

Port-of-Entry Neighborhood and Its Effects on the Economic Success of Refugees in Sweden

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Abstract

We investigate the degree to which the ethnic group composition of “port-of-entry neighborhood” (PoE), the first permanent settlement after immigration, affects the employment prospects of refugees in Sweden during the subsequent 10 years. We use panel data on working-age adults from Iran, Iraq, and Somalia immigrating into Sweden from 1995 to 2004. We control for initial individual and labor market characteristics, use instrumental variable regression to avoid bias from geographic selection, and stratify models by gender and co-ethnic employment and education rates within the neighborhood. We find that the impact of co-ethnic neighbors in the PoE varies dramatically by gender.

Keywords

neighborhood effects, refugee migration, co-ethnic clusters, resettlement policy

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Introduction

In 2014 and 2015, unprecedented numbers of asylum seekers came to Sweden. In Spring 2016, nearly 180,000 of these asylum seekers — predominantly accommodated in rural Sweden — waited for a decision on their permission-of-stay application, and the integration issue dominated the country's political and media discourses¹ (Ekengren Oscarsson and Bergström 2017). The geography of refugee settlement is key in these debates, primarily due to the high level of ethnic residential segregation experienced by earlier refugee groups and the anticipated negative effects of further minority clustering when asylum is granted to the many waiting for such a decision. Whether the current rural-dominated settlement pattern will persist is unclear but highly unlikely given the overall integration aim of getting as many newcomers as possible rapidly into employment (Andersson 2017).

The issue of immigrant opportunities in Sweden was thrust into the world's attention in May 2013 when unprecedented intensities of arson and civil disorder wracked several immigrant-dense neighborhoods in the outskirts of Stockholm. Though covered in sensational fashion, these events belie the fact that public concerns over the economic performance of immigrants, especially refugees, have been longstanding in Sweden and indeed, across Western Europe (Musterd and Ostendorf 1996; SOU 1996; Andersson 1999; Phillips 2010). As early as 1985, in fact, Swedish policymakers, like those in other European countries such as the United Kingdom and the Netherlands, adopted strategies requiring the geographic dispersal of new refugee arrivals in an effort to avoid the ethnic segregation widely thought to limit their opportunities (Andersson and Solid 2003; Robinson, Andersson, and Musterd 2003; Andersson, Bråmås, and Holmqvist 2010).

Social scientists have made major contributions to documenting how immigrants have fared economically in Sweden (e.g., Ekberg 1994; Scott 1999; Arai and Vilhelmsson 2001; Le Grand and Szulkin 2002; Lundh et al. 2002; Nekby 2002; Vilhelmsson 2002; Hammarstedt 2003; Rooth and Ekberg 2003; Rosholm, Scott, and Husted 2006; Behtoui 2008; Andersson 2015) and quantifying the degree to which aspects of their residential environments — particularly income, employment, educational, and ethnic composition — affect their economic performance (e.g., Edin, Fredricksson, and Åslund 2003; Grönqvist 2006; Musterd et al. 2008; Urban 2009; Andersson, Musterd, and Galster 2014). Generally, residential environments with socioeconomically “stronger” residents appear to provide positive spillovers for immigrants who have a “weaker” position themselves.

Our paper extends this line of policy-relevant scholarship by focusing for the first time on refugees and the impacts of the neighborhoods in which they initially settled permanently after their temporary accommodations in Sweden, what we call

¹ <https://www.dn.se/nyheter/politik/sverige-gar-till-val-med-en-helt-ny-agenda/?forceScript=1&variantType=large>.

port-of-entry (PoE) neighborhoods, on their employment prospects. These initial exposures to residential environments in an advanced Western nation may be especially formative for refugees from Third World countries who emigrate involuntarily. We investigate the degree to which the PoE neighborhood's co-ethnic composition affects employment prospects of adult refugees from Iran, Iraq, and Somalia during their subsequent 10 years in Sweden and the degree to which these impacts vary by composition of refugees (gender) and context (share of co-ethnics who are employed and highly educated). Our aim is to provide guidance for refugee resettlement policymakers about the consequences of refugees settling within concentrations of those from the same national background (*co-ethnics* hereafter).

Our paper proceeds as follows. We begin by briefly summarizing the evolution of Swedish refugee resettlement policy to demonstrate our investigation's relevance for contemporary public debates and establish a context for the particular choices we have made for refugee cohorts to analyze. We continue with a review of the theoretical and empirical literature undergirding our research, emphasizing the importance of testing for compositional and contextual impacts of co-ethnic concentrations. Based on this review, we introduce our empirical strategy and modeling framework for predicting our two outcomes of interest: employment five years after settling and the number of years employed within 10 years after settling. We employ instrumental variables to overcome bias due to potential geographic selection effects caused by unobserved refugee characteristics and stratify models by gender and co-ethnic neighbor employment and education to test for compositional and contextual effects. Following this, we present our data sources from a full coverage, longitudinal, and individual database (GeoSweden register files); justify our focus on Iranian, Iraqi, and Somali refugees; and explain how we operationalize neighborhoods in our study. Our results indicate that the impact on refugees of co-ethnic neighbors in the PoE varies dramatically by gender and the context provided by these neighbors. Female refugees' future employment prospects are harmed by co-ethnic PoE concentrations unless these neighbors rank in the highest quartile of employment. Male refugees, by contrast, appear not to be affected by co-ethnic PoE concentrations regardless of context. We interpret and discuss our findings in light of current scholarly and public policy debates and in closing offer conclusions, caveats, and suggestions for further research.

Background: Immigration and Refugee Dispersal Policies in Sweden²

In terms of both absolute numbers and per capita rates, Sweden has been a major refugee resettlement country in Europe for several decades (Andersson and Solid

²If otherwise not indicated by references, this brief policy background draws in particular on Andersson and Solid (2003).

2003; Andersson 2015). At the end of 2015, 17 percent of the country's 9.8 million inhabitants were foreign-born, and close to 1.1 million were born outside Europe.³ During the first major wave of post–World War II refugees (1974–1984), there was no explicit policy regarding their geographic placement, producing a pattern of distinctive ethnic concentrations in the outskirts of metropolitan areas that, ironically, often offered limited economic opportunities. From 1984 through mid-1994, the official response was a refugee dispersal policy, under which the Swedish Board of Immigration recruited municipalities with few immigrants and good economic prospects to sponsor, house, and orient a contractually agreed on number of refugees. This ideal of “refugee dispersal to opportunity-rich areas” was soon abandoned, however, as the volume of refugees exceeded all expectations due to wars and civil strife in Eritrea, Iran, Iraq, Lebanon, Somalia, Turkey, and the former Yugoslavia. Though the number of Swedish municipalities taking part in the local reception strategy sharply escalated annually, the time required for asylum cases to be settled grew ever longer, and the number of centers temporarily housing refugees increased steadily. By the early 1990s, the rise of anti-immigrant political parties, a neo-liberal ascendancy in parliament, and a severe economic downturn conspired to create a new policy regime. Beginning July 1, 1994, refugees were entitled to arrange their own accommodations, thereby avoiding long waits at refugee camps and often allowing them to settle near friends and family. More than half of all refugees chose this option, wildly exceeding official expectations.

There is little doubt that the compulsory dispersal policy led to a change in the residential distribution of post-1984 refugees across Sweden (Andersson and Solid 2003; Andersson 2012). Almost all municipalities in southern Sweden and many in the northern part had near or above municipal mean values for refugee immigrants per capita. Although secondary migration tended to reconcentrate these initial waves of refugees as the years passed,⁴ they remained much less concentrated than those refugees arriving under the post-1994, noncompulsory dispersal regime. The net effect of primary and secondary moves for this latter group was a notable concentration in eight Swedish municipalities, where refugees constituted over 5 percent of the population by 2000.

Several officially sanctioned evaluations proved highly critical of the post-1994 policy's limited impact on refugees' economic and social integration into Sweden, measured by a variety of indicators (SOU 1996; Statens Invandrarverk 1997; Hammarstedt 2002). This failure was primarily linked to refugees' residential

³http://www.statistikdatabasen.scb.se/pxweb/en/ssd/START__BE__BE0101__BE0101E/?rxid=0a02c052-152d-4c0d-b4b2-4555765ec22c.

⁴About half the refugees entering Sweden in a particular year leave their original placement municipality within five years, a figure that has remained roughly constant since 1980 (Andersson and Solid 2003), indicating that placement policy per se might have limited effects on refugees' geographical mobility (Andersson 2012).

concentration, so combating segregation was seen as a top priority (Andersson 1999). Today, the programmatic ambition continues to be distributing refugees more evenly over the country, and the dramatic events during Fall 2015⁵ made the Swedish government act promptly. Sweden enacted new tough legislation regarding border controls and asylum rights and has now decided to force all municipalities to once again take part in the settlement of those refugees granted permission to stay. We therefore currently see a return of the Sweden-wide policy of refugee settlement, reopening the tricky question of in which spatial environments (PoE neighborhoods, municipalities, labor market area) conditions for labor market integration are most favorable.⁶

Given this longstanding geographic emphasis in Swedish public policy toward refugees, the importance of rigorously exploring the consequences of residential environments on refugees' prospects for economic success are manifest. Our research therefore addresses the following questions for three of Sweden's most numerous refugee groups:

To what degree does the co-ethnic composition of the PoE neighborhood affect employment opportunities of working-age refugees in Sweden during the subsequent 10 years?

To what degree do these impacts depend on the composition of refugees (gender) and context (share of co-ethnic neighbors who are employed and highly educated)?

