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# Ethnic Enclaves and Immigrant Labour Market Outcomes: Quasi-Experimental Evidence

Department of Economics

# ETHNIC ENCLAVES AND IMMIGRANT LABOUR MARKET OUTCOMES: QUASI-EXPERIMENTAL EVIDENCE[1]

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## Abstract

This study investigates empirically how residence in ethnic enclaves affects labour market outcomes of refugees. Self-selection into ethnic enclaves in terms of unobservable characteristics is taken into account by exploitation of a Danish spatial dispersal policy which randomly disperses new refugees across locations conditional on six individual-specific characteristics.

The results show that refugees with unfavourable unobserved characteristics are found to self-select into ethnic enclaves. Furthermore, taking account of negative self-selection, a relative standard deviation increase in ethnic group size on average increases the employment probability of refugees by 4 percentage points and earnings by 21 percent. I argue that in case of heterogeneous treatment effects, the estimated effects are local average treatment effects.

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# I. Introduction

Immigrants in advanced societies tend to live spatially concentrated in the larger cities, see for instance Bartel (1989) or Borjas (1998) for US evidence.

Residential segregation of immigrants is commonly believed to hamper integration of immigrants into the society. This is a key reason for which many West-European countries spatially disperse refugees and asylum seekers. Migration researchers agree that integration of immigrants into the labour market is of major importance for overall integration of immigrants into the society. It is therefore important to know how residence in ethnic enclaves affects labour market outcomes of immigrants. At least five theories exist on how living in an ethnic enclave may affect labour market outcomes of immigrants. According to one hypothesis, residence in an ethnic enclave promotes economic assimilation of recent immigrants because ethnic enclaves create closer ethnic networks that are likely to give members of the networks access to additional information in the host country. Four other hypotheses all predict that living in an ethnic enclave hampers economic assimilation of recent immigrants, by decreasing the rate of acquisition of host-country-specific human capital due to reduced social interaction with natives, by decreasing reservation wages of recent immigrants, by decreasing the quality of local public institutions such as job offices and by increasing natives' prejudice against ethnic minorities and thereby increasing labour market discrimination of ethnic minorities.

Hence, theoretically the effect of residence in an ethnic enclave on labour market outcomes of immigrants is ambiguous in sign. As a consequence, determination of the sign, as well as the size, of the causal effect of ethnic enclave size is an empirical matter.

Empirical evidence of the causal effect of ethnic enclave size on economic success of immigrants is scarce. This is probably due to difficulties of identifying the effect of ethnic enclave size. The difficulties arise because individuals sort into cities and neighbourhoods based on unobserved personal attributes that also affect their labour market outcomes.

The aim of this study is to estimate the causal effect of ethnic enclave size on the employment probability of recent immigrants, because Scandinavian welfare states face an important challenge of how to increase the employment rate of immigrants. Due to a high degree of central wage bargaining and high minimum wages relative to countries outside Scandinavia, wage assimilation of immigrants, once employed, constitutes less of a challenge (see e.g. Husted et al. 2001; Rosholm et al. 2000). In addition, the study reports estimates of the causal effect of ethnic enclave size on labour market earnings of immigrants.

The study proposes a novel way of exploiting a spatial dispersal policy on refugees and asylum seekers in order to estimate the causal effect of ethnic enclave size on socio-economic outcomes of immigrants. Ethnic enclave size is measured by the number of co-

nationals in the municipality of residence. The identification strategy is to exploit a former Danish spatial dispersal policy under which 90% of all refugees and asylum seekers were randomly assigned to locations at the time of asylum, conditional on six characteristics of the individual. Ethnic enclave size some years after immigration is instrumented by the number of co-nationals placed under the terms of the Danish spatial dispersal policy in an individual's municipality of assignment in the year of immigration and prior to the year of immigration, i.e. by the initial and past inflows of placed co-nationals. Conditional on the six characteristics of the individual which may have influenced the location of assignment, the initial and past inflow of placed co-nationals is a valid instrument for future ethnic enclave size. Furthermore, the instrument makes it possible to take account of location sorting both within and across larger geographical areas. In addition, it allows for over-identification tests for the validity of the over-identifying restrictions. Finally, the validity of our instrument is robust to differential sorting of ethnic groups into locations, i.e. that ethnic groups react to potential group-specific labour market returns to residence in a given local labour market.

The causal effect of ethnic enclave size on socio-economic outcomes of immigrants is estimated using data on refugees and asylum seekers extracted from longitudinal administrative registers of Statistics Denmark for the total immigrant population in Denmark 1984-2000. The registers provide information on country of origin, labour market attachment, earnings as well as geographic and individual information. Using a variety of specifications and samples, we consistently find strong evidence of location sorting, specifically negative self-selection of refugees and asylum seekers into ethnic enclaves. Taking account of location sorting, the average causal effect of ethnic enclave size is positive and significant. The larger the ethnic enclave, the larger the employment probability and labour market earnings of refugees. The estimated effects are economically significant in size. According to the preferred model, a relative standard deviation increase in ethnic enclave size on average increases the employment probability of refugees by on average 4 percentage points and earnings by 21 percent. The results indicate that the positive effects of ethnic networks dominate the potential negative effects of living in an ethnic enclave. The results are consistent with the hypothesis that living in an ethnic enclave facilitates access to ethnic networks that promote economic assimilation of recent immigrants by giving members of the networks access to additional information in the host country.

The final point of the paper is to demonstrate that the instrumental variables estimate of ethnic enclave size identifies an interesting policy parameter even if one relaxes the assumption of homogeneous treatment effects. In case of heterogeneous treatment effects where individuals with the same observable characteristics select into the program on the basis of the idiosyncratic component of their response to the program, the instrumental

variables estimate identifies the average effect of ethnic enclave size on the labour market outcome gain of the subgroup of refugees subject to the spatial dispersal policy who are induced to decrease their future ethnic enclave size because opting out of the dispersal programme after initial assignment to a municipality is costly due to migration costs.

## II. Theory

Several competing hypotheses exist on how residence in an ethnic enclave affects labour adjustment of adult immigrants.

Many researchers believe that ethnic enclaves create closer social networks between people of a common ethnicity living in geographical proximity of each other, henceforth referred to as ethnic networks. As noted by Bertrand, Luttmer and Mullainathan (2000), social networks affect individual behaviour through two potentially important channels: information and norms. The information channel stresses how a person's knowledge depends on the behaviour of other members of the social network, for instance due to herd behaviour/informational cascades which occur when it is optimal for an individual to follow the behaviour of the preceding individual without regard to his own information (Banerjee 1992; Bikhchandani, Hirshleifer and Welch 1992) or due to contacts to individuals in certain circumstances being crucial for attainment of some outcome. The norms channel emphasizes that an individual's behaviour may be a social custom, defined as an act whose utility to the individual performing it depends on the beliefs or actions of other members of the social network (Akerlof 1980).[3]

In this paper, focus is on the importance of ethnic networks for employment and earnings of recent immigrants. Several researchers, notably sociologists, argue that ethnic networks are likely to promote economic assimilation of recent immigrants by giving members of the networks access to additional information in the host country. The information channel is likely to operate through contacts with valuable knowledge of various kinds. Contacts may have knowledge about job vacancies, for instance in ethnic businesses or ethnic niches, or valuable information for establishment of own business such as knowledge about business and loan opportunities and knowledge about disciplined co-ethnic workers searching for a job.[4] Alternatively, contacts may disseminate information about income alternatives to employment, such as welfare eligibility. Studies of Chinatown in New York,

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[3]Such social customs may be of pecuniary disadvantage to the individual. As illustrated theoretically by Akerlof (1980), they may nevertheless persist as a result of the social sanction imposed by loss of reputation from breaking the custom.

[4]Ethnic niches emerge when an ethnic group is able to colonise a particular sector of the labour market in which members of the network are given priority to vacant positions. In the American literature examples range from jobs in restaurants and textile factories to police and fire departments and certain departments of the public administration in New York and Miami.

Little Havana in Miami and Coreatown in Los Angeles provide empirical evidence that ethnic networks constitute a crucial resource for ethnic businesses (Portes 1998, 13).

Social norms may influence employment status directly (e.g. work ethics, self-employment traditions or attitudes towards receipt of social benefits or traditional versus modern division of labour between spouses) and indirectly (e.g. norms regarding early marriage which may affect the fertility rate as well as the level of educational attainment of women) (see e.g. Coleman et al. 1966; Wilson 1987; Case and Katz 1991; Borjas 1995; Glaeser, Sacerdote and Scheinkman 1996; Bertrand et al. 2000). Such peer effects may promote or hinder economic success of ethnic minorities depending on the nature of the social norms which are prevalent in the ethnic community.

Some economists speculate that residence in an ethnic enclave slows down the rate of acquisition of host-country-specific human capital, such as host country language (Chiswick 1991; Lazear 1999). The reason is that residence in an ethnic enclave may decrease the extent to which ethnic minority members interact socially with natives, thereby decreasing the incentives for ethnic minorities to invest in the acquisition of the host-country language and increasing the costs of acquiring such language skills. Chiswick and Miller argue further that lack of host-country language proficiency reduces an individual's education opportunities and job mobility between geographical locations, occupations and employers, thereby limiting the earnings opportunities of the individual (Chiswick 1991; Chiswick and Miller 1995).

Furthermore, Chiswick and Miller speculate that living in an ethnic enclave has a negative effect on earnings of ethnic minorities for another reason. They argue that ethnic minorities will be willing to accept a job at a lower wage if the job is located in an ethnic enclave, because residence in an ethnic enclave reduces the cost of consumption of ethnic goods and services such as traditional food products from the source country, clubs for co-nationals and places for practising religion (Chiswick and Miller 1995).[5]

In addition, an increase in the share of ethnic minorities in a neighbourhood may decrease the quality of local public institutions such as job centers unless budgets of local public institutions are increased to cover the increased work load arising from language and cultural barriers between public employees and users. Deterioration of the quality of local public institutions may turn into a self-reinforcing process if the best employees decide to change workplace to a workplace with more resources.

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[5]One may object to this hypothesis by noting that in standard search models a relatively low reservation wage implies a relatively high job offer acceptance probability. In that case the short-run effect of living in an ethnic enclave on individual annual earnings is ambiguous, because annual earnings are the product of hours of work throughout the year - which may increase - and hourly wages - which may decrease. Furthermore, the medium-run effect of living in an ethnic enclave may be unambiguously positive because hourly wages are increasing in work experience.

Finally, according to the spatial mismatch hypothesis by Kain (1968), residential segregation of Blacks in US cities has reduced job opportunities of Blacks. Kain's spatial mismatch hypothesis for Blacks in U.S. cities may apply more generally as a description of how residential segregation of ethnic minorities hurts economic success of ethnic minorities. On the supply side, there may be a mismatch between the location of ethnic enclaves and workplaces. Working outside an ethnic enclave may involve so high commuting costs and time so as to deter ethnic enclave members from searching for a job where vacant jobs are located. In addition, ethnic enclave members may receive less information about job vacancies distant from the ethnic enclave. On the demand side, prejudices against other ethnic groups may be reinforced by separate living of ethnic groups. As a consequence, native employers may discriminate against ethnic minorities due to actual or imaginary fear for retaliation from native customers for bringing ethnic minorities into neighbourhoods for natives only. Similarly, employers in ethnic enclaves may prefer job applicants from their own ethnic group.

The short description of the five hypotheses mentioned above demonstrates that theoretically the effect of residence in an ethnic enclave on labour market outcomes of immigrants is ambiguous in sign. Therefore, the effect of living in an ethnic enclave on labour market outcomes of immigrants must be determined by empirical analysis to which we now turn.

