistically unfeasible. Only 319 (14.2 per cent) of all women made the transition from social assistance to work versus 49.8 per cent of all men (Table 1). We therefore present models on men in the main text and we check whether our results differ when we also include female refugees (see Supplementary material). The main and income samples are quite similar in terms of percentage of men transitioning (35.2 vs. 33.0 per cent), mean months at risk (72.5 vs. 72.8), and the overall hazard rate (0.0049 vs. 0.0045).[17]

Table 2 shows the descriptive statistics for men at the level of person-months. On average, the proportion of co-ethnics in the municipality is 0.003 (i.e. 0.3 per cent). The average population size of a Dutch municipality is about 45,000. In the average Dutch municipality, an average refugee would thus have about 135 co-ethnics. The employment share among co-ethnics is with 28 per cent much lower than that of natives (71 per cent) and the same is true for their income (22,201 vs. 35,431). In general, the income sample is very similar to the main sample, except with respect to ethnic origin, which reflects historical changes in refugee migration flows. Descriptive statistics for women and the total sample can be found in Supplementary material Table SA2.

Results

Main Analyses

We report the results of the analyses for the male main sample in Table 3 and the male income sample in Table 4 (see Supplementary material for full tables). In both tables, the coefficients represent the estimated effect on the probability of transitioning from social benefit receipt to employment in a given month, conditional upon not (yet) having made this transition, for a one-

Table 1. Descriptive statistics of time at risk and hazard rate for men and women in the main sample

	Main sample men	Main sample women	Main sample total
No events (% of individuals)	1,611 (49.80)	319 (14.23)	1,930 (35.24)
Mean time at risk (in months)	63.93	84.96	72.52
Hazard rate (mean)	0.0078	0.0017	0.0049
N (person-months)	206,808	190,405	397,213
N (individuals)	3,235	2,241	5,476

Notes: Mean time at risk reflects the average across individuals in which first month at risk is coded 1, whereas time at risk in Table 2 and in the rest of the analyses is at the level of person-months and first month at risk is coded 0.

Table 2. Descriptive statistics for men in the main and income sample

Variables	Main sample men		Income sample men	
	Mean/Prop. (SD)	(P5, P95)[a]	Mean/Prop. (SD)	(P5, P95)[a]
Enters labour market	0.008		0.007	
Months at risk/12	3.605 (2.635)	(0.25, 8.583)	3.604 (2.624)	(0.25, 8.500)
Prop. employed among co-ethnics (municipality)	0.279 (0.155)	(0, 0.523)	0.291 (0.154)	(0, 0.522)
Prop. employed among natives (neighbourhood)	0.711 (0.066)	(0.592, 0.797)	0.719 (0.065)	(0.592, 0.802)
Median yearly income among co-ethnics (municipality)			22,200.63 (9,954.60)	(0.01, 33,311)
Median yearly income among natives (neighbourhood)			35,431.08 (4,237.12)	(28,935, 42,709)
No co-ethnics in municipality (ref. > 0 co-ethnics)	0.018		0.021	
Share of co-ethnics (municipality)	0.003 (0.003)	(0.000, 0.007)	0.003 (0.003)	(0.000, 0.007)
Age/10	3.722 (0.831)	(2.6, 5.2)	3.675 (0.845)	(2.5, 5.2)
Household position (categorical)				
Child in household	0.006		0.004	
Single (ref.)	0.484		0.566	
Partner without children	0.075		0.074	
Partner with youngest child < 4	0.165		0.122	
Partner with youngest child 4–12 years	0.184		0.143	
Partner with youngest child 12 < years	0.062		0.065	
Single parent with youngest child < 4 years	0.000		0.000	
Single parent with youngest child 4–12 years	0.007		0.007	
Single parent with youngest child 12 < years	0.008		0.008	
Other	0.009		0.011	
Ethnic-origin (categorical)				
Iraq (ref.)	0.352		0.439	
Afghanistan	0.208		0.075	
Former Yugoslavia	0.061		0.038	
Former Soviet-Union	0.054		0.060	
Somalia	0.199		0.282	
Angola	0.005		0.005	
Iran	0.047		0.047	
Sierra Leona	0.014		0.007	
(former) Sudan	0.034		0.020	
China	0.003		0.005	
Syria	0.020		0.019	

(continued)

Table 2. (Continued)