Literature Review: Effects of Neighborhood Ethnic Concentrations on Immigrants⁷

There are a number of theoretical arguments about how co-ethnic clustering might affect immigrants' economic prospects that are relevant to the Swedish context, and they differ in their conclusions about whether such a pattern is advantageous.

⁵For Sweden, what was typically labeled *the European Refugee Crisis* meant an increase in the number of asylum seekers from 1,000 to 2,000 per week during January through July 2015 to 7,000 in mid-September and over 10,000 by late October 2015 (<https://www.migrationsverket.se/Om-Migrationsverket/Statistik.html>).

⁶Some researchers argue that dispersing refugees more evenly within metropolitan regions is probably an even more important priority (Holmqvist 2009; Andersson, BråmÅ, and Holmqvist 2010).

⁷Four other theories about the determinants of immigrants' economic success can be identified — cultural assimilation, human capital, globalization and economic restructuring, and ethnic enterprise. These, however, place no significance on the neighborhood context in which immigrants find themselves and therefore will not be reviewed here. For such theories, see Portes (1995, 1996). For a broader review of the various mechanisms by which neighborhoods can affect residents, see Galster (2012).

Several mechanisms imply negative consequences. Immigrant-dense areas may, for example, enhance the potential for place-based stigmatization of residents (Wacquant 1993; Hastings and Dean 2003; Permentier 2009). Such areas may possess fewer “bridging” networks that link their residents to the mainstream economy (Blasius and Friedrichs 2007; van der Laan Bouma-Doff 2007; Vervoort 2011). If, in the extreme, co-ethnic concentrations can completely serve all social and institutional needs, new immigrants may have less motivation to develop host country language and other cultural skills, which may hinder them in gaining stronger economic positions (Massey and Denton 1987; Lazear 1999).

Despite these arguments to the contrary, there are also theoretical reasons why co-ethnic residential concentrations may benefit immigrants economically. Immigrant concentrations may pay dividends for entrepreneurs and laborers alike through access to dense ethnic networks that provide financial resources, employment information, and niche markets for specialized goods and services (Wilson and Portes 1980; Portes and Bach 1985; Light and Rosenstein 1995; Kloosterman and Van der Leun 1999; Waldinger and Lichter 2003). Still other causal mechanisms hold more ambiguous implications for immigrants’ economic prospects. A well-known process explaining how neighbors may influence residents is socialization, through which collective norms and values are inculcated (Galster 2012). Co-ethnic neighbors may play a more powerful role for immigrants in this regard than for natives given better congruence in language and cultural backgrounds. Whether these normative outcomes prove economically beneficial to immigrants will depend on the particular values being transmitted concerning education, work, fertility, and welfare (Bertrand, Luttmer, and Mullainathan 2000).

Theory suggests not only that different potential neighborhood mechanisms imparting positive and negative consequences for immigrants are at work but also that the strength of alternative mechanisms may vary depending on the composition of immigrants (Sharkey and Faber 2014). Of particular relevance here is gender. We would expect that immigrant women, especially those with children, would have more of their routine activity spaces within the neighborhood (Pinkster 2008), all else equal, and thus be more intensely subjected to all the aforementioned potential neighborhood effects operating there. As argued by Pinkster (2008), compared to immigrant men, immigrant women may have more limited bridging social networks and access to capital within co-ethnic enclaves. Collective social control exerted by co-ethnic neighbors in extreme circumstances may also limit female immigrants’ ability and willingness to seek employment, particularly outside the neighborhood (Pinkster 2008). This may be especially true for immigrant clusters where more traditional, patriarchal norms affect women’s ability to work, particularly if they have children.

Finally, there are strong theoretical reasons to believe that the impact of co-ethnic residential clustering will be contingent on the particular attributes of the immigrants involved. Borjas (1995, 1998) posits that geographically concentrating members of an immigrant group will expose them to greater “ethnic capital” externalities emanating from the collective and that this force will play a more dominant role in

shaping immigrants' economic destinies. Such externalities could prove positive if these co-ethnic neighbors possess superior educational levels, native language competencies, employment levels, and so on but could also prove negative if they do not. Analogous arguments can be made regarding collective norms about work or the value of education (Dryler 2001).

Unfortunately, only a limited number of studies have empirically investigated the relationships between adult immigrants' socioeconomic outcomes and their neighborhoods' co-ethnic concentrations. These studies often differ in their findings due not only to differences in national contexts and measures of economic outcomes, as well as neighborhood contexts, but also to their methods, as we explain in the following. The earliest multivariate statistical models finding negative economic impacts from co-ethnic clustering include Logan, Alba, and Zhang (2002); Galster, Metzger, and Waite (1999); and Clark and Drinkwater (2002). More mixed or insubstantial impacts, however, were discerned by Sanders and Nee (1987), Zhou and Logan (1989), Van der Klaauw and van Ours (2003), and Urban (2009).

The veracity of these first-generation studies' conclusions can be questioned on methodological grounds because none accounted for the potential biases raised by geographic selection of immigrants with particular characteristics into locations offering distinctly different economic prospects (Galster 2008). Careful econometric analyses, in fact, have found strong patterns of selective immigrant migration patterns that bias the direction of the apparent effect of ethnic clustering if not controlled (Edin, Fredricksson, and Åslund 2003; Piil Damm 2009).

Subsequent empirical work has more effectively accounted for geographic selection through one technique or another⁸ but seems to have reached disparate conclusions based on national context. The US-based studies of Bertrand, Luttmer, and Mullainathan (2000) and Cutler, Glaeser, and Vigdor (2008), for example, find negative impacts on immigrant incomes from co-ethnic clustering. However, Edin, Fredricksson, and Åslund (2003) find the opposite for lower-skilled Swedish immigrants unless they live in neighborhoods with co-ethnics with low incomes and low rates of self-employment. The Sweden-based studies of Musterd et al. (2008) and Andersson, Musterd, and Galster (2014) find, respectively, that an initially positive immigrant income effect turns negative if residence in an ethnic enclave exceeds a couple of years or the percentage of co-ethnics has been growing substantially in the neighborhood. Piil Damm (2009) finds that greater concentrations of co-ethnics in Denmark generally reduce immigrants' earnings regardless of their skill levels but also reduce the probability of full-time employment for more skilled immigrants.

⁸Bertrand, Luttmer, and Mullainathan (2000) and Cutler, Glaeser, and Vigdor (2008) use instrumental variables; Edin, Fredricksson, and Åslund (2003) use a natural experiment supplemented by instrumental variables; Musterd et al. (2008) use differencing; and Andersson, Musterd, and Galster (2014) use fixed effects to limit bias from geographic selection.

The conclusions from this body of work regarding gendered impacts are also inconsistent.⁹ Edin, Fredricksson, and Åslund (2003) and Piil Damm (2009) find no substantial differences in effects between males and females. By contrast, Musterd et al. (2008) find no statistically significant impacts on male immigrant incomes from neighboring co-ethnics. Andersson, Musterd, and Galster (2014) find that although both genders generally benefit similarly from more co-ethnic neighbors, male immigrants' incomes are boosted more by co-ethnics in weaker employment contexts.

The only realm of tentative consensus is that it is not just immigrant concentration that matters but also the characteristics of the immigrants that are clustered. Outcomes of co-ethnic clustering indeed appear highly contextualized, as implied by the theories of Borjas (1995, 1998) noted earlier. The studies of Bertrand, Luttmer, and Mullainathan (2000); Cutler, Glaeser, and Vigdor (2008); Edin, Fredricksson, and Åslund (2003); Musterd et al. (2008); and Andersson, Musterd, and Galster (2014) all show that co-ethnic concentrations can be detrimental to immigrant earnings when co-ethnic neighbors are poorly educated, lower income, or have lower rates of employment.

Clearly, despite the aforementioned path-breaking studies, many uncertainties and unanswered questions remain. Of primary salience, the literature mentioned previously discusses immigrant prospects without a specific focus on either just-arrived immigrants or refugees, as we do.¹⁰ Refugee migration creates a very special situation in the sense that when they enter the country, they start from scratch; virtually none have income from work. Our empirical modeling is thus distinct from prior work not only in its focus on this particular group as it begins its economic life in Sweden but also in its measurement of this group's employment gains over 10 years, starting from a baseline of zero. We also distinguish our work by focusing on long-term effects from the PoE neighborhood's co-ethnic character experienced at this baseline of refugee resettlement, not the contemporaneous impacts of co-ethnic concentrations of larger geographic areas (like municipalities).¹¹ We essentially investigate the degree to which the PoE neighborhood's co-ethnic character

⁹Gendered effects have been observed previously in analyses of the Moving to Opportunity random assignment demonstration (Sanbonmatsu et al. 2011; Chetty, Hendren, and Katz 2015), natural experimental studies (Galster, Santiago, and Lucero 2015), and observational studies (Galster, Andersson, and Musterd 2010) of neighborhood effects on economic outcomes for non-immigrant populations.

¹⁰The exceptions are Piil Damm (2009) and Edin, Fredricksson, and Åslund (2003), though they end up analyzing a mixed sample of refugees and immigrants because they select sample migrants from a set of national origins and nothing else.

¹¹The aforementioned studies all examine short-term neighborhood effects, with the exception of Musterd et al. (2008), who consider five-year lag effects. Edin, Fredricksson, and Åslund (2003) and Piil Damm (2009) measure co-ethnic concentration at the municipal level, so it is perhaps misleading to term their findings *neighborhood effects*.

(mediated by other contextual factors) establishes for just-arrived refugees a path-dependent trajectory over time and space that leads to distinctive employment outcomes over the next decade.