## III. Methodology

### III.A. Empirical Framework

Suppose the true data generating model for employment status is given by

$$y_i = \text{I}(y_i^* > 0) \tag{1}$$

$$y_i^* = \gamma e_i + X_{1i}\beta_1 + \varepsilon_i \tag{2}$$

where $y_i$ is an observable indicator variable equal to 1 if individual $i$ is employed and 0 otherwise, $y_i^*$ is an unobserved latent random variable which is a function of an observed scalar variable $e_i$ which is a measure for ethnic enclave size of individual $i$, a vector of observed and unobserved personal, ethnic group and (other) local area characteristics, $X_{1i}$, and an error term $\varepsilon_i$. $\gamma$ is the key parameter of interest. It is meant to capture the average effect of ethnic enclave size on the employment probability.

Estimation of this model poses omitted variables problems, because individuals choose where to live, i.e. the ethnic enclave size. For example, individuals who live in a relatively large ethnic enclave may differ in some unobserved ways from individuals who live in a relatively small ethnic enclave.

Previous literature has used different empirical strategies to identify the effect of residential segregation of ethnic minorities on socio-economic outcomes of ethnic minorities.

Two previous studies, Cutler and Glaeser (1997) and Bertrand, Luttmer and Mullainathan (2000), avoid the issue of within-city sorting by ability by exploiting the variation across a larger geographical area. Cutler and Glaeser (1997) examine the effects of residential segregation on outcomes for blacks in US cities in schooling, employment and single parenthood by examining whether outcomes for blacks as a whole are better or worse in cities that are less racially segregated. They address the issue of reverse causality where poor outcomes lead to increased segregation by instrumenting segregation across cities by the structure of local government finance and topographical features of the city that affect segregation. They address the issue of cross-metropolitan mobility by examining the effect of location early in life on adult outcomes.

Bertrand et al. (2000) investigate how access to ethnic networks affects social welfare dependency of ethnic minorities. They suggest to avoid omitted variables bias from omitted neighbourhood characteristics (e.g. differences in job availability and administrative welfare eligibility practices) and ethnic group characteristics (e.g. discrimination) by inclusion of neighbourhood and ethnic group fixed effects. The remaining potential omitted bias stems from omitted personal characteristics. It arises if the individuals differentially self-select away from their ethnic group in the host country. Bertrand et al. (2000) take such omitted variables bias into account by exploiting the variation across a larger geographical area.

However, as pointed out by Edin, Fredriksson et Åslund (2003), instruments that exploit the variation across a larger geographical area only take selection *within* the larger geographical areas into account, while they ignore the potential selection across larger geographical areas. Furthermore, inclusion of neighbourhood fixed effects for the *current* neighbourhood does not take all sorting across locations into account, because the choice of the current neighbourhood is an endogenous outcome which is likely to be correlated with unobserved characteristics of the individual.

The ideal data for empirical determination of the effect of ethnic enclave size on socio-economic outcomes of ethnic minorities would be a randomized experiment in which ethnic minorities were randomly distributed across locations and persuaded to stay in the location of assignment for a considerable period of time. Such data rarely exist. However, quasi-experimental evidence may exist for countries which have implemented spatial dispersal policies on refugees and asylum seekers aiming at settlement of refugees and asylum seekers outside the immigrant-dense cities.

Edin et al. (2003) argue that almost all refugees and asylum seekers were randomly assigned to locations at the time of asylum under a former Swedish spatial dispersal policy on refugees and asylum seekers and exploit the resulting quasi-experimental data

to examine the effect of ethnic enclave size on earnings of immigrants. Their measure for ethnic enclave size is the number of co-nationals living in individual $i$'s municipality of residence. Ethnic enclave size eight years after asylum is instrumented by the initial ethnic enclave size, i.e. the stock of co-nationals in an individual's municipality of assignment at the time of asylum. The instrument has two strengths. First, it takes account of location sorting both within and across larger geographical areas. Second, omitted neighbourhood characteristics are captured by inclusion of fixed effects of the municipality of assignment rather than of the current municipality of residence.

I will argue that a former Danish spatial dispersal policy on refugees and asylum seekers also provide quasi-experimental evidence on the effect of ethnic enclave size on labour market outcomes of immigrants. I will estimate the effect of ethnic enclave size on labour market outcomes of immigrants along the lines of Edin et al (2003). However, I will argue that better candidates for exclusion restrictions exist than the initial ethnic enclave size.

Following Edin et al. (2003) I will measure the ethnic enclave size, $e_i$, by the ethnic group size of individual $i$ which is defined as the number of co-nationals living in individual $i$'s municipality of residence. Hence, implicitly *ethnicity* is measured by country of origin following Borjas (1992, 1995, 1998). Co-nationals are first and second generation immigrants from individual $i$'s country of origin. The implicit definition of an ethnic enclave underlying the empirical analysis is that individual $i$ lives in an ethnic enclave if he lives in a municipality in which the number of co-nationals exceeds a given threshold.

I will estimate the model given by Equations (1) and (2) using a panel of administrative register data for refugees and asylum seekers who were subject to the former Danish spatial dispersal policy. Specifically, I will estimate the model seven years after asylum, at which time individuals are recent immigrants but have had time for establishment of social networks in the host-country and acquisition of basic skills necessary for job search in the host country. $X_1$ will include a vector of observed personal characteristics at the time of immigration which may affect labour market outcomes of the individual. In addition, $X_1$ contains three types of fixed effects to capture omitted variables bias in $\hat{\gamma}$ stemming from calendar time effects, omitted ethnic group characteristics, i.e. the "quality" of the ethnic enclave, and omitted local area characteristics: 1) year of immigration fixed effects, 2) ethnic group fixed effects and 3) fixed effects for the municipality to which an individual was assigned by the authorities at the time of asylum.

The model may still suffer from omitted variables bias to the extent that individuals with certain unobserved characteristics, e.g. poor abilities or lack of ambitions about socio-economic assimilation in the host country, are overrepresented in locations with a relatively large ethnic group size. I propose to take account of self-selection into ethnic enclaves by exploiting a former spatial dispersal policy on refugees and asylum seekers in

Denmark to find a valid and strong instrument of the potentially endogenous explanatory variable, the ethnic group size of individual $i$ seven years after immigration. I now turn to a description of the aims and the actual implementation of that policy.

*III.B. The Natural Experiment*

1986 marks the start of the first Danish spatial dispersal policy on refugees and asylum seekers who had just received a permit to stay for reasons of asylum.[6] Henceforth, I refer to such recognised refugees and asylum seekers as refugees. The Danish Government urged the Danish Refugee Council to implement the dispersal policy after a surge of refugees in the mid-eighties made it increasingly difficult for the Council to satisfy the location preferences of most new refugees for accommodation in the larger cities. The policy was in force until 1999 under the charge of the Council.

Spatial dispersal was a two-stage process. At the country level, the Council aimed at the attainment of an equal number of refugees relative to the number of inhabitants across counties. At the county level, the Council aimed at attaining an equal number of refugees relative to the number of inhabitants across municipalities (local authority districts) with suitable facilities for reception such as housing, educational institutions, employment opportunities and co-nationals.[7] In practice, these dispersal criteria implied that refugees were provided with permanent housing in cities and towns and to a lesser extent in the rural districts (Ministry of Internal Affairs 1996). In 1987, 243 out of a total of 275 municipalities in Denmark had received refugees (Danish Refugee Council 1987).

In practice, the settlement took place in three steps. First, as soon as a refugee was granted asylum, the individual was offered assistance from the Council in finding housing. If the individual accepted the offer, he/she filled in a form concerning his/her background such as nationality.[8] Second, approximately 10 days later the Council assigned a refugee to one of 15 counties. Third, having been provided with temporary housing in the receiving county, local offices of the Council assisted the assigned refugees in finding permanent housing in the county. The local offices were mobile within a county. They typically stayed in a local area for about 3 years focusing on finding permanent housing for assigned

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[6]Until June 2002, Denmark gave asylum to Convention refugees, i.e. persons who were defined as refugees according to the Geneva Convention of 1951, and to foreigners who were not defined as refugees according to the Geneva Convention, but who for similar reasons as stated in the Convention or other weighty reasons should not be required to return to the home country ('de facto' refugees). [Coleman and Wadensjö 1999, 249].

[7]In order to facilitate local refugee reception, the Council made an effort only to place one or two different nationalities in small municipalities whereas larger municipalities would be assigned more nationality groups of refugees. Refugees were expected to benefit from this practice as well, since it enabled them to live near co-nationals irrespective of the size of the municipality of assignment.

[8]In the first years after the introduction of the dispersal policy the form also contained questions about any location wishes and their reasons.

refugees in that particular area of the county in order to facilitate establishment of ethnic networks and give refugees easy access to services provided and activities offered by the local office.

Dispersal was voluntary in the sense that only refugees who were unable to find housing themselves were subject to the dispersal policy. However, the take-up rate was high; between 1986 and 1997 approximately 90% of refugees were provided with permanent housing by the Council (or after 1995 by a local government) under the terms of the dispersal policy (Annual Reports of the Danish Refugee Council 1986-1994 and the Council's internal administrative statistics for 1995-1998).

Once settled, refugees participated in Danish language courses during an introductory period of 18 months while receiving social assistance. Refugees were urged to stay in the assigned municipality during the entire introductory period. However, there were no relocation restrictions. Refugees could move away from the municipality of assignment at any time, in so far as they could find alternative housing elsewhere. Receipt of welfare was unconditional on residing in the assigned municipality.

The dispersal policy did, at least in the short run, influence the location pattern of refugees. In 1993, the settlement pattern of refugees resembled that of the Danish population and differed greatly from that of non-western immigrants. 33% of refugees and 26% of the Danish population lived in the capital or its suburbs while as much as 71% of non-western immigrants lived there. 56% of refugees and 59% of the Danish population lived in towns outside the capital as opposed to only 24% of non-western immigrants. The remaining shares lived in rural districts (Danish Refugee Council 1993).

The important question of whether refugees were randomly distributed across locations under the spatial dispersal policy is analysed in a related study, Damm (2005). The study examines the initial settlement pattern of refugees who got asylum between 1986 and 1998 based on a range of data sources: an interview with two placement officers at the Council, the Council's internal administrative statistics and administrative registers. The study concludes that the Danish spatial dispersal policy on refugees carried out between 1986 and 1998 gave rise to a *random initial distribution* of refugees who were provided with or assisted in finding permanent housing by the Council, *conditional* on six characteristics of the individual: health (in need of special medical or psychiatric treatment), educational needs, location of close relatives, family size (single or not), nationality as well as year of immigration. Refugees in need of special treatment or special education, refugees who insisted on living near close relatives in Denmark, married refugees with children and refugees who immigrated early in the dispersal policy period may have been more likely to realise their preferred settlement option than other refugee groups. The main reasons are given below.

First, an interview with two former placement officers at the Council reveals that

the Council aimed at satisfying location wishes of refugees who wished to be assigned to a location near close family members and at assigning refugees who were in need of special medical or psychological treatment or education to a location in which the required treatment or educational facilities were present.

Secondly, the Council's internal administrative statistics on the length of stay in temporary housing in the years 1988-1996 suggest that it was typically a somewhat more difficult task to find permanent housing for singles than for refugee families. On average a refugee lived in temporary housing 6-7 months after assignment to a county, but the duration depended on the local housing market situation. In almost every year a larger share of singles than refugees with family waited more than 9 months for permanent housing. Only 0-4 % had not found permanent housing within the introductory period of 18 months (Administrative statistics of the Danish Refugee Council for the years 1992-1997 and annual reports of the Danish Refugee Council for the years 1986-1994). Concerning the location of the permanent dwelling, for practical reasons it was typically located near the temporary dwelling, not necessarily in the same municipality but at the least in a nearby municipality.

Thirdly, over time it became increasingly difficult for the Council to find vacant rental housing units in the larger and medium-sized towns which suggests that refugees who arrived in the first years after the introduction of the dispersal policy may have been more likely to realise their potential location wish. Note however, that the location wish of refugees to live in a larger city soon became less pronounced after the implementation of the dispersal policy. The Council's Annual Report 1986 contains a paragraph on this issue. The translation of the paragraph reads: "The former wish among refugees to be assigned to certain larger cities no longer poses significant problems. In general refugees have been welcomed by the local community and experienced the conditions in a small community to be at least as good and of the same kind as offers available in the larger cities" (Danish Refugee Council 1986).