Variables	Main sample men		Income sample men	
	Mean/Prop. (SD)	(P5, P95)[a]	Mean/Prop. (SD)	(P5, P95)[a]
Turkey	0.003		0.003	
Year of placement (categorical)				
1999 (ref. in main sample)	0.065			
2000	0.051			
2001	0.098			
2002	0.106			
2003 (ref. in income sample)	0.077		0.112	
2004	0.038		0.056	
2005	0.056		0.083	
2006	0.063		0.093	
2007	0.102		0.151	
2008	0.141		0.208	
2009	0.202		0.296	
Percentage unemployed (COROP)	4.154 (1.446)	(2.0, 6.8)	4.652 (1.319)	(2.7, 7.1)
Percentage employed in primary and secondary sector (COROP)	21.200 (6.677)	(8.1, 31.2)	20.711 (6.612)	(7.7, 30.3)
N (person-months)	206,808		139,759	
N (individuals)	3,235		2,176	
N (neighbourhood × year)	2,674		1,777	
N (COROP × year)	425		269	

Notes: Descriptive statistics calculated at the level of person-months. Because the income data are only available from 2003 and onwards, the income sample covers the period 2003–2009 only. Proportions are depicted for categorical variables and may due to rounding not add up to exactly 1.

[a]For privacy reasons, Statistics Netherlands does not allow the release of minimum and maximum values, particularly for income. Instead, we show the 5th (P5) and 95th (P95) percentiles for the continuous variables.

unit change in the variable. Model 1 (Table 3) shows the likelihood for male refugees to find work as a function of time. The likelihood increases in the two first years, remain unchanged between 2 and 4 years, and then decreases when not (yet) having found work after 4 years. Without controls (model 2), the effect of the proportion of employed co-ethnics in the municipality is not significant ($b = 0.0025$; $P > 0.05$, two-sided). This effect increases and becomes significant when we take into account the year of moving into regular housing, household position, ethnic background, and age in model 3 ($b = 0.0040$; $P < 0.05$, two-sided). However, when we include the indicators of economic opportunities in the region, it becomes clear that the effect of the proportion of employed co-ethnics in the municipality is spurious. Male refugees are more likely to find work in regions with low unemployment. How many co-ethnics in their municipality are employed does not contribute to this likelihood. The findings are not in line with hypothesis 1a.

Model 2 shows an insignificant relationship between the share of employed natives and the conditional likelihood of transitioning to employment. When control variables are added (model 3), the results indicate that the higher the share of employed natives, the higher the conditional likelihood of male refugees' transitioning into employment ($b = 0.0139$; $P < 0.005$, two-sided).[18] Similar to the effect of having employed co-ethnics in the municipality, the effect of employed natives in the neighbourhood is partly due to the economic opportunities in the region. However, it remains significant, even after taking these opportunities into account (model 4: $b = 0.0090$; $P < 0.05$, two-sided). The estimate suggests that a male refugee placed in a neighbourhood where the proportion of employed natives is high (95th percentile) relative to a neighbourhood where it is low (5th percentile), has a 0.0018 higher conditional likelihood of transitioning into employment in each month, which amounts to a relative increase of 21.7 per cent.[19] This is in line with hypothesis 2a.

We expected that the effect of the share of employed natives would depend on the share of co-ethnics in the municipality. Model 5 indicates that this is not the case ($b = -0.9693$; $P > 0.05$, two-sided). The main effect of the share of co-ethnics in the municipality is not significant either. An additional analysis revealed that this also

Table 3. Analyses on main sample, men only. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear-probability, discrete-time event-history modelling with robust standard errors. Z-statistics between parentheses