Geographic Selection, Swedish Refugee Resettlement Policy, and Our Identification Strategy

Before we present our empirical model for the analysis, we must clarify the change of policy with regard to refugees' spatial distribution since it bears directly on methodological concerns. As mentioned in an earlier section, from 1984 to mid-1994, Swedish policy was characterized by the government geographically allocating refugees (Andersson and Solid 2003). After mid-1994, however, refugees were entitled to arrange their own accommodation (the Swedish acronym EBO), typically with the assistance of relatives and co-ethnics who had previously come to Sweden. Not surprisingly, most EBO refugees settle in one of Sweden's larger city regions that already have a substantial proportion of migrants. Since the EBO option's introduction, a majority of newly arrived refugees have managed to find housing without government assistance (Andersson and Solid 2003). According to the Board of Migration, though, the proportion opting for EBO has varied over the years in inverse relation to the number of new refugees entering (Andersson 2017). Higher numbers apparently make it more difficult for refugees to find housing using their own network resources.

Because the EBO system permits refugees to self-select their locations, any statistical model trying to estimate to what extent the economic integration outcome depends on the characteristics of neighborhoods in which refugees start their new lives must account for the potential bias associated with geographic selection. It is likely that unobserved characteristics (e.g., the extent and resources of personal networks or particular skills in locating housing and jobs) can both steer refugees into (or lead them to select) a particular neighborhood and assist them in ultimately finding jobs. Failure to control for such unobserved individual characteristics will therefore undermine any causal inferences one might draw concerning neighborhood effects.

We attempt to overcome this geographic selection challenge by using instrumental variables (IVs): exogenous variables that affect the refugee's selection of neighborhood but not the employment outcome we will model as the neighborhood effect. The IV approach has often been employed in neighborhood effects investigations based on nonexperimental data; see the review in Galster and Sharkey (2017).

Modeling Framework and Empirical Approach

We are interested in modeling employment outcomes for refugees given that employment is commonly seen as the primary vehicle for immigrants' broader integration into the host society. We do not employ income as a measure of

economic success (as all aforementioned studies do) because of the nature of our sample: The particular refugees we analyze are so deprived upon arrival in Sweden that almost two-thirds remain unemployed after five years and thus would need to be expunged from a study using income measures. We consider two aspects of employment success: how quickly after finding their first “permanent” accommodation in Sweden (i.e., the PoE) refugees become employed and how consistent their employment is over the following 10 years. In particular, we operationalize these outcomes as the probability of being employed at the five-year mark and the (natural logarithm)¹² of the number of years being employed during the first 10 years after “permanent” accommodation in the PoE.

We aim to ascertain the degree to which the PoE neighborhood’s co-ethnic composition is predictive of these longer-term employment outcomes, controlling for local labor market and individual characteristics. We identified the PoE of individual refugees as the place they were living at the end of the year after arrival (t1). We made this choice because the initial housing placement(s) during the year of arrival is unstable, often involving temporary accommodation and potentially several moves. We measured at the end of year t6 whether the migrant was employed and cumulated the employment histories for the ends of years t1 through t10 as the bases for dependent variables.¹³

The model specification for refugee individual i in PoE j at the end of year $t1$ is:

$$E_{ij} = \alpha + \beta[P_{t1i}] + \theta N_{t1ij} + \mu[L_{t1k}] + \varepsilon_i \quad (1)$$

where:

E_{ij} = employment outcomes for individual i ; either: (a) $E_{t6ij} = 1$ if employed five years after occupying PoE (t1), zero otherwise, or (b) $E_{t1t10ij} = \ln$ (number of years employed within 10 years after occupying PoE)

$[P_{t1i}]$ = characteristics for individual i observed at $t1$ presumed to influence their employment prospects

N_{t1ij} = percentage of the individual’s own ethnic group residing in PoE neighbourhood j at end of year $t1$ ¹⁴

$[L_{t1k}]$ = a set of dummy variables denoting the k th regional labor market in which the individual lived at the end of year $t1$

ε_i = random errors assumed identically distributed but not independent here due to (a) multiple observations in the same neighborhood (which we correct by

¹²This transformation was applied because the variable’s values were positively skewed.

¹³In our data set, employment is assessed on the basis of an annual point-in-time survey in November; details are in the following.

¹⁴Note that this measure of neighborhood j varies according to the individual refugee’s ethnicity but is identical for all refugees in that neighborhood having the same national origin and arriving during the same year.

estimating clustered robust standard errors) and (b) correlations with unobserved individual characteristics that may influence E (which we correct by using rental occupancy and household turnover rates as identifying instrumental variable estimators for N; details in results section that follows).

The precise definitions of all variables used in the models are provided in Table 1 and discussed in the following data section. We emphasize that the timing of when individual refugee characteristics (national origin, year of entering Sweden, age, gender, educational attainment, receipt of various social benefits, coupling and parental status, and refugee permit reason) are measured (i.e., time of entry into PoE, t_1) is crucial to maintain their exogeneity from neighborhood effects that may occur subsequently. We expect the impacts of co-ethnic neighbors to take both direct (e.g., via norms and networks related to work) and indirect (e.g., via changing individual attributes like education and fertility that affect labor force participation) forms. By measuring these personal characteristics before PoE neighborhood effects can occur (instead of concurrently when employment outcomes are measured), we avoid “over-controlling” and minimizing the apparent neighborhood effect thereby. We also employ labor market fixed effects as controls for local economic conditions that potentially affect employment prospects of all working-age adults in that area, including refugees.¹⁵

Our empirical approach involves estimating (1) for both employment outcomes using a linear regression model because of its minimal distributional assumptions and ease of interpretation. We compare estimates of θ both with and without instrumenting for $N_{t,ij}$ to assess the bias of geographic selection in the ordinary least squares (OLS) estimate. We test for *compositional effects* on θ by reestimating the instrumented version of (1) stratified by gender of refugee. We test for *contextual effects* on θ by reestimating the instrumented version of (1) stratified by employment rate of co-ethnics in the neighborhood and, alternatively, by percentage of neighboring co-ethnics with higher education (defined as 15 years or more of schooling). Finally, we consider the interaction of composition and context by jointly stratifying.

Data and Descriptive Statistics

Swedish Data

The variables we employ are constructed from data drawn from the GeoSweden database. This database contains a large amount of information on all individuals

¹⁵Sweden’s 290 municipalities are clustered into 100 labor market regions using statistics on commuting. Preliminary trials reveal that local labor market fixed effects captured much more variation in refugee employment outcomes than municipal fixed effects, so we employ the former.

Table 1. Descriptive Statistics.

| Variable | Observations | Mean | SD | Minimum | Maximum |
|---|--------------|-------|-------|---------|---------|
| Employed after five years (dependent variable) | 26,366 | 0.38 | 0.49 | 0 | 1 |
| Number of years employed t_1-t_{10} (dependent variable) | 24,659 | 3.33 | 3.04 | 0 | 10 |
| Individual characteristics (measured at t_1) | | | | | |
| Male | 26,366 | 0.48 | 0.50 | 0 | 1 |
| Receiving parental leave benefits (1 = yes) | 26,366 | 0.19 | 0.39 | 0 | 1 |
| Receiving sickness leave benefits (1 = yes) | 26,366 | 0.01 | 0.08 | 0 | 1 |
| Currently enrolled in education (1 = yes) | 26,366 | 0.07 | 0.26 | 0 | 1 |
| <12 years of education (1 = yes) | 26,366 | 0.33 | 0.47 | 0 | 1 |
| 12–14 years of education (1 = yes) | 26,366 | 0.28 | 0.45 | 0 | 1 |
| 15+ years of education (1 = yes) | 26,366 | 0.24 | 0.43 | 0 | 1 |
| Somalian (1 = es) | 26,366 | 0.09 | 0.28 | 0 | 1 |
| Iraqi (1 = yes) | 26,366 | 0.69 | 0.46 | 0 | 1 |
| Iranian (1 = yes) | 26,366 | 0.22 | 0.42 | 0 | 1 |
| Single, no child (1 = yes) | 26,366 | 0.32 | 0.46 | 0 | 1 |
| Single parent with child (1 = yes) | 26,366 | 0.05 | 0.22 | 0 | 1 |
| Couple without child (1 = yes) | 26,366 | 0.17 | 0.37 | 0 | 1 |
| Couple with child (1 = yes) | 26,366 | 0.46 | 0.50 | 0 | 1 |
| Entered 1995 (1 = yes) | 26,366 | 0.09 | 0.26 | 0 | 1 |
| Entered 1996 (1 = yes) | 26,366 | 0.06 | 0.24 | 0 | 1 |
| Entered 1997 (1 = yes) | 26,366 | 0.09 | 0.29 | 0 | 1 |
| Entered 1998 (1 = yes) | 26,366 | 0.11 | 0.31 | 0 | 1 |
| Entered 1999 (1 = yes) | 26,366 | 0.10 | 0.30 | 0 | 1 |
| Entered 2000 (1 = yes) | 26,366 | 0.13 | 0.34 | 0 | 1 |
| Entered 2001 (1 = yes) | 26,366 | 0.13 | 0.33 | 0 | 1 |
| Entered 2002 (1 = yes) | 26,366 | 0.14 | 0.35 | 0 | 1 |
| Entered 2003 (1 = yes) | 26,366 | 0.10 | 0.30 | 0 | 1 |
| Entered 2004 (1 = yes) | 26,366 | 0.07 | 0.25 | 0 | 1 |
| Age at time of entry into Sweden (t_0) | 26,366 | 30.71 | 7.54 | 19 | 48 |
| Permit to entry (1 = employment or family reasons) | 26,366 | 0.42 | 0.49 | 0 | 1 |
| Neighborhood characteristics | | | | | |
| Neighborhood population t_1 | 26,366 | 3,134 | 2,427 | 10 | 18,516 |
| Number of own group t_1 | 26,366 | 160 | 226 | 1 | 1,423 |
| Neighborhood % co-ethnics t_1 | 26,366 | 5.01 | 6.03 | 0.00 | 55.91 |
| Neighborhood % Swedish-born t_1 | 26,366 | 67.37 | 18.88 | 17.58 | 97.83 |
| Neighborhood % employed (20–64 years) t_1 | 26,366 | 57.03 | 16.7 | 13.7 | 89.38 |
| Neighborhood % co-ethnics employed t_1 | 26,366 | 25.29 | 15.58 | 1.15 | 100 |
| Neighborhood % co-ethnics highly educated (15+ years) t_1 | 24,221 | 25.62 | 13.28 | 15.15 | 100.00 |
| Neighborhood % rental occupancy t_0 | 25,644 | 67.3 | 30.42 | 0 | 100 |
| Neighborhood % turnover rate ($t_0 - t_1$) | 25,951 | 14.66 | 10.61 | 0 | 100 |