Finally, refugees from certain source countries seem to have been less likely to be assigned to a larger city. Empirical evidence from administrative registers presented in Damm (2005) show that this is the case for refugees from Sri Lanka, Palestinian refugees from Lebanon and in particular for refugees from Bosnia-Herzegovina who were dispersed under a special introduction programme that included settlement in rural districts.

Note, however, that the way in which the dispersal policy was implemented implied little opportunity for municipalities to cream-skim refugees, i.e. to express a wish for receiving, for instance, well-educated refugees. First, the Council placed refugees in temporary housing shortly after a refugee had received his/her residence permit and the Council did not know which groups of refugees would next receive residence permits. This procedure left little time for negotiation between the Council and municipalities. Furthermore, the

Council acted as a private agent searching for housing in the local housing market on be-half of refugees who had just received a residence permit. The local authorities typically weren't informed about the relocation of a refugee to the municipality until after a refugee had been provided with housing in the municipality. There is some empirical evidence to back this claim. Linear regression of the number of inhabitants in the municipality of assignment on a range of characteristics of the individual shows absence of a correlation between the size of the municipality of assignment and an individual's educational level.[9]

Three of the six characteristics which may have influenced the location of assignment of a refugee are observable in longitudinal administrative registers of Statistics Denmark on the total immigrant population in Denmark 1984-2000: family status (marital status and number of children), nationality and year of immigration. In addition, the registers contain variables which may be good proxies for two of the three unobservable characteristics: age and nationality may be decent proxies for an individual's educational need and nationality and the size of the ethnic stock, defined as the total number of co-nationals in Denmark, at the time of immigration may be decent proxies for the likelihood of having close relatives in Denmark at the time of immigration. In conclusion, one potentially important individual characteristic for initial settlement is unobserved in the Immigrant Data Set: health status at the time of immigration.

*III.C. Instrumenting ethnic group size*

The model in which the ethnic group size seven years after immigration is instrumented corresponds to rewriting the model given by Equations (1) and (2) as a simultaneous equation system with a structural equation for the endogenous variable of interest, $y_i^*$, and a reduced-form equation for the endogenous explanatory variable, $e_i$. Formally, the system is written as follows:

$$y_i = \text{I}(y_i^* > 0) \tag{3}$$
$$y_i^* = \gamma e_i + X_{1i}\beta_1 + \varepsilon_i \tag{4}$$
$$e_i = X_i\Pi + \upsilon_i \tag{5}$$
$$i = 1,...,n$$

where $X_i$ includes $X_{1i}$ and the instrument, $Z_i$. Conditional on $X_{1i}$, the error terms of Equations (4) and (5) are assumed to be multivariate normal,

$$(\varepsilon_i, \upsilon_i) = N(0, \Sigma) \tag{6}$$

This model is a binary choice model with one endogenous continuous explanatory variable which is a special case of the cross-sectional limited dependent models with en-

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[9]The estimation results are available from the author upon request.

dogenous explanatory variables discussed in Heckman (1978), Amemiya (1978), Smith and Blundell (1986), Blundell and Smith (1989) who propose different consistent estimators. Three types of consistent instrumental variables estimators are suggested: Heckman Two-Step (Heckman 1978) and the closely related instrumental variables Probit estimator (Lee 1981), Amemiya's Generalized Least Squares (AGLS) (Amemiya 1978, Newey 1987) and Two-Stage Conditional Maximum Likelihood (Smith and Blundell 1986, Rivers and Vuong 1988). An alternative consistent - and efficient - estimator for such a model is joint Maximum Likelihood discussed in Amemiya (1978).

I will use Two-Stage Conditional Maximum Likelihood (2SCML) to test for weak exogeneity of ethnic group size seven years after immigration, $e_i$, and AGLS to estimate the model given by the Equations (3)-(6).

Turning to the important issue of the existence of identifying variables, I argue that the number of adult co-nationals placed in the municipality of assignment in the year of asylum and prior to the year of asylum constitute *valid* exclusion restrictions in estimation of the effect of ethnic group size on the employment probability seven years after immigration, controlling for all characteristics of the individual which may have influenced initial assignment to a municipality of residence. Unfortunately I am unable to include one characteristic of the individual which may have affected the initial assignment as well as labour market outcomes of the individual in $X_1$: the individual's health status at the time of asylum. Individuals in need of special psychological/psychiatric treatment at the time of immigration may have been more likely to be settled in a large city and therefore more exposed to other immigrants initially than others, ceteris paribus. Such individuals are also less likely to be employed initially. However, for at least two reasons this unobserved characteristic may not be of concern in the analysis. First, individuals who received psychological treatment at the time of settlement are likely to constitute a minor fraction of the sample.[10] Second, the correlation between number of inhabitants in the municipality of assignment and the number of co-nationals in the municipality of assignment is moderate. There are two reasons for that. Under the dispersal policy co-nationals tended to be dispersed across locations in ethnic clusters, and for almost all ethnic groups in the sample, the number of co-nationals who had arrived prior to the introduction of the dispersal policy and settled in the larger cities was small.

The instrument will contain the subset of the candidates for identifying variables that gives the best linear predictor among the instruments for which the test of over-identifying variables cannot be rejected. This instrument will be a good predictor for ethnic enclave

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[10]In an interview with the Danish national newspaper, Politiken, social worker Bente Midtgård, Rehabilitation and Research Centre for Torture Victims, Denmark, said that new refugees are not examined for complications due to torture. Therefore, no official numbers exist on the number of new refugees who have been subject to torture. (Politiken, 1st Section, p. 4, 5th of December 2003).

size seven years after asylum to the extent that a considerable share of refugees have remained settled in the municipality of assignment.

Furthermore, the instrument has two strengths relative to the instrument used by Edin et al. (2003), initial ethnic group size. First, the validity of the instrument can be tested by performing an over-identification test, because the instrument contains more than one identifying variable. Any identifying strategy that relies on an assumption of orthogonality between the instrument and the error terms should aim at testing the validity of over-identifying restrictions. Second, the validity of the instrument is robust to differential sorting of ethnic groups into locations, i.e. that ethnic groups relocate into different locations after initial assignment based on potential group-specific labour market returns to residence in a given local labour market. In case of differential sorting of ethnic groups into locations, the initial ethnic group size of individual $i$ may be the result of the relocation pattern of past cohorts of initially placed co-nationals rather than a result of random assignment of refugees under the dispersal policy.[11]

*III.D. Data*

Micro data on refugees is extracted from longitudinal administrative registers of Statistics Denmark on the immigrant population in Denmark 1984-2000. The refugee sample is a balanced panel of 13,927 individuals who are observed annually in the registers from the time of receipt of a permit to stay, also referred to as the time of immigration, until seven years after immigration.[12]

Ideally the sample should cover observations on all adult refugees who were assigned to a municipality by the Council under the terms of the spatial dispersal policy in place from 1986 to 1998. However, information on admission category of immigrants and the assignment municipality of refugees is missing in the registers. I take account of the first issue by applying an algorithm based on country of origin and the first year of residence permit to Denmark to extract individuals from 11 main refugee-sending countries. The algorithm was constructed from official figures on the annual number of residence permits granted to asylum-seekers by country of origin. Solving the second data issue is further complicated by the fact that refugees may initially have lived in temporary housing in

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[11]In case of differential sorting of ethnic groups into locations, the initial ethnic group size would only be a valid instrument for future ethnic group size if interaction terms between municipality of assignment fixed effects and ethnic group fixed effects were included as controls in Equation (2). In the present study that would amount to large reduction of the number of degrees of freedom, in particular by 272 (municipality of assignment indicator variables) times eleven (ethnic group indicator variables) degrees of freedom.

[12]Permanent return-migrants who emigrated prior to seven years of residence in Denmark and 18 individuals who were observed in the registers seven years after immigration but not annually up to that point are excluded from the sample.

proximity of the municipality to which they were later assigned, on average after 1 year. I identify the municipality of assignment by using a rather complicated algorithm which I constructed based on information on the Council's internal administrative statistics on temporary housing. I define the first municipality of residence observed in the registers as a municipality of temporary housing if the person relocates to another municipality within the county within one year after receipt of the first permit of residence. Otherwise the first municipality is defined as the municipality of assignment.

Furthermore, we want to exclude family-reunified immigrants from refugee-sending countries, because they were not subject to spatial dispersal, unless they immigrated shortly after their spouse. I, therefore, exclude immigrants from refugee-sending countries, who at the time of immigration were married to either 1) a Dane, 2) an immigrant from a non-refugee-sending country or 3) an immigrant from a refugee-sending country who had immigrated at least one year earlier. Unfortunately the registers do not allow us to exclude the 10% of refugees who turned down the Council's offer of housing under the terms of the spatial dispersal policy. Finally, I include only individuals aged 18-59.

The refugee sample has rich information on demographic and socio-economic characteristics of each individual, most importantly labour market status in November of each year and annual labour market earnings. An individual is regarded as being employed, if his main occupation is wage-employment with at least 9 hours of weekly work or self-employment. The employment measure therefore includes part-time work of at least 9 hours of weekly work as well as full-time work. Real annual labour market earnings, henceforth referred to as real annual earnings, are defined as the sum of wage earnings, profits from own company and sickness benefits deflated by the consumer price index which has 1980 as its base year.

Further variable definitions are reported in Table A.I in the Appendix.

*III.E. Summary Statistics*

Seven years after immigration, the employment rate of the overall sample of refugees is 0.29. However, the reported summary statistics in Table I show that the employment rate varies substantially between ethnic groups and gender. For men, the employment rate seven years after immigration varies between 0.24 for Palestinian refugees from Lebanon and 0.56 for Rumanians. For women, the employment rate seven years after immigration varies between 0.06 for Palestinian refugees from Lebanon and 0.5 for Rumanian and Chilean refugees. For comparison, the employment rate of the overall Danish population aged 18-59 fluctuated between 0.78-0.84 for men and 0.66-0.69 for women in the period 1986-1998.

Turning to real annual earnings, 5,647 individuals have positive real annual earnings seven years after immigration. Conditional on having positive earnings, they earn on

Table I

Summary statistics for the dependent variables by ethnic group.

| | No. of individuals | Employment rate | | Mean real annual earnings | |
|---|---|---|---|---|---|
| Ethnic group: | | Men | Women | Men | Women |
| Poland | 393 | .52 (.50) | .42 (.49) | 76,945 (49,689) | 61,798 (42,985) |
| Iraq | 2,213 | .32 (.47) | .13 (.33) | 54,308 (49,890) | 50,685 (49,582) |
| Iran | 2,829 | .34 (.47) | .19 (.39) | 42,663 (37,633) | 37,168 (33,315) |
| Vietnam | 1,331 | .48 (.50) | .24 (.43) | 64,571 (39,421) | 48,410 (31,288) |
| Sri Lanka | 1,770 | .54 (.50) | .34 (.47) | 58,716 (39,770) | 41,381 (28,571) |
| Lebanon | 3,867 | .24 (.43) | .06 (.24) | 40,872 (41,075) | 47,940 (41,119) |
| Ethiopia | 146 | .25 (.44) | .26 (.44) | 50,673 (41,275) | 41,696 (21,366) |
| Afghanistan | 268 | .37 (.48) | .16 (.37) | 47,594 (43,023) | 50,977 (39,075) |
| Somalia | 872 | .29 (.46) | .09 (.29) | 52,591 (42,401) | 42,506 (38,891) |
| Rumania | 216 | .56 (.50) | .5 (.50) | 76,283 (52,911) | 57,547 (35,849) |
| Chile | 22 | .36 (.50) | .5 (.53) | 64,003 (45,290) | 50,448 (19,319) |
| All | 13,927 | .35 (.48) | .18 (.38) | 52,154 (43,026) | 46,026 (37,138) |

Notes: Standard deviations are reported in parentheses. Real annual earnings are reported in the Danish currency, Danish Kroner, for the subsample of refugees with positive earnings seven years after immigration. Refugees from Lebanon are Palestinian refugees with no citizenship.

average 50,774 Danish Kroner, henceforth abbreviated as DKK. Unsurprisingly, mean real annual earnings also vary a lot between ethnic groups and gender. For men, mean real annual earnings seven years after immigration range from 40,872 DKK for Palestinian refugees from Lebanon to 76,945 DKK for Polish refugees. For women, mean real annual earnings range from 37,168 DKK for Iranians to 61,798 DKK for Poles. For comparison, mean real annual earnings conditional on having positive earnings of the overall Danish population aged 18-59 fluctuated between 114,644 and 126,951 DKK for men and 68,230 and 86,066 DKK for women in the period 1986-1998.