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	0.0047 (7.59)**	0.0047 (7.53)**	0.0100 (7.71)**	0.0094 (6.84)**	0.0094 (6.80)**
Months at risk/12 (<2 years)	0.0032 (6.54)**	0.0033 (6.56)**	0.0034 (6.77)**	0.0034 (6.76)**	0.0034 (6.76)**
Months at risk/12 (2–4 years)	0.0005 (1.11)	0.0005 (1.13)	0.0006 (1.48)	0.0006 (1.45)	0.0006 (1.46)
Months at risk/12 (4–7 years)	−0.0009 (−2.72)*	−0.0009 (−2.71)*	−0.0007 (−2.10)*	−0.0007 (−2.12)*	−0.0007 (−2.10)*
Months at risk/12 (≥7 years)	−0.0010 (−2.53)*	−0.0010 (−2.54)*	−0.0010 (−2.50)*	−0.0010 (−2.50)*	−0.0010 (−2.50)*
Explanatory variables					
Prop. employed among co-ethnics (munic.)[a]		0.0025 (1.39)	0.0040 (2.18)*	0.0032 (1.68)	0.0032 (1.69)
Prop. employed among natives (neighb.)[a]		0.0008 (0.24)	0.0139 (3.73)**	0.0090 (2.26)*	0.0091 (2.19)*
No co-ethnics in munic. (ref. >0 co-ethnics)		0.0002 (0.10)	−0.0004 (−0.26)	−0.0007 (−0.42)	−0.0007 (−0.48)
Share of co-ethnics (in municipality)[a]					−0.0855 (−1.05)
Prop. employed among natives[a] × Share of co-ethnics[a]					−0.9693 (−0.95)
Control variables:					
% unemployed (COROP)[a]				−0.0008 (−3.24)**	−0.0008 (−3.14)**
% employed in primary and secondary sector (COROP)[a]				0.0000 (1.06)	0.0000 (0.99)
Age/10[a]			−0.0054 (−17.65)**	−0.0054 (−17.70)**	−0.0054 (−17.70)**
Household-position dummies	NO	NO	YES	YES	YES
Ethnic-origin dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	YES	YES	YES
Var (COROP × year)	0.0000**	0.0000**	0.0000**	0.0000**	0.0000**
Var (neighbourhood × year)	0.0000**	0.0000**	0.0000**	0.0000**	0.0000**
Var (residual)	0.0077**	0.0077**	0.0077**	0.0077**	0.0077**
Log pseudolikelihood	209,720	209,721	209,996	210,003	210,003
BIC	−419,342	−419,308	−419,478	−419,467	−419,444
N (person-months)			206,808		
N (neighbourhood × year)			2,674		
N (COROP × year)			425		

*P < 0.05 and **P < 0.005, two-sided.

[a]Mean-centred at mean of main sample. See Supplementary Material Table A3 for the effects of all categorical variables.

Table 4. Analyses on income sample, men only. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear-probability, discrete-time event-history modelling with robust standard errors. Z-statistics between parentheses

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	0.0063 (10.05)**	0.0062 (9.54)**	0.0069 (6.63)**	0.0075 (7.19)**	0.0075 (7.09)**
Months at risk/12 ($<$ 4 years)	0.0016 (6.34)**	0.0016 (6.29)**	0.0017 (6.77)**	0.0017 (6.77)**	0.0017 (6.78)**
Months at risk/12 ($\geq$ 4 years)	−0.0013 (−6.74)**	−0.0013 (−6.73)**	−0.0012 (−6.13)**	−0.0012 (−6.17)**	−0.0012 (−6.16)**
Explanatory variables:					
Prop. employed among co-ethnics (munic.)[a]		−0.0005 (−0.17)	0.0046 (1.51)	0.0030 (0.99)	0.0029 (0.95)
Prop. employed among natives (neighb.)[a]		0.0125 (2.60)*	0.0202 (4.07)**	0.0109 (2.09)*	0.0111 (2.07)*
Median income among co-ethnics (munic.)[a,b]		0.0001 (0.95)	−0.0000 (−0.17)	−0.0000 (−0.24)	−0.0000 (−0.15)
Median income among natives (neighb.)[a,b]		−0.0071 (−2.24)*	−0.0072 (−2.21)*	−0.0049 (−1.29)	−0.0048 (−1.24)
No co-ethnics in munic. (ref. $>$ 0 co-ethnics)		0.0005 (0.24)	−0.0009 (−0.47)	−0.0012 (−0.62)	−0.0012 (−0.64)
Share of co-ethnics (in municipality)[a]					−0.0556 (−0.58)
Median income among natives[a,b] x Share of co-ethnics[a]					−0.8882 (−0.70)
Control variables:					
Per cent unemployed (COROP)[a]				−0.0012 (−4.53)**	−0.0012 (−4.43)**
Per cent employed in primary and secondary sector (COROP)[a]				0.0000 (0.86)	0.0000 (0.86)
Age/10[a]			−0.0050 (−13.73)**	−0.0050 (−13.79)**	−0.0050 (−13.78)**
Household-position dummies	NO	NO	YES	YES	YES
Ethnic-origin dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	YES	YES	YES
Var (COROP × year)	0.0000**	0.0000**	0.0000**	0.0000**	0.0000**
Var (neighbourhood × year)	0.0000**	0.0000**	0.0000**	0.0000**	0.0000**
Var (residual)	0.0071**	0.0071**	0.0071**	0.0071**	0.0071**
Log pseudolikelihood	146,760	146,763	146,911	146,920	146,920
BIC	−293,448	−293,396	−293,371	−293,366	−293,343
N (person-months)			139,759		
N (neighbourhood × year)			1,777		
N (COROP × year)			269		

*$P < 0.05$ and **$P < 0.005$, two-sided.