(continued)

Table 1. (continued)

| Variable | Observations | Mean | SD | Minimum | Maximum |
|---|--------------|------|------|---------|---------|
| Labor markets (LMs) | | | | | |
| Port of entry in Stockholm (1 = yes) | 26,366 | 0.36 | 0.48 | 0 | 1 |
| Port of entry in Malmö (1 = yes) | 26,366 | 0.09 | 0.29 | 0 | 1 |
| Port of entry in Gothenburg (1 = yes) | 26,366 | 0.15 | 0.36 | 0 | 1 |
| Port of entry in nonmetro LM region ^a (1 = yes) | 26,366 | 0.40 | 0.49 | 0 | 1 |

^aAll 91 LM regions controlled for individually.

and is assembled from a range of administrative statistical registers (income, education, labor market, real estate, immigration-emigration, and population). We merged selected information about individuals arriving from Iraq, Iran, and Somalia between 1995 and 2004.¹⁶ Their total number was 52,600, but our criteria (e.g., de-selecting repeated entries, return and onward migrants, people dying, and those who did not have a continuous coverage in the registers) reduced their number to 26,366. Most importantly, we kept only those who were of prime working age upon entry and would remain in that category throughout our follow-up period (i.e., ages 19–48 upon entry and 25–59 when measuring employment outcome).

It is worth noticing that the identified population is not a sample but instead includes all that meet the country of origin and immigration year, age, time of residence, register continuity, and neighborhood criteria noted previously. In terms of number of entries, the three groups peak in different years, and the numbers sometimes vary substantially from one year to another. Since labor market conditions vary over time and space and hence affect labor market integration prospects differently for each cohort, we control for year of immigration as well as the local labor market's general conditions (estimated by fixed effects).

Neighborhoods

In this study, we operationalize the scale of neighborhood as a “SAMS” (Small Area Market Statistics) area, as defined by Statistics Sweden. However, for Stockholm City, with rather big SAMS areas, we instead apply the County of Stockholm base area definitions. SAMS contain about one-quarter the population of the US census tract geographical division (average 1,000 residents in Swedish SAMS). We

¹⁶In Sweden, the bulk of people from refugee-sending countries arrive as asylum seekers or relatives to earlier asylum seekers having received a permission to stay. There is however some labor immigration as well from these countries, making it somewhat incorrect to label the entire population under study as *refugee migrants*. In the analyses, we are able to control for these different groups (see the following).

recognize that SAMS are not the only way of delineating neighborhoods (cf. Bolster et al. 2007; Van Ham and Manley 2009; Andersson and Musterd 2010; Andersson and Malmberg 2015) and indeed may represent too large an area to correspond to what residents perceive as their neighborhood (Galster 2008). Thus, we expect any measured effects at this SAMS scale of neighborhood to be underestimates given that Buck (2001), Bolster et al. (2007), Van Ham and Manley (2009), Andersson and Musterd (2010), and Andersson and Malmberg (2015) consistently found stronger neighborhood effects at smaller spatial scales.

We end up with 26,366 individuals arriving in 1,965 neighborhoods located in 265 different municipalities. This means that neighborhoods in more than 90 percent of Sweden's 290 municipalities are included in our analysis (for an overview, see Figure 1). Our PoE neighborhoods were also represented in 91 of Sweden's 100 labor market areas. Thirty-four neighborhoods were PoEs for 100 or more refugees while another 82 were PoEs for between 50 and 100. In total, these 116 neighborhoods took 45 percent of the settlers (11,933 refugees). Twenty-six of these 116 PoE neighborhoods are situated in Stockholm City, 15 in Gothenburg, 11 in Malmö, and five in Södertälje (located in the Stockholm region).

Descriptive Statistics

Table 1 presents characteristics of our analyzed refugees and their neighborhoods. Our sample's ethnic composition was 69 percent Iraqi, 22 percent Iranian, and 9 percent Somali. The three groups' relative share entering Sweden has grown steadily since 1995, peaking in 1999 and declining thereafter. Forty-eight percent of selected refugees were males; 32 percent were single with no children, 5 percent were single with children, 15 percent were coupled with no children, and 46 percent were coupled with children at time of entry. A plurality (33 percent) had the lowest educational attainment (11 years of school or less), 28 percent had a moderate educational attainment (12 to 14 years), and 24 percent had the highest attainment (15+ years).

We include indicators denoting the reason for being granted permission to stay in Sweden. The bulk of people were admitted either on family reunion reasons or refugee or humanitarian grounds (these two are separated in our variable specification). A small number were admitted as labor migrants (see note 16). We judge that they share some basic features with family reunion migrants: They have a network or at least a job waiting for them, and in both cases that affects their PoE neighborhood and subsequent employment trajectory. About 42 percent of the sample constitute labor plus family reunion migrants while the remaining 58 percent are admitted on refugee or similar grounds.

Five years after arrival in the PoE, 38 percent of refugees managed to get a job, and the average number of years employed after 10 years was 3.3, with slightly less than one-third recording no employed year over the period. About 19 percent worked at least seven out of 10 years. Many did not stay in the neighborhood of entry, though. The average refugee under study stayed 3.6 years in the PoE neighborhood, when

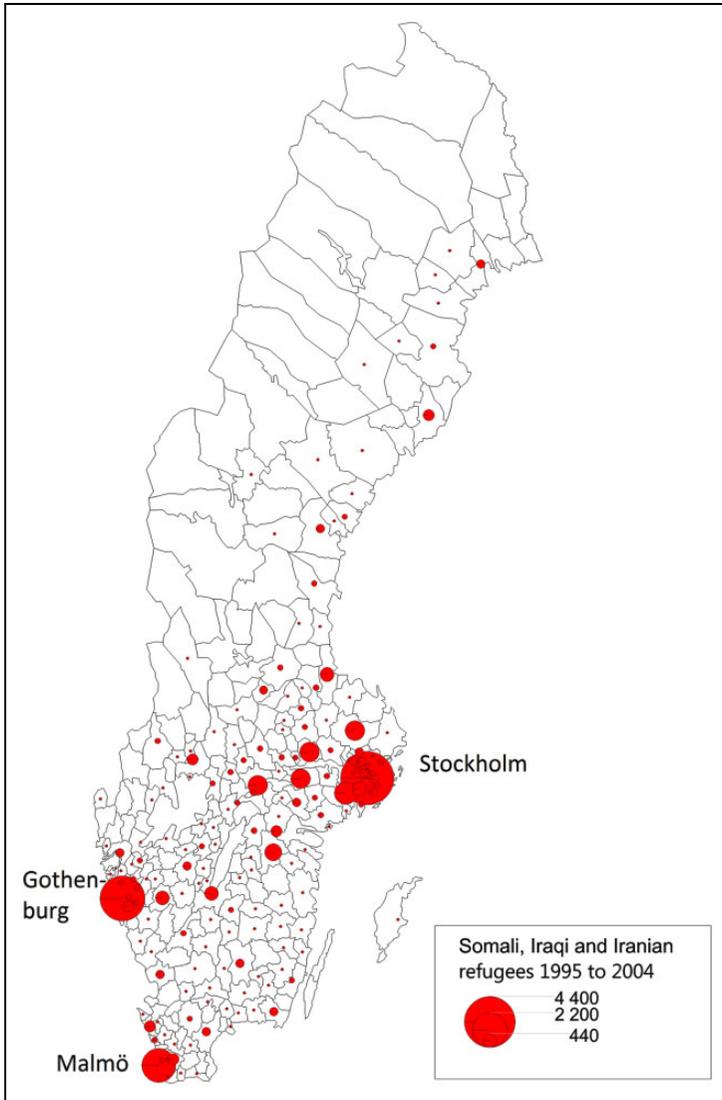


Figure 1. Settlement Pattern of the Population under Study (t1).

measured over a five-year period.¹⁷ Six out of 10 refugees under study resided in one of the three largest labor market areas (Stockholm, Malmö, and Gothenburg).

¹⁷Supplemental analyses found that sample refugees lived for longer spells in port-of-entry (PoE) neighborhoods that had higher shares of their own ethnic group.

The average percentage of a refugee's co-ethnics in the PoE neighborhood was 5 percent but could take values between 0 percent and 56 percent. On average in PoE neighborhoods, 25 percent of the resident co-ethnic group was employed, ranging from 1 percent to 100 percent. The average percent of highly educated co-ethnics was in the same range, around 25 but ranging from 15 to 100.