Table II shows summary statistics for highest completed educational level and mean ethnic group size seven years after immigration by ethnic group which may help explain the variation in employment rate and mean real annual earnings between ethnic groups.[13] The fraction of sampled individuals who is known to have completed a professional degree of education varies from 0.06 for Vietnamese refugees to 0.28 for Poles and Rumanians. The summary statistics for ethnic group size seven years after immigration by ethnic group

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[13]Unfortunately information on the highest completed educational level is missing for between 44% and 67% of the sampled individuals in each ethnic group. These are individuals who have not completed any education in Denmark seven years after immigration, but they may have completed foreign education.

Table II

Summary statistics for selected variables seven years after immigration by ethnic group.

| Ethnic group: | Highest degree of education attained. Fractions. | | | | Ethnic group size |
| | No degree | High school | Professional | Unknown | |
| --- | --- | --- | --- | --- | --- |
| Poland | .04 (.19) | .10 (.30) | .28 (.45) | .58 (.49) | 731 (957) |
| Iraq | .05 (.22) | .24 (.43) | .27 (.45) | .43 (.50) | 1,407 (1,496) |
| Iran | .05 (.21) | .23 (.42) | .18 (.38) | .54 (.50) | 1,063 (999) |
| Vietnam | .10 (.30) | .22 (.41) | .06 (.24) | .62 (.48) | 770 (687) |
| Sri Lanka | .05 (.22) | .37 (.48) | .09 (.28) | .49 (.50) | 161 (154) |
| Lebanon | .10 (.30) | .28 (.45) | .09 (.29) | .53 (.50) | 1,280 (1,307) |
| Ethiopia | .08 (.26) | .15 (.36) | .10 (.30) | .67 (.47) | 180 (136) |
| Afghanistan | .03 (.16) | .28 (.45) | .26 (.44) | .44 (.50) | 223 (205) |
| Somalia | .08 (.28) | .24 (.43) | .16 (.37) | .52 (.50) | 1,482 (1,165) |
| Rumania | .01 (.10) | .12 (.33) | .28 (.45) | .59 (.49) | 83 (82) |
| Chile | .05 (.21) | .32 (.48) | .09 (.29) | .55 (.51) | 156 (146) |
| All | .07 (.25) | .26 (.44) | .15 (.36) | .52 (.50) | 1,010 (1,177) |

Notes: Standard deviations are reported in parentheses. Refugees from Lebanon are Palestinian refugees with no citizenship.

also reported in Table II show that mean ethnic group size varies from 83 for Rumanians to 1,482 for Somalis.

Summary statistics for the full sample are reported in Table A.II in the Appendix.

III.F. Natural Variation in Ethnic Group Size

Did the spatial dispersal policy provide 'natural' variation in the ethnic enclave size of refugees? To answer this question I first compare the initial settlement pattern of refugees who were granted a permit to stay before the implementation of the spatial dispersal policy with the initial settlement pattern of refugees in the sample, henceforth referred to as pre-reform refugees.

Initial settlement of pre-reform refugees is defined as their settlement one year after immigration. This definition of initial settlement allows us to use three pre-reform cohorts of refugees as comparison groups, the 1983-1985 cohorts of refugees from the 11 main refugee-sending countries between 1986 and 1997. In addition, it takes into account that pre-reform refugees may initially have lived in temporary housing in another municipality than the initial municipality of permanent housing.

Comparison of the geographical settlement pattern of the pre-reform refugees with that of refugees in the refugee sample indicates that the dispersal policy on refugees affected the initial settlement pattern of refugees considerably.

Table III

Mean ethnic group size in the year of immigration.

| Refugee cohort | No. of individuals | Mean |
|---|---|---|
| 1983 cohort | 359 | 395.4 (555.2) |
| 1984 cohort | 1,277 | 288.3 (389.9) |
| 1985 cohort | 4,309 | 190.3 (285.1) |
| 1986 cohort | 3,657 | 132.6 (258.9) |
| 1987 cohort | 1,855 | 224.3 (350.9) |
| 1988 cohort | 1,430 | 255.9 (334.9) |
| 1989 cohort | 1,651 | 268.0 (315.5) |
| 1990 cohort | 1,215 | 335.8 (337.4) |
| 1991 cohort | 1,343 | 337.4 (470.5) |
| 1992 cohort | 1,587 | 422.5 (550.5) |
| 1993 cohort | 1,189 | 381.2 (515.5) |
| Refugee sample | 13,927 | 265.3 (399.3) |

Notes: Standard deviations are reported in parentheses.

First, the dispersal policy increased the geographical dispersion of refugees across municipalities. 60%, i.e. the majority, of the 1983 and 1984 cohorts initially lived in one of the four largest municipalities as compared to only 31.5% of refugees in the refugee sample. In addition, both the share of refugees who were initially settled in a small municipality and the share of refugees who were initially settled in a medium-sized municipality increased substantially. 2.7% of the 1983 and 1984 cohorts initially lived in a small municipality as compared to 9.3% of refugees in the refugee sample while 37.6% of the 1983 and 1984 cohorts initially lived in a medium-sized municipality as compared to 59.2% of refugees in the refugee sample. Note however, that the tendency towards increased dispersion across municipalities started with the 1985 cohort of refugees, that is one year prior to implementation of the dispersal policy. This was caused by lack of housing in the large cities in 1985.

Secondly, Table III presents evidence that the dispersal policy reduced the mean initial ethnic group size of refugees. This is seen by noting that the mean initial ethnic group size of the 1986-1988 cohorts of refugees is significantly lower than the mean initial ethnic group size of the 1983 and 1984 cohorts of refugees despite an increase in ethnic stock over time of all ethnic groups as documented in Table A.III in the Appendix. This increase in ethnic stock together with the fact that the Council aimed at dispersing refugees in clusters of co-nationals explains the increase in the mean initial ethnic group size over time and cohorts in spite of continued spatial dispersal of new cohorts of refugees.
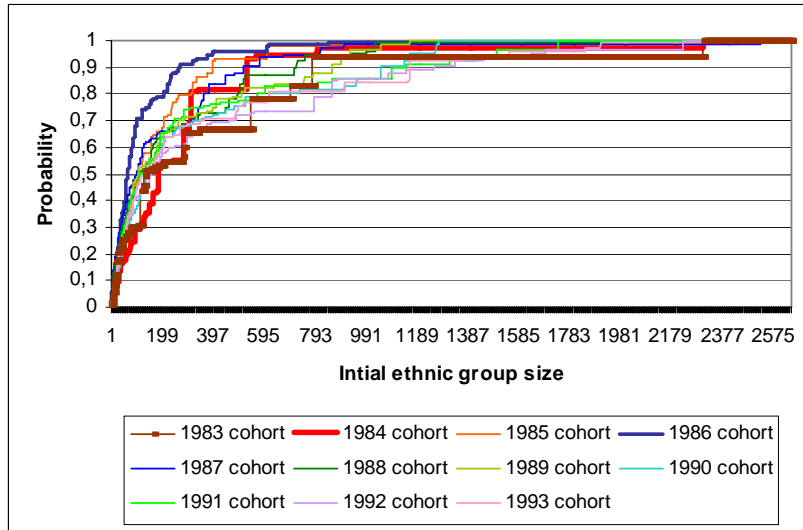
Figure 1: Cumulative distribution function for initial ethnic group size. By refugee cohort.

Over time the cumulative distribution function of post-reform refugees shifts to the right due to the increase in ethnic stock over time and due to dispersal of refugees in clusters of co-nationals, but despite increases in ethnic stocks over time the cumulative distribution function of the 1983 cohort dominates that of the 1986-1988 cohorts and the cumulative distribution function of the 1984 cohort dominates that of the 1986 cohort.

There is ample evidence that the dispersal policy affected the settlement pattern of refugees even in the medium-run.

First, post-reform refugees are still overrepresented in small and medium-sized municipalities relative to pre-reform refugees seven years after immigration. Seven years after immigration 45% of individuals in the refugee sample live in a large municipality as compared to 58 and 64% of the 1983 and 1984 cohorts of refugees, respectively.

Finally, evidence is presented in Figure 1 that the dispersal policy shifted the overall cumulative distribution function of initial ethnic group size to the left, especially at the top of the distribution. The shift is most spectacular for the 1986 cohort of refugees relative to the 1983 and 1984 cohort of refugees.

Second, as reported in Table IV seven years after immigration the 1986 cohort on average still has a significantly lower ethnic group size than 1984 and 1985 cohorts of refugees. In contrast, 1987-1993 cohorts of refugees on average have a significantly higher ethnic group size than pre-reform cohorts of refugees.

Third, Figure 2 shows that the cumulative distribution function for ethnic group size seven years after immigration of 1983 and 1984 cohorts of refugees lies to the right of that of the first cohorts of post-reform refugees for low and intermediate ethnic group size. In other words, seven years after immigration the first cohorts of post-reform refugees have

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Table IV

Mean ethnic group size seven years after immigration.

| Refugee cohort | No. of individuals | Mean |
|---|---|---|
| 1983 cohort | 374 | 639.3 (645.4) |
| 1984 cohort | 1,142 | 790.0 (643.7) |
| 1985 cohort | 3,863 | 675.4 (701.6) |
| 1986 cohort | 3,657 | 600.8 (781.2) |
| 1987 cohort | 1,855 | 859.5 (943.3) |
| 1988 cohort | 1,430 | 930.2 (991.5) |
| 1989 cohort | 1,651 | 1,078.3 (1,131.2) |
| 1990 cohort | 1,215 | 1,153.5 (1,214.3) |
| 1991 cohort | 1,343 | 1,219.6 (1,304.7) |
| 1992 cohort | 1,587 | 1,414.6 (1,462.5) |
| 1993 cohort | 1,189 | 1,580.1 (1,595.3) |
| Refugee sample | 13,927 | 1,009.9 (1,176.9) |

Notes: Standard deviations are reported in parentheses.

a higher tendency to live in municipalities with a low or intermediate ethnic group size relative to pre-reform refugees.

The evidence presented in this subsection shows that on average refugees who were subject to the dispersal policy received lower levels of treatment than they would have received otherwise. In other words, the dispersal policy created natural variation in the ethnic enclave size of refugees.