[a]Mean-centred at mean of income sample.

[b]Natural logarithm of variable included. See Supplementary Material Table A4 for the effects of all categorical variables.

applies when the interaction term is not included in the model (results available upon request). Moreover, the indicator for having no co-ethnics in the municipality is insignificant in all models, which provides further evidence of a negligible effect of ethnic composition on transitioning to work. Hence, the results provide no support for hypothesis 3a. The effect of the share of employed natives does not depend on the share of co-ethnics in the population.

Analyses on the Income Sample

The outcomes of the analyses on the income sample are shown in Table 4. We include the employment share variables together with the income variables, to assess whether median income has an effect over and above that of employment of the same groups.

We expected that a higher level of income among co-ethnics would make refugees more likely to transition from social assistance to work. Model 2 in Table 4 suggests that this is not the case for male refugees: The median income among co-ethnics does not affect the conditional likelihood of transitioning from social assistance to work ($b = 0.0001$; $P > 0.05$, two-sided). This is also true when control variables are added in models 3 and 4, and implies that male refugees do not fare worse nor better because of the median level of income among co-ethnics in the municipality they are placed in. Thus, the results are not in line with our theoretical expectation (H1b).

There is a significant negative effect of the median income level among natives on male refugees' conditional likelihood of transitioning into work in model 2 in Table 4 ($b = -0.0071$; $P < 0.05$, two-sided). Counter to our hypothesis (H2b), this suggests that refugees placed in a neighbourhood in which natives have a higher median income, have a *lower* conditional likelihood of transitioning into work. When we take into account possible cofounding variables in model 3, the effect remains the same. However, when we add the economic opportunities in the region in model 4, the effect of the median income level among natives on male refugees' conditional likelihood of transitioning into work appears to be spurious.

Next, we test whether the effect of median income among natives depends on the share of co-ethnics in the municipality. Model 5 in Table 4 shows that the effect of native's income does not vary by the share of co-ethnics in the municipality ($b = -0.8882$; $P > 0.05$, two-sided). This runs counter to hypothesis 3b.

Finally, we briefly comment on the effect of employment share among natives. The estimated effects in

Table 4 are somewhat larger compared to the estimates in Table 3. Considering the (insignificant) negative effect of median income among natives, this suggests that the effect found on the main sample in Table 3 is slightly suppressed—in the sense that the effect of the employment share among natives is biased downward because it partially captures the negative effect that median income among natives has on the conditional transition probabilities. In sum, we observe that the point estimate for the employment share among natives is in the same direction as in Table 3, and thus in line with its hypothesized effect (H2a).

Results for the Total Population

Although the low number of transitions to work for women does not allow separate analyses for women, we can compare the results for the total sample with those for men only. The results for the total sample can be found in supplementary material Tables SA5 and SA6 in the. In general, the effects of our indicators of the neighbourhood context are smaller in the overall analyses compared to the analyses for men only. The proportion of employed natives in the neighbourhood significantly affected the likelihood that male refugees made the transition out of social welfare, but does not have a significant effect if women are included as well. Note that also the effect of unemployment in the wider region is only half as strong in the total sample as it is in the male sample. This indicates that for female refugees, the social and economic environment is less important for their likelihood to leave social assistance than for male refugees.

Discussion and Conclusions

In many European countries, the labour market integration of refugees is a key element of the policy debate. This study aimed to shed light on the importance of the neighbourhood context in which refugees are placed for their future employment opportunities. We have drawn on a natural quasi-experiment in the Dutch housing policy for refugees—asylum migrants who were granted asylum—regarding their first regular accommodation. This allowed us to obtain intent-to-treat estimates of contextual effects that are not biased by self-selection. We focused on the employment share and median income in the neighbourhood among co-ethnics and natives, and expected that in areas where these are high, refugees would have a higher likelihood of transitioning from welfare to work. Additionally, we presumed that the influence of the employment share and median

income among natives would depend on the concentration of co-ethnics in the area. These arguments were tested using Dutch longitudinal administrative data and multilevel linear probability, discrete time event-history modelling.