Results

Refugees' Neighborhood Selection Processes and Evaluation of Instruments

We present our first-stage OLS models predicting the percentage of co-ethnics in the PoE neighborhood population at year t_1 in Table 2. Following a standard two-stage least squares (2SLS) procedure, we employed as regressors all exogenous variables (P_{t1i}) and (L_{t1k}) in equation (1) and our identifying instruments — neighborhood rental occupancy and household turnover rates.¹⁸ This regression not only is crucial for supplying a strong instrumental variable estimate of neighborhood co-ethnic composition used in our second-stage model (equation [1]) of its effects on refugees' employment outcomes but also offers insights into refugees' geographic selection processes.

All standard analyses indicated that our identifying instruments — rental occupancy and household turnover rates — were both valid and strong. To be valid, our IVs must: (1) be correlated (with plausible causality) with neighborhood co-ethnic composition, (2) be uncorrelated with the error term in the employment outcome equation (1), and (3) not be otherwise included in (1). First, we know, based on earlier research (BråmÅ and Andersson 2010; Skifter Andersen et al. 2016), that newly arrived refugees in Sweden have few housing options outside the public or private rental market because of their typically low wealth. Thus, we expect neighborhoods with higher shares of rental dwellings to attract greater shares of refugees as well as other immigrants of the same national origin. It is also clear from the Swedish data that neighborhoods experiencing higher residential turnover rates are less desirable locations. This means that controlling for rental rates, refugees and co-ethnics will be less likely to select such areas. As shown in Table 2, both our identifying instruments indeed proved strongly predictive at $p < .0001$ in the hypothesized direction. Second, we posit that our IVs *only* affect neighborhood co-ethnic composition and not refugees' employment prospects, other than through their relationship with this neighborhood characteristic, *ceteris paribus*.¹⁹

¹⁸These variables are defined as the percentages of all occupied dwelling units in the neighborhood that are rented during year t_1 and change household occupants between years t_0 and t_1 , respectively.

¹⁹In a context of overidentification, as we have here, it has been conventional to employ a Sargan test for the validity of instruments, which our model indeed passes. However, Parente and Silva (2012) argue that this is an inappropriate interpretation and that instead

Table 2. First Stage Regressions Predicting Co-ethnic Percentage in Port of Entry Neighborhood.

| Exogenous Predictors (measured at end of year t_1 unless noted) | Coefficient | SE | Significance |
|---|-------------|-------|--------------|
| Female (male omitted reference) | -0.084 | 0.079 | |
| Somalian (Iranian omitted reference) | 0.055 | 0.142 | |
| Iraqi (Iranian omitted reference) | 2.317 | 0.088 | *** |
| Receiving social benefits (1 = yes) | 0.657 | 0.100 | *** |
| Receiving parental leave benefits (1 = yes) | -0.121 | 0.101 | |
| Receiving sick leave benefits (1 = yes) | -0.036 | 0.412 | |
| Currently enrolled in school (1 = yes) | -0.424 | 0.125 | *** |
| 12-14 years of education (<12 omitted reference) | -0.259 | 0.078 | *** |
| 15+ years of education (<12 omitted reference) | -0.547 | 0.084 | *** |
| Single parent w/ child (single, no child omitted) | 0.248 | 0.165 | |
| Couple w/ child (single, no child omitted reference) | 0.779 | 0.092 | *** |
| Couple, no child (single, no child omitted reference) | 0.342 | 0.113 | *** |
| Entered 1996 (1995 omitted reference) | -0.088 | 0.185 | |
| Entered 1997 (1995 omitted reference) | 0.550 | 0.170 | *** |
| Entered 1998 (1995 omitted reference) | 1.068 | 0.163 | *** |
| Entered 1999 (1995 omitted reference) | 1.221 | 0.167 | *** |
| Entered 2000 (1995 omitted reference) | 1.831 | 0.161 | *** |
| Entered 2001 (1995 omitted reference) | 2.227 | 0.162 | *** |
| Entered 2002 (1995 omitted reference) | 1.920 | 0.158 | *** |
| Entered 2003 (1995 omitted reference) | 2.031 | 0.166 | *** |
| Entered 2004 (1995 omitted reference) | 1.972 | 0.181 | *** |
| Age at time of entry into Sweden | -0.005 | 0.005 | |
| Permit to entry = employment or family reasons | -0.246 | 0.085 | *** |
| Neighborhood % household turnover rate ^a | -0.015 | 0.003 | *** |
| Neighborhood % rental occupancy ^a | 0.039 | 0.001 | *** |
| Constant | -0.996 | 0.272 | *** |

Note. Number of observations = 2,5387. $F(113, 25273) = 79.88$. Prob > $F = 0.0000$. Total (centered) SS = 87.92. (Centered) $R^2 = 0.263$. Residual SS = 64.78. Root MSE = .051. Sargan statistic (overidentification test of all instruments): 1.121. Ho: IV uncorrelated with residuals of second stage; $\chi^2(1)$ p value = .2898. Underidentification tests: Anderson canon. corr. likelihood ratio stat.: $\chi^2(2) = 1,135.03$; p value = .0000; Cragg-Donald N^* minEval stat.: $\chi^2(2) = 1,160.79$; p value = .0000; Ho: matrix of reduced form coefficients has rank = $K - 1$ (underidentified). Partial R^2 of excluded instruments: 0.0437. Test of excluded instruments: $F(2, 25273) = 577.79$; Prob > $F = .0000$. Weak identification statistics: Cragg-Donald (N-L)*minEval/L2 F-stat = 578; Stock-Yogo critical values for maximal IV bias at $b < .05 = 21$ approx.; Stock-Yogo critical values for maximal IV size at $r < .10 = 220$ approx.; Anderson-Rubin test of significance of endogenous regressor B in main equation, Ho: $B = 0$ $F(2, 25273) = 8.42$; p value = .0002; $\chi^2(2) = 16.92$; p value = .0002.

^aMeasured during year prior to entry.

*** $p < .001$.

this test should be thought of as whether the instruments are coherent, namely, whether all instruments identify the same vector of parameters.

To be strong, our instruments must be highly correlated with co-ethnic percentages in the PoE neighborhoods and contribute significantly to the first-stage equation's explanatory power. They indeed prove extremely strong according to conventional criteria. Rental occupancy and household turnover rates jointly contribute a statistically significant .044 to the total R^2 of .263 in the first stage model shown in Table 2. The Cragg-Donald statistics are far above Stock-Yogo critical values, providing further confirmation of strength; see the notes in Table 2.

Many refugees' individual characteristics were predictive in the PoE neighborhoods' co-ethnic composition. The following predicted greater shares of one's own ethnic group in the neighborhood: originating in Iraq (vs. Somalia or Iran), receiving social benefits, having low educational credentials or not being enrolled in school, being coupled, arriving in Sweden in 1997 or later, and being granted refugee permission on the basis of employment or family. Our results regarding social benefits and education comport nicely with prior studies of refugee mobility in Scandinavia (Edin, Fredricksson, and Aslund 2003; Piil Damm 2009), which found that socioeconomically "weaker" refugees tended to sort into own-ethnic enclaves after several years of residence in the host nation. Supplemental analyses of local labor market fixed effects (not shown) revealed that those with higher mean incomes and employment rates were more likely to have refugees moving into PoE neighborhoods exhibiting higher shares of their own ethnic group.

Effect of Co-ethnic Neighbors on Refugee Employment: Composition Model

Table 3 presents the results of our neighborhood co-ethnic effect models of refugee employment outcomes (1), one set estimated using OLS and the other using 2SLS, namely, instrumental estimates of the neighborhood variable. Regardless of estimation technique, several individual characteristics measured at time of refugee entry into PoE strongly predict employment prospects. Clearly, males have a significantly higher probability of being employed after five years and work more during their first 10 years than females, all else being equal, as would be expected given the traditional, patriarchal culture of the three refugee groups under investigation. Already having middle- or higher-level educational credentials or currently studying for such has similarly felicitous results for employment, as would be predicted from standard human capital theory. Refugees from Iran have superior employment outcomes compared to refugees from Iraq and Somalia. Those arriving at the PoE with social welfare or parental leave benefits experience inferior employment outcomes, though those in coupled relationships fare better, especially if they already have a child. Refugees had weaker employment prospects if they were older when they entered Sweden, entered before 1999, or received permission to enter for employment or family reasons. Supplemental analyses of local labor market area fixed effects (not shown)

Table 3. Core Models.