## IV. Empirical Results

*IV.A. Employment Effects of Ethnic Group Size*

Table V reports the estimation results of the model given by Equations (1) and (2) in which ethnic group size seven years after immigration is treated as a weakly exogenous explanatory variable. The model is estimated using the probit estimator, i.e. under the assumption that the error term in Equation (2) is n.i.d. distributed. The probit estimates using the full sample show that controlling for other observed characteristics of the individual and for three types of fixed effects, the logarithmic value of ethnic group size on average has a significantly negative effect on the employment probability of the individual. Next, the sample is divided into two sub-samples, for low-educated and higher-educated individuals respectively. The sub-sample of higher-educated individuals is constituted by individuals who have completed at least thirteen years of education. On average the logarithmic value of ethnic group size has a significant negative effect on the
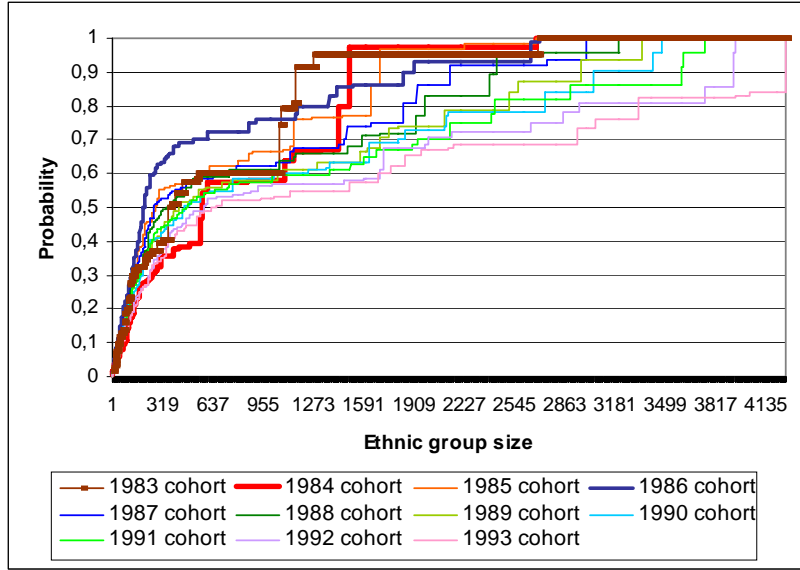
Figure 2: Cumulative distribution function for ethnic group size seven years after immigration. By refugee cohort.

employment probability for both sub-samples. That is, according to the probit estimates, current residence in an ethnic enclave is harmful to employment of refugees, irrespective of educational level.

We now test whether ethnic group size seven years after immigration is in fact weakly exogenous in the model given by Equations (1) and (2). This is tested using an instrument. The instrument contains the subset of the candidates for identifying variables of the effect of ethnic group size that gives the best linear predictor among the instruments for which the test of over-identifying variables cannot be rejected.

Summary statistics of the eight candidates for identifying variables, the number of co-nationals placed in the municipality of assignment of individual $i$ in year $t$, $t = -7, -6, ..., 0$, where $t$ denotes the number of years since immigration of individual $i$, are presented in Table A.II in the Appendix. The mean value of the number of co-nationals placed in the municipality of assignment is seen to increase with $t$. The reason is that a substantial share of refugees were assigned to municipalities which had received cohorts of placed refugees only in the recent, say 1 and 2 years. In that case the number of co-nationals placed in the municipality of assignment of individual $i$ in year $t$ takes the value 0 for $t$ lower than $-2$. The mean of the number of placed co-nationals in year 0 is 23, indicating that on average new refugees were distributed across municipalities in clusters of 23 co-nationals. It turns out that the number of co-nationals placed in the municipality of assignment in year 0 is included in the instrument used in estimations for the overall sample and for every subsample.

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The strength of the instrument can be inferred from Table VI. The results of the F-tests on excluded variables in which the identifying variables, the number of co-nationals placed in the municipality of assignment in year $t$, is excluded in the first stage regression confirm that the identifying variables are strongly correlated with the endogenous explanatory variable we are instrumenting, the logarithmic value of current ethnic group size. Similarly, the partial $R^2$ values which show how much inclusion of the identifying variables improves $R^2$ of the first stage regression model are relatively high for all three samples.

Over-identification test results are also reported in Table VI. The null hypothesis of orthogonality between the instrument and the error term in Equation (2) cannot be rejected at conventional significance levels for any of the (sub-) samples. This indicates that the identifying variables are valid exclusion restrictions.

Turning to the results of the test for weak exogeneity of ethnic group size, they are reported in Table VII. The t-test for weak exogeneity of ethnic group size is rejected for the full sample at a conventional 5% significance level and for the sub-sample of low-educated individuals at a 10% significance level. AGLS estimation of the instrumental variables model given by Equations (3)-(6) should therefore result in consistent estimates for the overall sample and for the sub-sample of low-educated individuals. In contrast, due to lack of significant location sorting by ability of higher-educated refugees, the probit estimate of the effect of the logarithmic value of ethnic group size should be consistent for higher-educated refugees. The negative sign of the 2SCML estimate of the predicted residual from first stage regression indicates that low-educated refugees with relatively unfavourable personal characteristics sort into ethnic enclaves.

The AGLS estimation results of the instrumental variables model are reported in Table V. The results show that controlling for unobserved heterogeneity, the logarithmic value of ethnic group size on average has a positive effect on an individual's employment probability for the overall sample of refugees and for the sub-sample low- and higher-educated refugees. However, the effect is insignificant at a 10% significance level for the sub-sample of low-educated individuals.

The marginal effect of logarithmic value of the ethnic group size calculated at the mean of the distribution of observed characteristics is reported in Table VIII. For the overall sample of refugees an increase of 1 in the logarithmic value of current ethnic group size on average increases the employment probability seven years after immigration by 4 percentage points. At the mean of the distribution, an increase of 1 in the logarithmic value of current ethnic group size corresponds to an increase in ethnic group size of 1,733 co-nationals (from 1,010 co-nationals to 2,742 co-nationals).

Turning to the sub-samples a log increase in current ethnic group size of 1 on average increases the employment probability seven years after immigration by 2.8 percentage

Table V

Model estimates. Dependent variable: Pr(employed).

| Sample | Full | | Low-educated | | Higher-educated | |
|---|---|---|---|---|---|---|
| Estimator: | Probit | AGLS | Probit | AGLS | Probit | AGLS |
| ln(ethnic group size) | -.060*** | .123** | -.056*** | .087 | -.082*** | .172* |
| | (.009) | (.059) | (.010) | (.065) | (.023) | (.091) |
| Woman | -.516*** | -.516*** | -.541*** | -.535*** | -.347** | -.376*** |
| | (.054) | (.055) | (.059) | (.059) | (.152) | (.153) |
| Age | .085*** | .086*** | .100*** | .099*** | .063 | .108** |
| | (.0153) | (.015) | (.017) | (.017) | (.050) | (.050) |
| Age squared | -.002*** | -.002*** | -.002*** | -.002*** | -.001** | -.002*** |
| | (.0002) | (.0002) | (.0002) | (.0002) | (.0006) | (.0006) |
| Married | .093*** | .084** | .078** | .067* | .259*** | .251*** |
| | (.036) | (.036) | (.039) | (.040) | (.100) | (.101) |
| Woman*Married | -0.101 | -0.089 | -0.084 | -.081 | -0.186 | -0.149 |
| | (0.063) | (0.063) | (0.068) | (.069) | (0.171) | (0.173) |
| Young children | -.300*** | -.293*** | -.296*** | -.284*** | -.286*** | -.316*** |
| | (.031) | (.031) | (.034) | (.034) | (.082) | (.083) |
| Older children | -.069** | -.021 | -.110*** | -.061* | .108 | .116 |
| | (.031) | (.033) | (.035) | (.036) | (.080) | (.081) |
| High school degree | .040 | .040 | .044 | .046 | | |
| | (.053) | (.054) | (.054) | (.054) | | |
| Professional degree | .179*** | .178*** | | | | |
| | (.058) | (.058) | | | | |
| ln(ethnic stock) | -.154** | -.268*** | -.220*** | -.314*** | 0.271 | .179 |
| | (.077) | (.086) | (.084) | (.094) | (.565) | (.213) |
| No. of individuals | 13,661 | | 11,577 | | 1,959 | |
| No. of parameters | 241 | | 236 | | 142 | |
| Log likelihood value | -7,314 | | -6,060 | | -1,124 | |
| Pseudo R$^2$ | 0.120 | | 0.133 | | 0.104 | |

Notes: Additional controls are year of immigration, ethnic group and municipality of assignment. Standard errors are reported in parentheses. One, two and three asterisks indicate significance at a 10, 5 and 1% significance level, respectively. Low-educated individuals are defined as individuals with less than 13 years of schooling.

points for low-educated individuals, but decreases the employment probability by 2.9 percentage points for high-educated individuals. In theory there could be gender differences

Table VI

Test of over-identifying restrictions.

| Sample | Full | Low-educated | Higher-educated |
|---|---|---|---|
| Over-identification test statistic | $2.905 \sim \chi^2_4$ | $6.078 \sim \chi^2_3$ | $.972 \sim \chi^2_1$ |
| Strength of identifying variables: | | | |
| F-test on excluded variables | $53^{***} \sim F(5,13648)$ | $54^{***} \sim F(4,11557)$ | $22^{***} \sim F(2,1882)$ |
| Partial $R^2$ | 0.017 | 0.015 | 0.021 |

Notes: Three asterisks indicate significance at a 1% significance level.

in the effect of living in an ethnic enclave on employment of recent immigrants. However, separate estimation of the average effect of ethnic enclave size on the employment probability for men and women indicates absence of gender differences. The probit estimate of the effect of the logarithmic value of ethnic group size on the employment probability is significantly negative for both sub-samples.

However, the estimated effect of the residual from the first stage regression indicates evidence of negative self-selection into ethnic enclaves of both men and women. But the hypothesis of weak exogeneity of the logarithmic value of the ethnic group size cannot be rejected at conventional significance levels. This is likely to be due to lack of degrees of freedom. Controlling for unobserved heterogeneity by instrumenting the logarithmic value of current ethnic group size, the AGLS estimate shows that the logarithmic value of the ethnic group size significantly increases the employment probability of both men and women. The marginal effect calculated at the mean of the distribution shows that an increase of 1 of the logarithmic value of ethnic group size on average increases the employment probability seven years after immigration by 4.7 percentage points for men and 3.4 percentage points for women.

In conclusion, seven years after immigration ethnic group size on average promotes employment of refugees. This result could be driven by a positive effect of current ethnic group size on employment chances for low-educated refugees, because the results do not allow us to draw strong conclusions on how living in an ethnic enclave affects employment chances of higher-educated refugees; the sub-sample of higher-educated refugees is too small. Finally, there is no evidence of gender differences in the effect of living in an ethnic enclave on employment chances.

To interpret the economic significance of the results I calculate the effect of a standard deviation (1,176) increase in ethnic group size relative to its mean of 1,010 co-nationals. Such a relative standard deviation increase corresponds to a 116% increase of ethnic group size and to a log increase of 1.022. For the overall sample, a relative standard deviation increase in ethnic group size seven years after immigration therefore on average increases an individual's employment probability by (1.022*0.04=) 0.04, i.e. 4 percentage points.

Table VII

Test for weak exogeneity of ln(ethnic group size). 2SCML estimates.

| Sample | Full | | Low-educated | | Higher-educated | |
|---|---|---|---|---|---|---|
| ln(ethnic group size) | .069 | (.062) | .084 | (.076) | .145 | (.147) |
| Residuals from first stage | -.131** | (.063) | -.142* | (.077) | -.232 | (.149) |

Notes: Standard errors are reported in parentheses. One and two asterisks indicate significance at a 10 and 5% significance level, respectively.

*IV.B. Earnings Effects of Ethnic Group Size*

This subsection investigates how living in an ethnic enclave affects real annual earnings of recent immigrants by estimating two earnings models for the sub-sample of refugees who have positive earnings seven years after immigration. The first model to be estimated is a model in which ethnic group size seven years after immigration is assumed to be weakly exogenous. In that case ordinary least squares (OLS) estimation of a model in which earnings of individual $i$ seven years after immigration is regressed on the ethnic group size of individual $i$ seven years after immigration as well as observed personal characteristics, year of immigration, ethnic group and municipality of assignment, yields a consistent estimate of the effect of ethnic group size seven years after immigration on earnings of employed individuals.

The second model to be estimated is a model in which ethnic group size seven years after immigration is treated as an endogenous variable. In this model ethnic group size seven years after immigration is instrumented by the subset of the candidates for identifying variables of the effect of ethnic group size that gives the best linear predictor among the instruments for which the test of over-identifying variables cannot be rejected. The model is estimated by Two Stage Least Squares (2SLS) estimation.