The findings suggest that male refugees placed in areas in which natives are employed more often transition into work. This is in line with our expectations (H2a) and corroborates findings from previous research, which indicate that a higher employment share among neighbours leads to more favourable labour market outcomes (e.g. Damm, 2014). The outcome points to the role of employed others as an important social resource for facilitating refugees' first step on the labour market. Contrary to our expectations, however, we do not find any evidence that co-ethnics in the municipality play a substantial role. Their employment share does not affect refugees' likelihood to make the transition from social assistance to work (H1a). One possible explanation could be that refugees contact co-ethnics outside the municipality. In that case, co-ethnics may be a source of social capital, but this is not connected to the neighbourhood. However, these findings are also in line with the idea that it is particularly bridging social capital, which spans socioeconomic or ethnic boundaries, that offers access to unique information (Putnam, 2000).

We used two strategies to increase the likelihood that we assess effects of social capital in the neighbourhood and not effects of the economic opportunities in the region. As a first strategy, we took the economic opportunities in the larger region into account. Since many people do not work in their municipality of residence, but also do not commute very far, the regional unemployment rate is probably a better indicator of labour market opportunities than the local (un)employment rate. Indeed, we find that refugees who are placed in a region with favourable employment opportunities are more likely to make the transition to work. Besides this regional effect, it also helps if more natives in their neighbourhood work. This supports the idea that not only economic opportunities, but also social capital may be at work. As a second strategy, we formulated the hypothesis that native neighbours would be a more important source of work-related information for refugees with few co-ethnics around. However, we did not find this (refuting H3a). This is in line with the lack of a main effect of employment among co-ethnics.

There is no evidence that refugees placed in an area with a higher level of income among co-ethnics or natives are better able to enter the labour market. This runs counter to our initial hypotheses (H1b and H2b),

but also to the outcomes of previous studies that generally find that higher income levels in the area of residence tend to advance refugees' labour market outcomes (Edin, Fredriksson and Åslund, 2003; Damm, 2009, 2014). One possible explanation is that higher-income contacts may not be the most relevant ones for refugees. Labour market disadvantages related to language proficiency and recognition of foreign educational credentials (de Vroome and van Tubergen, 2010), especially, impede refugees' access to the higher end of the labour market, and higher-income contacts are not useful in mitigating those (Lin, 1999). However, most refugees initially have to turn to lower-status jobs, and for those type of jobs, high-income neighbours may not be much of an asset. In other words, refugees need their neighbours to be employed, but not per se in high-income jobs. Studies show that immigrants may profit from knowing managers and entrepreneurs because they are likely to hire co-ethnics (e.g. den Butter, Masurel and Mosch, 2004). Our findings suggest that these are working in small firms and do not have very high incomes.

Finally, it seems that social capital in the neighbourhood and economic opportunities in the region are less important in facilitating the transition from social assistance to work for women than for men. Our evidence for this conclusion is not very strong, since we only observe few such transitions for women and therefore could not analyse female refugees separately. Possible explanations for such a gender difference could be that unskilled work performed by women (e.g. cleaning in private households) is available everywhere and is not found through contacts with employed others. Alternatively, it may be the case that male refugees from traditional societies are more in contact with 'strangers' than female refugees.

A possible limitation of the present study is its focus on refugees of prime-working age (25–55 years old). Future research could expand on this, by investigating the role of the social context for younger refugees. One might expect the initial social context to be more important for younger refugees, due to their limited previous work experience.

Our data do not allow us to investigate whether the refugees really establish contacts with people in their neighbourhood. Neither do we know whether these contacts provide the refugees with useful information. However, our findings suggest that future studies should not neglect the potential role of native neighbours in helping refugees finding their way on the labour market. It is also worthwhile to investigate to what extent contacts with co-ethnics living further away builds useful social capital.

We have investigated refugees who in principle have no say in the location of their first regular housing. We argue that unobserved heterogeneity stemming from self-selection into these initial contexts cannot bias our estimated contextual effects. The effects of the neighbourhood characteristics can therefore be interpreted as the average influence of being exposed to these social contexts, irrespective of subsequent residential mobility. Because it takes time to establish social contacts, it is reasonable to assume that refugees who remain in their first neighbourhood will be more affected by this context. However, a problem related to assessing the strength of the area characteristics by time of exposure to the initial context, is that length of stay and subsequent residential moving behaviour are very likely subject to self-selection: Refugees' perceived chances on the labour market could affect their choice of moving. Hence, if one were to include length of stay in the analyses, one would also introduce selection bias. Therefore, the effects we find of the initial, exogenous placement should be interpreted as 'intent-to-treat' estimates, as actual 'treatment' would depend on the length of residence. Our approach is nevertheless warranted because initial placement is malleable by policy, whereas later residential behaviour is not.