| Predictors | Outcome = Employed after 5 years | | | Outcome = ln (number of years employed after 10 years) | | |
|---|----------------------------------|-----------------------------|--------------|--|-----------------------------|--------------|
| | OLS regression | 2SLS regression | | OLS regression | 2SLS regression | |
| | Number of obs = 26,101 | Number of obs = 25,387 | | Number of obs = 24,618 | Number of obs = 23,943 | |
| | $F(102, 2439) =$ See note. | $Wald \chi^2(112) = 32,803$ | | $F(102, 2372) =$ See note. | $Wald \chi^2(112) = 88,418$ | |
| | $Prob > F = 0.000$ | $Prob > \chi^2 = 0.0000$ | | $Prob > F = 0.000$ | $Prob > \chi^2 = 0.0000$ | |
| | $R^2 = 0.109$ | $R^2 = 0.105$ | | $R^2 = 0.170$ | $R^2 = 0.170$ | |
| | Root MSE = .459 | Root MSE = .460 | | Root MSE = 6.373 | Root MSE = 6.345 | |
| | Coefficient | SE ^a | Significance | Coefficient | SE ^a | Significance |
| % of co-ethnics in neighborhood | -0.004 | 0.001 | *** | -0.011 | 0.003 | *** |
| Female (male omitted reference) | -0.196 | 0.007 | *** | -0.198 | 0.007 | *** |
| Somalian (Iranian omitted reference) | -0.134 | 0.012 | *** | -0.132 | 0.013 | *** |
| Iraqi (Iranian omitted reference) | -0.037 | 0.008 | *** | -0.022 | 0.011 | * |
| Receiving social benefits (1 = yes) | -0.025 | 0.010 | ** | -0.018 | 0.010 | *** |
| Receiving parental leave benefits (1 = yes) | -0.056 | 0.009 | *** | -0.056 | 0.009 | *** |
| Receiving sick leave benefits (1 = yes) | 0.023 | 0.038 | | 0.024 | 0.038 | |
| Currently enrolled in school (1 = yes) | 0.072 | 0.012 | *** | 0.070 | 0.012 | *** |
| 12-14 years of education (<12 omitted reference.) | 0.096 | 0.008 | *** | 0.095 | 0.008 | *** |
| 15+ years of education (<12 omitted reference) | 0.167 | 0.008 | *** | 0.165 | 0.008 | *** |
| Single parent with child (single, no child omitted) | 0.025 | 0.014 | | 0.027 | 0.014 | * |
| | | | | 0.055 | 0.217 | |
| | | | | 2.285 | 0.336 | *** |
| | | | | 1.168 | 0.142 | *** |
| | | | | 2.264 | 0.110 | *** |
| | | | | 3.295 | 0.118 | *** |
| | | | | 0.088 | 0.219 | |
| | | | | 2.289 | 0.336 | *** |
| | | | | 1.142 | 0.141 | *** |
| | | | | 2.264 | 0.111 | *** |
| | | | | 3.316 | 0.120 | *** |
| | | | | 0.088 | 0.219 | |

(continued)

Table 3. (continued)

| Predictors | Outcome = Employed after 5 years | | | Outcome = ln (number of years employed after 10 years) | | |
|--|----------------------------------|-----------------------------|--------------|--|-----------------------------|--------------|
| | OLS regression | 2SLS regression | | OLS regression | 2SLS regression | |
| | Number of obs = 26,101 | Number of obs = 25,387 | | Number of obs = 24,618 | Number of obs = 23,943 | |
| | $F(102, 2439) =$ See note. | $Wald \chi^2(112) = 32,803$ | | $F(102, 2372) =$ See note. | $Wald \chi^2(112) = 88,418$ | |
| | $Prob > F = 0.000$ | $Prob > \chi^2 = 0.0000$ | | $Prob > F = 0.000$ | $Prob > \chi^2 = 0.0000$ | |
| | $R^2 = 0.109$ | $R^2 = 0.105$ | | $R^2 = 0.170$ | $R^2 = 0.170$ | |
| | Root MSE = .459 | Root MSE = .460 | | Root MSE = 6.373 | Root MSE = 6.345 | |
| | Coefficient | SE ^a | Significance | Coefficient | SE ^a | Significance |
| | 0.024 | 0.009 | ** | 0.031 | 0.010 | ** |
| Couple with child (single, no child omitted reference) | 0.011 | 0.011 | | 0.015 | 0.011 | |
| Couple, no child (single, no child omitted reference) | 0.004 | 0.016 | | 0.005 | 0.017 | |
| Entered 1996 (1995 omitted reference) | 0.010 | 0.015 | | 0.016 | 0.016 | |
| Entered 1997 (1995 omitted reference) | -0.005 | 0.015 | | 0.003 | 0.015 | |
| Entered 1998 (1995 omitted reference) | -0.020 | 0.015 | | -0.009 | 0.016 | |
| Entered 1999 (1995 omitted reference) | 0.001 | 0.015 | | 0.017 | 0.016 | |
| Entered 2000 (1995 omitted reference) | 0.046 | 0.014 | *** | 0.063 | 0.016 | *** |
| Entered 2001 (1995 omitted reference) | 0.063 | 0.015 | *** | 0.077 | 0.016 | *** |
| Entered 2002 (1995 omitted reference) | | | | | | |
| | | | | 0.382 | 0.122 | ** |
| | | | | 0.507 | 0.143 | *** |
| | | | | 0.204 | 0.247 | |
| | | | | 0.267 | 0.225 | |
| | | | | 0.251 | 0.221 | |
| | | | | 0.766 | 0.213 | *** |
| | | | | 0.501 | 0.217 | * |
| | | | | 0.619 | 0.218 | ** |
| | | | | 0.687 | 0.201 | *** |
| | | | | 0.420 | 0.128 | *** |
| | | | | 0.529 | 0.144 | *** |
| | | | | 0.274 | 0.257 | |
| | | | | 0.290 | 0.232 | |
| | | | | 0.265 | 0.232 | |
| | | | | 0.823 | 0.224 | *** |
| | | | | 0.550 | 0.236 | * |
| | | | | 0.676 | 0.245 | ** |
| | | | | 0.724 | 0.223 | *** |

(continued)

unsurprisingly suggest refugee employment prospects are brighter in areas with higher employment rates.²⁰

Of more central interest to us are the results for the neighborhood co-ethnic composition variable. All models suggest that residence in PoE neighborhoods with larger shares of co-ethnics has a significantly harmful effect on refugee employment prospects regardless of how the outcome is measured. However, the apparent magnitude of this effect (i.e., coefficient) is approximately twice as large in absolute magnitude in the 2SLS (IV) model compared to the OLS model. This reconfirms an often found result that OLS leads to substantially biased (under)estimates of neighborhood effects (e.g., Edin, Fredricksson, and Åslund 2003; Piil Damm 2009). More specifically, our result suggests that refugees with unobserved personal characteristics that led them to select PoEs with greater co-ethnic concentrations also used these characteristics to advantage in finding employment, thereby tending to obscure (i.e., bias downward) the apparent neighborhood effect, though we have no compelling explanation of what these unobserved characteristics may be. Given the 2SLS estimator's evident superiority, we shall only report these results subsequently in the paper. The 2SLS point estimate presented in Table 3 indicates that refugees moving into a PoE with a one percentage point higher share of co-ethnic neighbors will decrease their probability of being employed after five years by .011, representing a 3 percent decline from the sample mean probability of .38. Such a situation would also be associated with an even greater proportionate fall in the cumulative number of years worked over a 10-year span since PoE residence: 9.7 percent.

Table 4 shows the results of our 2SLS estimates of (1) stratified by gender, as a test of the potential heterogeneity of findings. What is immediately obvious is that the aforementioned negative effects of co-ethnic concentrations in the PoE are almost entirely the result of female refugees. The point estimates for males are essentially zero and measured with little precision. By contrast, those for females are significantly negative and precisely measured ($p < .01$). The coefficients indicate that female refugees moving into a PoE with a one percentage point higher share of co-ethnic neighbors will decrease their probability of being employed after five years by .016, representing a 5.7 percent decline from the female sample mean probability of .28. Such a situation would also be associated with an even larger proportionate decline in females' cumulative number of years worked over a 10-year span since initial PoE residence: 19.0 percent. We speculate about the sources of this dramatic gendered variation in findings after we conduct further investigations in the following.

²⁰We cannot unambiguously interpret this as an area effect instead of a selection effect, however, since we cannot instrument for all these labor market areas.

Table 4. Gender-Stratified Two-Stage Least Squares (2SLS) Models.

| | Outcome = Employed after 5 Years | | | Outcome = ln (number years employed after 10 years) | | |
|--|----------------------------------|-------------------------|--------------|---|-------------------------|--------------|
| | Males | Females | | Males | Females | |
| | Number of obs = 12,209 | Number of obs = 13,178 | | Number of obs = 11,498 | Number of obs = 12,445 | |
| | F(93, 1786) = See note. | F(96, 2017) = See note. | | F(93, 1734) = See note. | F(96, 1962) = See note. | |
| | Prob > F = 0.000 | Prob > F = 0.000 | | Prob > F = 0.000 | Prob > F = 0.000 | |
| | R ² = 0.080 | R ² = 0.074 | | R ² = 0.126 | R ² = 0.157 | |
| | Root MSE = .482 | Root MSE = .433 | | Root MSE = 5.777 | Root MSE = 6.776 | |
| Predictors | Coefficient | SE ^a | Significance | Coefficient | SE ^a | Significance |
| % of co-ethnics in neighborhood | -0.004 | 0.004 | | 0.007 | 0.055 | |
| Somalian (Iranian omitted reference) | -0.138 | 0.021 | *** | -1.596 | 0.275 | *** |
| Iraqi (Iranian omitted reference) | -0.005 | 0.016 | | -0.542 | 0.208 | ** |
| Receiving social benefits (1 = yes) | -0.028 | 0.018 | * | -0.424 | 0.207 | * |
| Receiving parental leave benefits (1 = yes) | 0.054 | 0.025 | * | 0.534 | 0.282 | |
| Receiving sick leave benefits (1 = yes) | 0.015 | 0.054 | | 1.518 | 0.438 | *** |
| Currently enrolled in school (1 = yes) | 0.060 | 0.016 | *** | 0.815 | 0.171 | *** |
| 12-14 years of education (<12 omitted reference) | 0.100 | 0.011 | *** | 1.567 | 0.130 | *** |
| 15+ years of education (<12 omitted reference) | 0.174 | 0.012 | *** | 2.532 | 0.144 | *** |
| Single parent with child (single, no child omitted) | -0.069 | 0.043 | | -0.176 | 0.561 | |
| Couple with child (single, no child omitted reference) | 0.056 | 0.012 | *** | 0.739 | 0.155 | *** |
| Couple, no child (single, no child omitted reference) | 0.067 | 0.016 | *** | 0.835 | 0.195 | *** |
| | | | | 0.375 | 0.222 | |