The estimates of the marginal effect of the logarithmic value of the ethnic group size on real annual earnings for the sub-sample of refugees in the refugee sample who have positive earnings seven years after immigration are reported in Table IX. OLS estimation yields a negative and significant estimate of the logarithmic value of ethnic group size on earnings for the overall sample of refugees with positive earnings seven years after immigration. However, the test results reported in column 4, Table IX, shows that the null hypothesis of weak exogeneity of the logarithmic value of ethnic group size is rejected. The negative sign of the test score indicates negative self-selection of refugees into ethnic enclaves. The over-identification test statistic is reported in column 3, Table IX. The over-identifying restrictions cannot be rejected at conventional significance levels for the overall sample or for any of the sub-samples.

The instrumental variables estimate of the logarithmic value of ethnic group size on

Table VIII

Marginal effects of ln(ethnic group size) on Pr(employed) seven years after immigration.

| | Probit | | Exogeneity test | | AGLS | |
|---|---|---|---|---|---|---|
| Full sample | -.020*** | (.003) | -.044** | (.021) | .040** | (.019) |
| Low-educated sample | -.018*** | (.003) | -.046* | (.025) | .028 | (.021) |
| Higher-educated sample | -.029*** | (.008) | -.083 | (.053) | .060* | (.032) |

Notes: Standard errors are reported in parentheses. One, two and three asterisks indicate significance at a 10, 5 and 1% significance level, respectively. The reported marginal effects are the marginal effects for an individual in the estimation sample with mean characteristics.

earnings is reported in column 5, Table IX. Taking account of location sorting, the earnings elasticity of ethnic group size is estimated to be 18.9% in the overall sample, i.e. a percentage increase in ethnic group size increases earnings by 18.9%.

Turning to the results for the sub-samples of low-educated and higher-educated, the OLS estimate of the effect of the logarithmic value of ethnic group size on earnings is negative and significant. However, the results of the test for weak exogeneity of the logarithmic value of the ethnic group size show significant evidence of negative self-selection into ethnic enclaves for low-educated individuals. Therefore, 2SLS estimation is required to obtain a consistent estimate of the effect of the logarithmic value of the ethnic group size on earnings for low-educated refugees. In contrast, the OLS estimate of a negative effect of the logarithmic value of the ethnic group size on earnings reported in column 2, Table X, should be consistent for the sub-sample of higher-educated individuals. The 2SLS estimate of the logarithmic value of the ethnic group size is positive but insignificant at a 10% significance level for low-educated refugees. As in the case of the employment analysis, it cannot be ruled out that the estimate of a positive effect of the logarithmic value of the ethnic group size on earnings for the overall sample is driven by a positive effect for low-educated refugees only.

Does living in an ethnic enclave affect earnings of men and women in the same way? This is investigated by separate estimation of the effect of the logarithmic value of ethnic group size on real annual earnings for men and women. OLS estimation yields a negative and significant estimate of the effect for both sub-samples. However, the null hypothesis of weak exogeneity of the logarithmic value of the ethnic group size seven years after immigration is rejected for the sub-sample of men and the sub-sample of low-educated women. This indicates that 2SLS estimation is required to consistently estimate the effect of the logarithmic value of ethnic group size on earnings of men and of low-educated women, while the OLS estimates of the logarithmic value of the ethnic group size should be consistent for the sub-samples of higher-educated men and women.

Table IX

Marginal effects of ln(ethnic group size) on ln(real annual earnings) seven years after immigration.

|  | OLS | Over-identification test | Exogeneity test | 2SLS |
|---|---|---|---|---|
| Full sample | -.066*** | 2.26~$\chi_3^2$ | -.260*** | .189* |
|  | (.014) |  | (.100) | (.103) |
| Low-educated sample | -.052*** | 4.75~$\chi_2^2$ | -.218** | .160 |
|  | (.016) |  | (.102) | (.104) |
| Higher-educated sample | -.134*** | 1.35~$\chi_2^2$ | -.317 | .172 |
|  | (.034) |  | (.218) | (.229) |

Notes: Robust standard errors, allowing for correlation between individuals residing in the same municipality, are reported in parentheses. One, two and three asterisks indicate significance at a 10, 5 and 1% significance level, respectively. The controls are the same as the controls in the estimated employment models, see Table V. Number of observations: 5,647.

Overall, the results suggest that living in an ethnic enclave increases real annual earnings of low-educated individuals, irrespective of gender. However, living in an ethnic enclave may decrease real annual earnings of higher-educated individuals, irrespective of gender.

Finally, in order to determine the economic significance of the effect of the logarithmic value of the ethnic group size on earnings, I calculate the effect of a relative standard deviation increase of ethnic group size seven years after immigration on earnings seven years after immigration. For the overall sample, a relative standard deviation increase in ethnic group size seven years after immigration corresponds to an increase of log earnings by (1.022*0.189=) 0.193 which corresponds to a 21% earnings increase. Similarly, a relative standard deviation increase in ethnic group size seven years after immigration is estimated to increase earnings of the low-educated by 18%. This is similar to the estimate of a relative standard deviation increase in ethnic group size on earnings for the low-educated in Edin et al. (2003). According to their estimate, a relative standard deviation increase in ethnic group size increases earnings of the low-educated by 13%.

*IV.C. Specification Checks*

Do the results hold irrespective of ethnic group? To investigate this I have divided the sample into four subsamples based on region of origin (Middle-East, Asia, Africa and Eastern-Europa or South-America and estimated the employment and earnings models for each subsample. The results show evidence of negative self-selection into ethnic enclaves for refugees from the Middle East, Asia, Eastern Europa and South America, however only significantly so for refugees from Asia. The IV-estimates are significant and positive

for refugees from the Middle East (in the employment model only) and Asians. Lack of rejection of the test of weak exogeneity of the logarithmic value of the ethnic group size in the estimated models for Africans and Eastern Europe or South America could be due to lack of observations.

Could the results be driven by the fact that the instrument is weak for the subsample of refugees who belong to the ethnic groups who at the time of introduction of spatial dispersal policy had a relatively large ethnic stock in Denmark? To investigate this I have first reestimated the employment and earnings models excluding refugees from Poland from the sample. The results of negative self-selection into ethnic enclaves and a positive effect of an increase in ethnic group size on the employment probability and earnings are robust to the exclusion of refugees from Poland. Second, I have reestimated the employment and earnings models excluding refugees from Vietnam. All results, except for significance of the positive effect of an increase in the ethnic group size on earnings, are robust to this exclusion. Thirdly, for the subsamples of refugees from the Middle East and Asia, the results of negative self-selection into ethnic enclaves and a positive effect of an increase in the ethnic group size on the employment probability seven years after immigration are robust to the use of an instrument which is strong also for the subset of refugees with relatively large ethnic stocks in Denmark at the time of implementation of the dispersal policy, namely the logarithmic value of the initial ethnic stock.

Finally, I have investigated whether the effect of an increase in ethnic group size on the employment probability and earnings is in fact non-linear. I have tested this hypothesis by estimating the effect of the ethnic group size and the ethnic group size squared on the employment probability and earnings seven years after immigration, controlling for the same observed characteristics as those reported in Table V. In the employment model the probit estimate of ethnic group size is negative and significant, while the probit estimate of ethnic group size squared is positive but insignificant. The tests of weak exogeneity of each of the two variables cannot be rejected at conventional significance levels. In the earnings model, the OLS estimates of the ethnic group size and the ethnic group size squared are both negative and insignificant. Furthermore, the test for weak exogeneity of each of these variables are not rejected. The instrument contains the subset of the candidates for identifying variables of the effect of ethnic group size that gives the best linear predictor among the instruments for which the test of over-identifying variables cannot be rejected. The candidates for identifying variables are the number of co-nationals placed in the municipality of assignment in year $t$, $t = -7, -6, ..., 0$, and the number of conationals placed in the municipality of assignment in year 0 squared. As a consequence, I reject the hypotheses that an increase in the ethnic group size has a non-linear effect on the employment probability and earnings seven years after immigration.

## Table X

Marginal effects of ln(ethnic group size) on Pr(self-employment) and Pr(wage-employment) seven years after immigration.

| | Pr(self-employment) | | Pr(wage-employment) | | |
| | Probit | Exogeneity test | Probit | Exogeneity test | AGLS |
| Sample | | | | | |
| --- | --- | --- | --- | --- | --- |
| Full | 0.0002 | 0.0008 | -0.0203*** | -0.0467*** | 0.0392** |
| | (0.0014) | (0.0100) | (0.0025) | (0.0184) | (0.0154) |
| Low-educated | -0.0005 | 0.0009 | -0.0179*** | -0.0444** | 0.0378** |
| | (0.0015) | (0.0115) | (0.0028) | (0.0205) | (0.0168) |
| Higher-educated | 0.0042 | -0.0025 | -0.0348*** | -0.0896* | 0.0692** |
| | (0.0047) | (0.0337) | (0.0072) | (0.0480) | (0.0270) |

Notes: Standard errors are reported in parentheses. One, two and three asterisks indicate significance at a 10, 5 and 1% significance level, respectively. The controls are the same as the controls in the estimated employment models, see Table V.

### IV.D. Network Mechanisms?

The results suggest that ethnic networks promote employment of refugees seven years after immigration. But how? Do ethnic networks promote self-employment or wage-employment or both? To answer this question I estimate the effect of the logarithmic value of ethnic group size on the probability of being self-employed seven years after immigration and on the probability of being a wage-earner (with on average at least 9 hours per week during the year). Summary statistics for the dependent variables in these models, i.e. the self-employment rate and wage employment rate seven years after immigration are reported by ethnic group and for the overall sample in Table A.IV in the Appendix. The estimation results are reported in Table X.

According to the probit estimation results for the self-employment model the relationship between ethnic group size and the probability of being self-employed seven years after immigration is insignificant. Furthermore, the test of weak exogeneity of the logarithmic value of ethnic group size is not rejected in the self-employment model. In other words, there is no evidence of self-selection into ethnic enclaves. As a consequence, the probit estimates should be consistent. In contrast, the estimation results for the wage-employment model show significant evidence of negative self-selection into ethnic enclaves. Controlling for such self-selection, the AGLS results show that an increase in ethnic group size seven years after immigration significantly increases the probability of being a wage-earner seven years after immigration. Comparison of the results in Table X with the results in Table V suggests that the positive effect of ethnic group size on the probability of being employed seven years after immigration is due to the result reported in Table X that an increase

in ethnic group size increases the probability of obtaining a job as a wage earner seven years after immigration. In other words, ethnic networks appear to promote employment of refugees seven years after immigration because access to ethnic networks increase the probability of being a wage earner seven years after immigration. Access to ethnic networks may for that matter increase the probability of being of wage earner either by increasing the chances of getting job offers, possibly from co-ethnic firms, or by increasing the job search efforts due to social norms in the ethnic group endorsing self-sufficiency through wage employment.

## V. A LATE Interpretation of the Instrumental Variables Estimates

Conventional applications of the method of instrumental variables assume a constant treatment effect for all unit increases in the treatment intensity and all individuals with the same observable characteristics. In that case, the instrumental variables estimand identifies the average treatment effect in a population of interest and in a subpopulation of the treated. In the current setting, the assumption of a constant unit treatment effect means that the labour market gain of living in a location with a given ethnic group size is constant for all levels of ethnic group size and for all persons with the same $X_1$. This is a strong assumption. But what kind of treatment effect does the instrumental variables estimand identify if we relax this assumption?

In case of heterogeneous treatment effects for individuals with the same $X_1$, the instrumental variables estimand will still identify the average treatment effect and the treatment effect of the treated if one is willing to assume that agents do not select into the program on the basis of the idiosyncratic component of their response to the program (Heckman and Robb 1985; 1986). This assumption implies that refugees do not sort into ethnic enclaves based on the idiosyncratic component of their return to living in a location with a given ethnic group size. This is also a strong assumption.