Our results offer a somewhat mixed picture. The employment share among natives is important in facilitating refugees' transition from welfare to work, whereas the median level of income in the area is not important. However, the findings of this study align with recent changes in policies concerning the initial placement of refugees in the Netherlands (Gerritsen, Kattenberg and Vermeulen, 2018). These aim to match refugees to a specific region or municipality based on their predicted compatibility and opportunities on the labour market.

Notes

1   The person is technically an asylum seeker until the asylum claim has been approved. Because we focus on refugees who have 'successfully' undergone the asylum application procedure, we use the term refugee throughout the text.
2   The quality of the neighbourhood means in this case level of income among the co-ethnics in the neighbourhood, and percentage employed among the co-ethnics.
3   Periodically, the AZCs have been supplemented due to housing shortages by so-called supplemental reception centres 'AVO'. We will in the following

refer to AZC, but this also includes these equivalent housing arrangements.
4   It has been shown that a lengthy asylum procedure negatively affects refugees' later employment (e.g. Hainmueller, Hangartner and Lawrence, 2016). However, to the best of our knowledge, the length of the procedure does not affect subsequent placement in regular housing.
5   A residence permit is typically always first granted on a temporary basis. After up to 5 years, the refugee may apply for a permanent residence permit.
6   If a refugee—after being granted a residence permit—has found work and still resides at an AZC, the refugee will receive reduced or no social assistance.
7   According to personal information from the COA this is very rare.
8   All results are based on authors' calculations using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. Further information: microdata@cbs.nl.
9   We received this information from Mark Kattenberg (CPB) and COA, and linked it to the administrative data.
10   To determine the largest refugee-sending countries, we used publicly available data on the national origin of non-Dutch immigrants with asylum as main motive (Statistics Netherlands, 2018).
11   Unfortunately, we cannot show these results because certain subgroups contain few refugees, which violates the CBS microdata privacy rules regarding identifiability and degrees of freedom in statistical analyses.
12   The cases are set at a low but non-zero value to enable taking the log of income in the neighbourhood. In a robustness analysis, we ran the analyses without the cases set at 0.01 (244 and 150 individuals from the total sample and men only sample, respectively). The substantive findings remain unchanged in this robustness analysis.
13   There are 43 Dutch COROP areas (largely equivalent to NUTS-3 regions). These have been defined in 1970 using commuting patterns and are commonly used to describe regional labour markets. The data are from CBS Statline.
14   For the same reason, refugees' level of education could have been included, but in the data information on this variable is incomplete.
15   Municipalities are not distinguished as a separate level. The presence of co-ethnics in the municipality varies between ethnic groups and is therefore not a

variable at the municipality level. As argued by Allison (2014), it is not necessary to account for the fact that each individual contributes multiple observations—months in our case—as long as each individual only contributes one event (or no event at all).

16 These cut-off points roughly correspond to the 25th, 50th, and 75th percentile of the person-month distribution of the observed cases. In the income sample, the 4-year cut-off point roughly corresponds to the 55th percentile of the observed person-months.

17 See Supplementary Material Table SA1 for the descriptive statistics of the income sample.

18 To ascertain why the estimated effect increases, we added the control variables to the model in various ways: (i) one by one; (ii) removing only one; and (iii) including all different combinations (not shown). It turns out that the negative relationship in part is due to variation over time, and in part from the combination of year and ethnic-origin groups or household-position. This pattern would be consistent with historical migration flows that overlap with fluctuations in the general economy.

19 These predictions are derived by first calculating the $\hat{y}_{95th}$ = intercept + b × (95th percentile-mean); second calculating the $\hat{y}_{5th}$ = intercept + b × (5th percentile-mean); third, taking $\hat{y}_{95th}$–$\hat{y}_{5th}$ to obtain the absolute difference and $(\hat{y}_{95th}/\hat{y}_{5th})$ − 1 × 100 to obtain the relative difference in percent. See Table 2 for descriptive statistics.

Supplementary Data

Supplementary data are available at *ESR* online.

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