(continued)

Table 4. (continued)

| | Outcome = Employed after 5 Years | | | Outcome = In (number years employed after 10 years) | | |
|--|----------------------------------|-------------------------|--------------|---|-------------------------|--------------|
| | Males | Females | | Males | Females | |
| | Number of obs = 12,209 | Number of obs = 13,178 | | Number of obs = 11,498 | Number of obs = 12,445 | |
| | F(93, 1786) = See note. | F(96, 2017) = See note. | | F(93, 1734) = See note. | F(96, 1962) = See note. | |
| | Prob > F = 0.000 | Prob > F = 0.000 | | Prob > F = 0.000 | Prob > F = 0.000 | |
| | R ² = 0.080 | R ² = 0.074 | | R ² = 0.126 | R ² = 0.157 | |
| | Root MSE = .482 | Root MSE = .433 | | Root MSE = 5.777 | Root MSE = 6.776 | |
| Predictors | Coefficient | SE ^a | Significance | Coefficient | SE ^a | Significance |
| Entered 1996 (1995 omitted reference) | 0.035 | 0.026 | | 0.216 | 0.336 | |
| Entered 1997 (1995 omitted reference) | 0.044 | 0.025 | | 0.528 | 0.292 | |
| Entered 1998 (1995 omitted reference) | 0.039 | 0.024 | | 0.716 | 0.310 | * |
| Entered 1999 (1995 omitted reference) | 0.021 | 0.024 | | 1.015 | 0.306 | *** |
| Entered 2000 (1995 omitted reference) | 0.039 | 0.024 | | 0.607 | 0.309 | * |
| Entered 2001 (1995 omitted reference) | 0.108 | 0.023 | *** | 0.781 | 0.305 | ** |
| Entered 2002 (1995 omitted reference) | 0.138 | 0.023 | *** | 1.122 | 0.297 | *** |
| Entered 2003 (1995 omitted reference) | 0.061 | 0.025 | * | 1.237 | 0.329 | *** |
| Entered 2004 (1995 omitted reference) | 0.091 | 0.030 | ** | 1.504 | 0.356 | *** |
| Age at time of entry into Sweden | -0.012 | 0.001 | *** | -0.247 | 0.009 | *** |
| Permit to entry = employment or family reasons | 0.010 | 0.013 | | -0.032 | 0.184 | *** |
| Constant | 0.810 | 0.032 | *** | 5.102 | 0.454 | *** |
| N of neighborhood clusters | 1,787 | 2,018 | | 1,735 | 1,963 | |

Note. All models include local labor market fixed effects. Stata suppressed the F statistics to not be misleading.

^aRobust standard errors adjusted for neighborhood clusters.

*p < .05, **p < .01, ***p < .001.

Table 5. Effects of Co-ethnic Neighbor Percentages Two-Stage Least Squares (2SLS) Models Stratified by Co-ethnic Neighbor Employment Rate Quartiles.

| Co-ethnic Neighbor Employment Quartiles by Refugee Sample | Outcome = Employed after 5 Years | | | | Outcome = ln (number of years employed after 10 years) | | | |
|---|----------------------------------|-----------------|--------------|-------|--|-----------------|--------------|-------|
| | Coefficient | SE ^a | Significance | N | Coefficient | SE ^a | Significance | N |
| All | | | | | | | | |
| 0–25 | –0.0109 | 0.0060 | * | 5,670 | 0.0124 | 0.1032 | | 5,323 |
| 25–50 | –0.0193 | 0.0062 | *** | 5,730 | –0.1373 | 0.0961 | | 5,411 |
| 50–75 | –0.0067 | 0.0063 | | 5,678 | –0.0985 | 0.0915 | | 5,361 |
| 75–100 | –0.0033 | 0.0047 | | 8,272 | –0.0252 | 0.0647 | | 7,813 |
| Males | | | | | | | | |
| 0–25 | –0.0039 | 0.0084 | | 3,015 | 0.1598 | 0.1321 | | 2,823 |
| 25–50 | –0.0144 | 0.0086 | | 2,856 | –0.0467 | 0.1066 | | 2,692 |
| 50–75 | 0.0031 | 0.0094 | | 2,655 | 0.0546 | 0.1105 | | 2,503 |
| 75–100 | –0.0012 | 0.0068 | | 3,664 | 0.0563 | 0.0815 | | 3,461 |
| Females | | | | | | | | |
| 0–25 | –0.0190 | 0.0082 | ** | 2,655 | 0.0047 | 0.1469 | | 2,500 |
| 25–50 | –0.0278 | 0.0089 | *** | 2,874 | –0.2665 | 0.1503 | * | 2,719 |
| 50–75 | –0.0104 | 0.0079 | | 3,023 | –0.2147 | 0.1291 | * | 2,858 |
| 75–100 | –0.0056 | 0.0062 | | 4,608 | –0.0926 | 0.0933 | | 4,352 |

Note. All models include controls as shown in Table 3. Cut points for employment rate quartiles: .147, .240, .364.

^aRobust standard errors adjusted for neighborhood clusters.

*p < .05. **p < .01. ***p < .001.

Effect of Co-ethnic Neighbors on Refugee Employment: Context Model

To explore the degree to which the impact of co-ethnic neighbors varies by the contextual characteristics of this group, we alternatively stratify our 2SLS models of (1) by quartiles of the proportion of neighboring co-ethnics who are employed and have higher educational attainments (15 years and above) as well as gender. Results are presented in the upper panels of Tables 5 and 6, respectively; for brevity, we report only the coefficients for the percentage of co-ethnic neighbors. The main lesson from these results is that though parameters are estimated imprecisely, it appears that co-ethnic employment context matters, as does co-ethnic residential clustering.

Table 5 indicates that co-ethnics cause the least harm for refugee employment prospects when the PoE neighborhood is in the highest quartile of co-ethnic employment rates. For both employment outcomes, there is a pattern of the point estimates getting progressively larger in absolute magnitude as the quartiles get lower, although this pattern is inexplicably interrupted in the lowest quartile. By contrast, there is no clear pattern of results across co-ethnic higher education quartiles, and the estimates are extremely imprecise (see Table 6’s upper panels).

Table 6. Effects of Co-ethnic Neighbor Percentages Two-Stage Least Squares (2SLS) Models Stratified by Co-ethnic Neighbor Higher Education Quartiles.

| Co-ethnic Neighbor Education Quartiles by Refugee Sample | Outcome = Employed after 5 Years | | | | Outcome = ln (number of years employed after 10 years) | | | |
|--|----------------------------------|-----------------|--------------|-------|--|-----------------|--------------|-------|
| | Coefficient | SE ^a | Significance | N | Coefficient | SE ^a | Significance | N |
| All | | | | | | | | |
| 0–25 | –0.0090 | 0.0060 | | 5,866 | 0.0430 | 0.0943 | | 5,487 |
| 25–50 | –0.0129 | 0.0071 | * | 5,841 | –0.1200 | 0.1064 | | 5,495 |
| 50–75 | –0.0082 | 0.0061 | | 5,844 | –0.0625 | 0.0935 | | 5,535 |
| 75–100 | –0.0069 | 0.0050 | | 7,836 | –0.1101 | 0.0647 | * | 7,426 |
| Males | | | | | | | | |
| 0–25 | –0.0091 | 0.0089 | | 2,657 | 0.0746 | 0.1061 | | 2,491 |
| 25–50 | –0.0002 | 0.0089 | | 2,891 | 0.0679 | 0.1231 | | 2,700 |
| 50–75 | 0.0006 | 0.0084 | | 2,846 | –0.0455 | 0.1075 | | 2,693 |
| 75–100 | –0.0054 | 0.0068 | | 3,815 | –0.0030 | 0.0860 | | 3,614 |
| Females | | | | | | | | |
| 0–25 | –0.0105 | 0.0078 | | 3,209 | –0.0422 | 0.1457 | | 2,996 |
| 25–50 | –0.0283 | 0.0091 | *** | 2,950 | –0.3275 | 0.1472 | ** | 2,795 |
| 50–75 | –0.0161 | 0.0084 | * | 2,998 | –0.0963 | 0.1376 | | 2,842 |
| 75–100 | –0.0081 | 0.0066 | | 4,021 | –0.2079 | 0.0948 | ** | 3,812 |

Note. All models include controls as shown in Table 3. Cut points for highly educated quartiles: .174, .240, .317.

^aRobust standard errors adjusted for neighborhood clusters.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Thus, it appears that only certain aspects of ethnic enclaves may matter for refugees' employment prospects.²¹

Given the strong gender differences in co-ethnic concentration effects observed for the entire sample of neighborhoods, it is appropriate to discern whether the aforementioned results for co-ethnic context are also distinctly gendered. Examination of the bottom panels of Tables 5 and 6 reveals that indeed they are. It remains the case that male refugees' employment prospects seem unaffected by variations in percentages of co-ethnic neighbors in their PoE regardless of these co-ethnics' aggregate employment or educational attainments. By contrast, sizable and statistically significant adverse consequences for female refugees' employment prospects only begin to manifest themselves if the PoE is not in the top quartile of co-ethnic

²¹ As a robustness check on our conclusion that neighborhood employment context matters a great deal, we replicated the analysis presented in Table 5 substituting the employment rate of all neighborhood residents with percentage of co-ethnics with high educational credentials. The results were virtually identical.

employment rates.²² The same conclusion can be drawn regarding the top quartile of co-ethnic higher education, at least when it comes to the outcome of female refugees being employed after five years.