However, Imbens and Angrist (1994) show that in the case of heterogeneous treatment effect the instrumental variables estimand still identifies a treatment effect under the weak assumption of monotonicity, i.e. if all individuals are shifted by the instrument in a monotone way. Under the monotonicity assumption, the instrumental variables estimand identifies the local average treatment effect (LATE). In the current context of a multi-valued treatment variable, LATE is the average treatment effect for individuals who are induced to change treatment intensity (rather than treatment status) by changing an exogenous regressor that satisfies an exclusion restriction, see Angrist and Imbens (1995).[14] This group of individuals is called compliers, switchers and persons at the

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[14]In the case of binary treatment LATE is $E[Y_1 - Y_0 | D(z) = 1, D(z') = 0]$ where $Y_j$ is a discrete random variable of counterfactual response to treatment intensity $j$ , $Z$ is the instrument and $D$ is an indicator variable for programme participation and $z \neq z'$.

margin of being treated.

Relative to average treatment effects (ATE) and average treatment effects on the treated (ATT), LATE has two drawbacks. First, it measures the effect of treatment on a generally unidentifiable subpopulation, namely the individuals who are shifted by the instrument. The subgroup of compliers is unidentifiable because membership involves unobserved counterfactual treatment intensities. Second, LATE depends on the particular instrumental variable that we have available because the instrument determines the subgroup of compliers. However, the LATE assumptions impose weaker assumptions on the counterfactual data than the classical selection model first proposed by Heckman (1976) in which one imposes parametric functional form distributional assumptions (Vytlacil 2002).

I will now investigate whether one can argue that the instrumental variables estimand identifies a LATE in the current context. Such investigation requires investigation of whether the monotonicity assumption is satisfied and if so, who the compliers are.

*V.A. Monotone Treatment*

To investigate whether the monotonicity assumption is satisfied, let us reformulate our problem as an evaluation problem. Let $E_z$ be a discrete random variable of the counterfactual multi-valued treatment intensity, i.e. ethnic group size seven years after immigration conditional on the instrument $Z$, $e = 1, ..., J$. Furthermore, let $Y_j$ be a discrete random variable of counterfactual (labour market outcome) response to treatment intensity $j$. To simplify the exposition, let there only be one multi-valued, discrete identifying variable in $Z$, e.g. the number of co-nationals placed in the municipality of assignment in the year of immigration, $z = 0, ..., K$.

The monotonicity assumption will be satisfied if with probability 1, either $E_k \geq E_{k-1}$ or $E_k \leq E_{k-1}$ for all $k$ and for each person with the same $X_1$. In the ethnic enclave context, monotonicity means that because of spatial dispersal in a cluster of $k$ co-nationals, either refugees dispersed in a cluster of $k$ co-nationals have at least as large an ethnic group size seven years after immigration as refugees dispersed in a cluster of $k-1$ co-nationals or vice versa. This assumption has the testable implication that the cumulative distribution function (CDF) of $E_k$ and $E_{k-1}$ should not cross, because if, say, $E_k \geq E_{k-1}$ for all $k$ with probability 1, then $\Pr(E_k \geq j) \geq \Pr(E_{k-1} \geq j)$ for all $j$. This implies $\Pr(E_z \geq j | Z = k) \geq \Pr(E_z \geq j | Z = k - 1)$ or $F_{E_z}(j | Z = k) \geq F_{E_z}(j | Z = k - 1)$, where $F_{E_z}$ is the CDF of $E_z$, see e.g. Angrist and Imbens (1995).

For each of the eight potential identifying variable candidates, the number of co-nationals placed in the municipality of assignment in the year $t$, $t = -7, -6, ..., 0$, I investigate empirically whether the candidate satisfies the testable implication of the monotonicity assumption. This is done by first rescaling each identifying variable into a
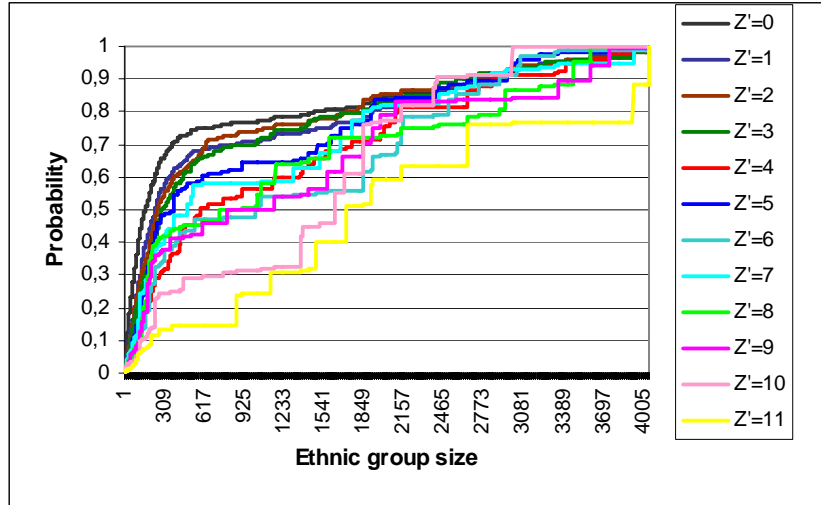
Figure 3: Cumulative distribution function for ethnic group size seven years after immigration. By treatment intensity.

new discrete variable $Z\prime$, $z' = 0, ..., K'$, where $K\prime$ is substantially smaller than $K$.[15]

Figure 3 presents empirical evidence that the monotonicity assumption is approximately satisfied for the number of co-nationals placed in the year of assignment in the year of immigration, even unconditional on $X_1$. In particular, $F_{E_{z'}}(j|Z' = k) \geq F_{E_{z'}}(j|Z' = k-1)$, i.e. the cumulative distributive function of ethnic group size seven years after immigration for individuals with a higher value of the instrument stochastically dominates the cumulative distributive function of ethnic group size seven years after immigration for individuals with a lower value of the instrument. This means that refugees dispersed in a cluster of $k$ co-nationals have at least as large an ethnic group size seven years after immigration as refugees dispersed in a cluster of $k-1$ co-nationals. This result rules out the existence of a subgroup of defiers, i.e. a subset of refugees dispersed in a cluster of $k$ co-nationals have a lower ethnic group size seven years after immigration than refugees dispersed in a cluster of $k-1$ co-nationals.[16] The result is important empirical evidence in support of the claim that in case of heterogenous treatment effects, the instrumental variable estimand identifies LATE.

Figure 4 is a plot of the differences in the CDF for ethnic group size seven years

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[15]For instance, for $Z$ equal to the number of co-nationals placed in the municipality of assignment in the year of immigration, $Z\prime = 0$ for $Z = [0, 4]$, $Z\prime = 1$ for $Z = [5, 9]$, $Z\prime = 2$ for $Z = [10, 14]$, $Z' = 3$ for $Z = [15, 19]$, $Z\prime = 4$ for $Z = [20, 24]$, $Z\prime = 5$ for $Z = [25, 29]$, $Z\prime = 6$ for $Z = [30, 34]$, $Z\prime = 7$ for $Z = [35, 39]$, $Z\prime = 8$ for $Z = [40, 44]$, $Z\prime = 9$ for $Z = [45, 54]$, $Z\prime = 10$ for $Z = [55, 69]$, $Z\prime = 11$ for $Z = [70, 104]$.

[16]Empirical evidence that the monotonicity assumption is approximately satisfied for the seven remaining identifying variable candidates is available from the author upon request.
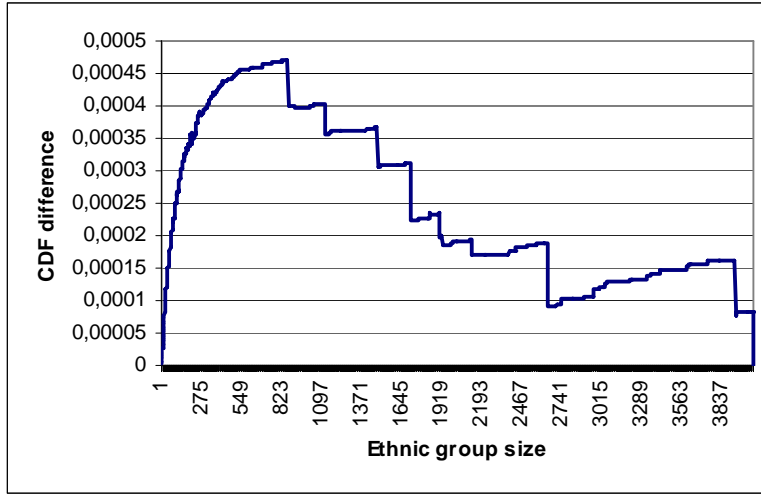
Figure 4: Difference in ethnic group size CDF for Z'=0 and Z'=11.

after immigration for $Z$ equal to the number of co-nationals placed in the municipality of assignment in the year of immigration and for $Z' = 0$ and $Z' = 11$, normalized to 1. The figure illustrates which treatment intensity groups contribute most to the LATE. The function declines at an ethnic group size seven years after immigration of 868 co-nationals, implying that for $Z\prime = 0$ relative to $Z\prime = 11$ most of the contribution to LATE comes from groups at the lower end of the distribution of ethnic group size seven years after immigration. This result is in accordance with the economic model for programme participation that I present shortly. According to the model, the dispersal policy affects mainly refugees who do not want to live in a large ethnic enclave at any prize, i.e. irrespective of the expected migration costs relative to the expected net benefits of relocation to a larger ethnic enclave. Further evidence, not presented here, shows that for all adjacent pairs of $Z = k$ and $Z = k-1$, most of the contribution to LATE comes from groups with low and intermediate treatment intensities.

### V.B. Compliers

Recall that LATE is the mean impact on compliers. Who are the compliers in the current setting? The evidence presented in Section III.F indicates that it is the subgroup of placed refugees who take treatment at a lower level than they would have done in the absence of the policy and if they had received a higher value of the instrument. How did the dispersal policy induce any placed refugees to change their treatment intensity?

A placed refugee faces a problem of finding an optimal location in the host country, i.e. he has to decide whether or not to move away from the municipality of assignment which in the evaluation framework corresponds to the decision of whether or not to participate in the dispersal programme and take treatment at the assigned treatment intensity. I argue

that the non-participation decision of a placed refugee can be modeled by the migration model by Nakosteen and Zimmer (1980) according to which the potential migrant (non-participant) weighs the net expected pecuniary and mental benefits of migration against the expected pecuniary and mental costs of migration. Migration (non-participation) will occur, if the former exceeds the latter. In other words, the potential programme participant (a placed refugee) makes his participation decision by weighing the expected costs of non-participation against the net expected benefits of non-participation. The candidates for identifying variables, the number of placed co-nationals in the municipality of assignment in year $t$, $t = -7, -6, ..., 0$, are potentially important determinants of non-participation. The reason is that placed refugees are likely to derive high utility from living in the same location as co-nationals according to the ethnic network hypothesis by Piore (1979) and Kobrin and Speare (1983) and the ethnic goods hypothesis by Chiswick and Miller (2005). These two hypotheses imply that the expected utility gain decreases and the expected costs of non-compliance increase with the value of the candidates for identifying variables. Therefore, the value of the candidates for identifying variables unambigously decreases the migration probability. To test this hypothesis I regress the programme participation status of individual $i$ seven years after immigration on the logarithmic value of the candidates for identifying variables, controlling for personal attributes and other regional attributes of the municipality of assignment. The vector of personal attributes contains two sets of controls, first controls for the personal attributes which may have affected the initial location: marital status, children indicators, age and size of the ethnic stock at the time of immigration as well as year of immigration and country of origin and second, additional personal attributes which may affect the programme participation decision: gender and years of education. The vector of regional attributes of the municipality of assignment contain initial values of: 1) demographic attributes, 2) labour market attributes and 3) housing market attributes of the municipality of assignment of potential importance for the programme participation decision.