Discussion

We have found that greater co-ethnic concentrations in the Swedish PoE neighborhood experienced by female refugees from Iran, Iraq, and Somalia have profoundly unfortunate consequences for their longer-term employment prospects unless these females reside in neighborhoods where co-ethnics are employed at high rates. There are no clear impacts on refugee males in any contexts. It is revealing to compare these results to those from the only two other plausibly causal analyses of neighborhood impacts on refugees in Scandinavia. Both Edin, Fredricksson, and Åslund (2003) and Piil Damm (2009) generally find positive effects of co-ethnic clustering on refugees' incomes, no substantial differences in effects between males and females, and context effects where higher-income co-ethnic clusters generate more positive outcomes.²³ At one level, one might expect our results to be different from these two studies because of crucial differences in data and measures. Both Edin, Fredricksson, and Åslund (2003) and Piil Damm (2009) (1) analyze refugees from distinctly different national origins than we did; (2) measure co-ethnic concentrations in the municipality, not the SAMS scale; (3) focus on contemporaneous effects of co-ethnic clustering experienced seven or eight years after arrival, instead of longer-term effects from the PoE neighborhood; and (4) consider income, not employment outcomes, and thus analyze only refugees who are working.²⁴ At another level, however, it may be the case that these alternative findings are not the result of methodological inconsistencies but instead co-exist. In other words, it is possible that co-ethnic clustering harms the prospects for refugee women finding work in the future while at the same time enhancing the earnings of all refugees who currently are working. Analogously, it is possible that co-ethnic clustering at the municipal scale has more distinctive impacts than at the neighborhood scale.

Our findings interface with a longstanding literature about whether distinctive types of lower-income or immigrant-dense neighborhoods hinder or enhance residents' chances for assimilating into their host society (Boal 1976; Murie and Musterd 2004), perhaps by moving out of the neighborhood (Massey 1985).²⁵ Our study

²² Once again, the pattern is less consistent for the number of years employed outcome.

²³ There is not perfect correspondence of these findings. For example, Edin, Fredricksson, and Åslund (2003) find a positive impact only for lower-skilled immigrants, and Piil Damm (2009) finds only a weak context effect.

²⁴ In a supplemental analysis, Piil Damm (2009) finds no impact of co-ethnic clustering on the probability of a refugee being employed.

²⁵ Boal (1976) advanced the position that there were three neighborhood types performing different functions for immigrants. The colony was a port of entry serving as a base for

provides qualified support for this view of neighborhood types with heterogeneous impacts on immigrants, suggesting that most PoE neighborhoods in Sweden occupied by the cohorts of Iranian, Iraqi, and Somalian refugees we analyzed have sufficient percentages of co-ethnics to severely retard females' employment prospects over the succeeding 10 years. Other PoE neighborhoods with substantial numbers of employed co-ethnic residents appear more neutral in their economic impact on refugee females. These distinctive immigrant neighborhood typologies must be qualified, however, by our observation of minimal impacts of any type of co-ethnic residential context on male refugees.

Although necessarily speculative, it is also useful to consider what underlying causal mechanism(s) might be generating the strongly gendered neighborhood effect we observed. We posit that primarily, collective socialization is at work. Traditional patriarchal norms and values typically represented in Iran, Iraq, and Somalia likely create a dominant normative environment in more concentrated co-ethnic enclaves that discourage females from working. These dominant values against female work may be countered, however, in neighborhood contexts where more co-ethnics are employed. Presumably in these circumstances there will be more co-ethnic women employed who serve as nontraditional role models.

Given the aforementioned inconsistencies in results of prior studies and the ambiguous explanations for these inconsistencies, we are loath to make firm policy recommendations. Nevertheless, some discussion of policy implications is in order. In terms of the Swedish refugee placement and settlement issue (currently revisited by a special government investigation asked to provide new recommendations by Spring 2018), the strong gendered differences in outcomes are problematic for policymakers in a situation where families, rather than individuals, typically immigrate. What seems to be sound advice for the placement of female refugees — avoid co-ethnic clustering in the neighborhood — is less sound for their spouses. A general conclusion, of course, is that if employment rates of earlier cohorts of co-ethnics are high in a neighborhood, the presence of co-ethnics is of less consequence for females and could indeed be beneficial to males.²⁶ Identifying and encouraging refugees to settle in PoE neighborhoods with high co-ethnic employment rates is obviously a more sensible strategy than simply dispersing them more randomly. The potential problem with operationalizing such a strategy, however, is that these neighborhoods may be less accessible for recently arrived refugees due to less rental housing, less

assimilation and a springboard to other neighborhoods, the ethnic enclave was a voluntary concentration not necessarily resulting in eventual assimilation or geographic dispersal, and the ethnic ghetto was a place where many immigrants resided involuntarily and from which it was difficult to escape.

²⁶This may be the case if Piil Damm (2009) is correct that co-ethnic clusters provide job information networks that improve the match between refugee skills and job requirements in ways that boost earnings from the jobs that ultimately are found.

residential turnover, and more competition from natives because they are more attractive locales. With reference to Andersson, Bråmås, and Holmqvist (2010) and as pointed out in the introduction, solving the refugee settlement issue within strong labor market areas is what policymakers should be concerned with if combatting segregation and improving refugees' labor market integration are priorities. Unfortunately, such a refugee settlement strategy is inconsistent with the Swedish *laissez-faire* approach practiced after 1994. Moreover, it is currently constrained by a severe housing shortage, especially a shortage of affordable rental housing in most urban localities. Finding housing for new refugees is difficult per se; finding housing in a favorable location is even more difficult.

Although our analyses produce clear and robust results, two caveats must be mentioned before closing. First, though we focus on a key attribute of refugees' PoE, it is possible that other neighborhood features could enrich the analyses by providing more controls and added insights into the mechanisms at work. Besides other measurable aspects of the neighborhood's population, accessibility to employment, and environmental quality, there could be other, hard to measure neighborhood variations in the institutional support structure (e.g., language competence of actors at the local employment office and availability of interpreters) or the presence of co-ethnic entrepreneurs and sources of training and capital. Future researchers must recognize, however, that expanding the list of neighborhood attributes requires the concomitant expansion of instrumental variables for identifying our 2SLS approach, which poses its own daunting challenges.²⁷ Second, our findings may not necessarily be generalized to non-Swedish contexts, refugees from other national origins, immigrants who are not refugees, or different time periods than the one we investigated.

Conclusions

In 2015, more than 160,000 refugees arrived in Sweden and applied for asylum. In early summer 2016, close to 180,000 were awaiting decisions on their application. Even if the proportion receiving a positive decision from the Swedish Migration Board remains around 50 percent, the number of refugees that will need to be settled somewhere is larger than ever before, pushing the refugee integration issue to the top of the Swedish political agenda. The study presented here provides input into an intense ongoing debate on the geography of refugee reception and its consequences. By using longitudinal data for 10 annual cohorts of refugees to Sweden, we examine whether the PoE's co-ethnic composition affected the future labor market integration of refugees from Iraq, Iran, and Somalia in the 10 years following entry, controlling for individual and local labor market characteristics. Our results show

²⁷We note that such a requirement for identifying instruments invalidates the use of neighborhood fixed effects in this application.

that the PoE neighborhood co-ethnic share makes a substantial difference in refugees' employment prospects, though with crucial differences by gender and co-ethnic context. Greater percentages of co-ethnic neighbors in the PoE significantly harm female (but not male) refugees' subsequent employment prospects unless the co-ethnics have a high rate of employment.

While we are reluctant to derive very specific policy recommendations from our study, our analytical findings may have wider implications for at least some policy considerations and the study of international migration in connection to (economic) integration of migrants in the host society. Our evidence indicates that neither a uniform dispersal policy nor a *laissez-faire* policy will lead to the best possible economic integration outcomes. Instead, we advocate a more nuanced policy involving metro/neighborhood and perhaps gendered criteria that discourages refugee settlement in certain types of places and directs/incentivizes it toward others. Moreover, it is not just the economic perspective in the metropolitan region of settlement that is crucial but, as we have shown, also the local neighborhood composition and context where migrants settle. The presence of co-ethnics and the share of employed neighborhood residents appear to play significant roles as well, especially for females. Thus, when economic integration of asylum seekers is the focus of research, the metropolitan economic structure, neighborhood composition (in terms of share of co-ethnics and of employed people), and gender at the individual level should be taken into account simultaneously.

Recently (March 2018), a special committee appointed by the Swedish government (*Mottagandeutredningen*/“The Inquiry on the Reception and Housing of Applicants for Asylum and Newly Arrived Immigrants”) launched new proposals for the organization of the refugee reception and settlement systems. The proposed new system includes features that (a) make it more difficult for municipalities to avoid engaging in the refugee placement system and (b) constrain choice for individual refugees and thereby weaken the EBO option. The proposal includes considerations about intra-municipal (neighborhood) placement strategies, but the state leaves such decisions to the municipalities.²⁸

Future studies could benefit from further improvements on the already excellent Swedish register data available for social science research. We see several obvious candidates for data to be included in future work: other outcomes besides employment (e.g., further educational attainment and earnings), more precise educational and occupational profiles for refugees, and the presence of co-ethnic entrepreneurs in neighborhoods. While including the latter can further improve our understanding of how local contexts influence economic integration, including other outcome

²⁸<http://www.regeringen.se/496383/contentassets/fd9afaf058b144d5b719c56f56cb732f/ett-ordnat-mottagande-gemensamt-ansvar-for-snabb-etablering-eller-atervandande-sou-201822.pdf>.

variables can provide evidence on local effects beyond the socially important but relatively simple dichotomy employed—not employed.

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