The estimation results are shown in Table XI. They show that ceteris paribus refugees were more likely to have moved away from the municipality of assignment seven years after immigration, the smaller the logarithmic value of three of the candidates for identifying variables, specifically the smaller the value of ln(number of co-nationals placed in the municipality of assignment in year $t$), $t = -4, -1, 0$. The logarithmic value of the remaining candidates for identifying variables, ln(number of co-nationals placed in the municipality of assignment in year $t$), $t = -7, -6, -5, -2$, had an insignificant effect on the probability of migration in a specification in which all potential identifying variables were included. However, estimation results not reported here, show that ceteris paribus, the logarithmic value of every candidate for identifying variable has a significant and negative effect on the probability of migration, in specifications in which each candidate

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Table XI

Probit estimation of the Pr(having moved out of the municipality of assignment seven years after immigration). Marginal effects.

| Variables | Estimate | Std. error |
|---|---|---|
| Personal attributes: | | |
| Woman | -.037*** | (.011) |
| Age | -.006*** | (.001) |
| Married | -.099*** | (.015) |
| Young children | -.006 | (.013) |
| Older children | -.049*** | (.014) |
| ln(ethnic stock) | -.050* | (-.050) |
| High school level | -.004 | (.021) |
| University level | .004 | (.023) |
| Highest degree missing | .008 | (.020) |
| Regional attributes of the municipality of assignment: | | |
| Medium | .250*** | (.024) |
| Small | .340*** | (.025) |
| ln(number of immigrants) | -.015 | (.010) |
| ln(number of placed co-nationals in year 0) | -.043*** | (.005) |
| ln(number of placed co-nationals in year -1) | -.001** | (.001) |
| ln(number of placed co-nationals in year -4) | -.002*** | (.001) |
| Local unemployment rate | .010*** | (.003) |
| % right-wing votes | .001 | (.001) |
| Number of educ. institutions | .004*** | (.001) |
| % county jobs | -.005* | (.003) |
| % social housing | -.008*** | (.001) |
| Log likelihood | -8,248.11 | |

Notes: One, two and three asterisks respectively indicate significance at a 10, 5 and 1% significance level. Additional controls are year of immigration and country of origin. Excluded categories are: education level: basic (less than 10 years), municipality size category: large. Number of observations: 13,927. Number of movers: 7,266.

for identifying variable is included separately. This supports the view that individual $i$'s decision on whether or not to accept the assignment to a given level of treatment was affected by the instrument, $Z_i$=[ln(number of co-nationals placed in the municipality of assignment in year $t$)], $t = -7, -6, ..., 0$, through an economic model.

Further empirical evidence in support of this view is the result that the cumulative distribution function for initial ethnic group size of movers is seen to dominate that of

stayers, indicating that programme participants are overrepresented among refugees who were assigned to treatment at a relatively high level.

The migration results suggest that movers are attracted to municipalities in which a larger share of their co-nationals lives. That this is indeed the case is supported by the evidence that the cumulative distribution function for ethnic group size seven years after immigration of stayers dominates that of movers at intermediate ethnic group size, i.e. seven years after immigration non-participants (movers) are overrepresented among refugees who live in a municipality with a relatively large ethnic group size seven years after immigration. Further descriptive evidence reveals that after relocation movers on average live in municipalities with a significantly larger local population in which immigrants constitute a significantly larger share of the local population and with significantly larger number of co-nationals than stayers.

The result that stayers are overrepresented among refugees who were assigned to a municipality with a relatively large number of co-nationals initially and underrepresented among refugees with a relatively large number of co-nationals in the municipality of residence seven years after immigration is intuitive: For these individuals the number of co-nationals in the municipality of assignment was so close to their preferred (large) number of co-nationals that the costs of relocation to a municipality with an even larger number of co-nationals exceeded the benefits from relocation to such a municipality.

I conclude that in the case of heterogeneous treatment effects, the instrumental variables estimand identifies the average effect of ethnic enclave size on the labour market outcome gain of the subgroup of refugees subject to the spatial dispersal policy who are induced to decrease their ethnic group size seven years after immigration because opting out of the dispersal programme after initial assignment to a municipality is costly due to migration costs.[17]

## VI. Concluding remarks

The aim of this paper was to estimate the average causal effect of ethnic enclave size on labour market outcomes of immigrants, in particular the employment probability and real annual earnings. Ethnic enclave size is measured as the number of co-nationals living in the municipality of residence. Potential location by ability of individuals is taken into account by exploiting the Danish spatial dispersal policy 1986-1998 under which refugees were randomly assigned to locations conditional on six characteristics of the individual.

The findings fall into four categories. First, I find significant evidence of negative self-selection into ethnic enclaves of the overall sample of refugees who were subject to

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[17]Previous studies in which LATE is identified include evaluations based on natural experiments such as Angrist (1990) and Angrist and Krueger (1991).

the Danish dispersal policy 1986-1998. Second, taking account of location sorting, the ethnic enclave size is estimated to affect economic success of refugees positively. A relative standard deviation increase in ethnic enclave size on average increases the employment probability of refugees by 4 percentage points and earnings by 21 percent. Third, I present evidence that an increase in the ethnic enclave size increases the employment probability of refugees by increasing the probability of wage-employment rather than self-employment. Finally, I argue that the IV-estimates identify the average effect of ethnic enclave size on the labour market outcome gain of the subgroup of refugees subject to the spatial dispersal policy who are induced to decrease their future ethnic enclave size because opting out of the dispersal programme after initial assignment to a municipality is costly due to migration costs.

Separate estimation of the effect of ethnic enclave size for low- and higher-educated individuals indicates that the result of negative self-selection into ethnic enclaves is driven by negative self-selection into ethnic enclaves of low-educated individuals. Similarly, the estimate of a positive and significant effect of ethnic enclave size on labour market outcomes appear to be driven by a positive effect of ethnic enclave size for low-educated individuals only. The effect of ethnic enclave size on economic success of higher-educated appears to be negative and significant. However, one may argue that a larger sample of higher-educated refugees is necessary for drawing conclusions on the effect of living in an ethnic enclave on the economic success of high-educated refugees.

Separate estimation of the effect of ethnic enclave size for male and female refugees separately indicates that living in an ethnic enclave affects men and women in the same way.

The finding of a positive and significant average causal effect of ethnic enclave size on individual labour market outcomes may be interpreted as additional empirical evidence in support of the importance of social networks for an individual's labour market outcomes. The data at hand does not allow for disentanglement of peer group effects, e.g. work attitudes, and social network effects, e.g. social information spillover about job vacancies and earnings opportunities, within the ethnic group. However, a likely interpretation is that presence of co-nationals locally constitutes a network that conveys information about wage-employment and earnings opportunities in the host country and that ethnic enclaves constitute an environment in which the immigrant is less exposed to discrimination encountered elsewhere in the labour market. In contrast, ethnic enclaves do not appear to be associated with social norms that hamper self-sufficiency or employment of female refugees such as traditional gender roles. In addition, the finding is empirical evidence against the widespread belief that residential concentration of ethnic minorities slows down host-country-specific human capital, e.g. host-country language proficiency, and thereby damages labour market integration of immigrants. If residence in ethnic

enclaves does in fact slow down such acquisition, the findings of this analysis show that there is more to the story of ethnic enclaves than that, most likely social network effects, which dominate the effect of slow acquisition of host-country-specific human capital, at least for low-educated refugees.

Edin et al. (2003) report earnings findings which are similar to those reported in this study. Taking account of location sorting, they estimate that a relative standard deviation increase in ethnic enclave size increases earnings of the low-educated refugees eight years after immigration by 13% while the effect is insignificant for the overall sample and high-educated individuals. They emphasize that their findings are consistent with a story that ethnic enclaves are associated with ethnic enclave effects that primarily benefit the least skilled. The findings of this study do not contradict that story.

The study was primarily concerned with the issue of whether the size of the ethnic enclave matters to employment outcomes of immigrants. However, for policy recommendation purposes further research is needed into *why* ethnic enclave size matters to immigrants' labour market outcomes, i.e. research into the *exact* transmission mechanisms of ethnic enclave size effects. Important insights could arise, e.g. from empirical disentanglement of peer group effects from social information spillover.

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APPENDIX

Table A.I
Variable definitions and primary sources of data. Part A.

| Variable | Definition | Primary source of data |
|---|---|---|
| *Individual characteristics:* | | |
| Woman | Dummy for sex. | Population register, Statistics Denmark (DST). |
| Age | Age. | Population register, DST. |
| Married | Dummy for being married. | Population register, DST. |
| Young children | Dummy for presence of children between 0 and 2 years of age in the household. | Population register, DST. |
| Older children | Dummy for presence of children between 3 and 17 years of age in the household. | Population register, DST. |
| Country of origin | Dummy for immigrant source country. | Population register, DST. |
| Year of immigration | Dummy for first year of receipt of residence permit. | Population register, DST. |
| No degree | Dummy for 0-9 years of education constructed from an education code of highest degree attained. | Surveybased register on immigrants' education level attained prior to immigration and integrated pupil register, DST. |
| High school degree | Dummy for 10-12 years of education constructed from an education code of highest degree attained. | Surveybased register on immigrants' education level attained prior to immigration and integrated pupil register, DST. |
| Professional degree | Dummy for 13 or more years of education constructed from an education code of highest degree attained. | Surveybased register on immigrants' education level attained prior to immigration and integrated pupil register, DST. |
| Unknown education | Dummy for lack of information on highest degree attained. | Surveybased register on immigrants' education level attained prior to immigration and integrated pupil register, DST. |
| Ethnic stock | Number of immigrants and descendants from individual $i$'s source country $k$ in Denmark. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |
| *Municipality characteristics:* | | |
| Number of inhabitants | Number of inhabitants in municipality $j$. | Population statistics (population counted data), DST. |
| Large | Municipality with at least 100,000 inhabitants. | Population statistics (population counted data), DST. |
| Medium | Municipality with 10,000-99,999 inhabitants. | Population statistics (population counted data), DST. |
| Small | Municipality with less than 10,000 inhabitants. | Population statistics (population counted data), DST. |

Table A.I
Variable definitions and primary sources of data. Part B.

| Variable | Definition | Primary source of data |
|---|---|---|
| *Municipality characteristics:* | | |
| % immigrants | Number of immigrants and descendants living in municipality $j$ in per cent of the total number of immigrants and descendants in Denmark. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |
| % co-nationals | Number of immigrants and descendants from individual i's source country $k$ living in municipality $j$ in per cent of the total number of immigrants and descendants from source country $k$ in Denmark. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |
| Number of immigrants | Number of immigrants and descendants of immigrants residing in municipality $j$. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |
| Ethnic group size | Number of immigrants and descendants of immigrants from source country $k$ residing in municipality $j$. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |
| Local unemployment rate | The unemployment rate in a radius of DKK 60 (approx. USD 10) of transport around the largest post office in municipality $j$. | Unemployment register (population counted data), DST, and cost of transport statistics, the Ministry of Transport. Constructed by Local Government Studies. |
| % right-wing votes | Sum of votes for the Liberal Party and the Conservative People's Party in per cent of the sum of votes for the Liberal Party, the Conservative People's Party, the Social Democratic Party and the Socialist People's Party at the latest municipal election. The two former parties are traditional right-wing parties whereas the latter two are traditional left-wing parties. | Election statistics, DST. |
| Number of educ. institutions | Number of institutions for vocational and higher education in municipality $j$. | Integrated pupil register (population counted data), DST. |
| % county jobs | Number of individuals employed in municipality $j$ in per cent of the total number of individuals employed in the county. | Registerbased labour force statistics (population counted data), DST. |
| % social housing | Number of social housing dwellings for all-year residence in per cent of the total number of dwellings for all-year residence in municipality $j$. | Buildings and housing statistics (population counted data), DST. |
| Number of placed co-nationals in year $t$ | Number of immigrants from individual $i$'s source country placed by the authorities in individual $i$'s municipality of assignment in year $t$, $t$=-7, -6, …, 0, where 0 refers to the year of immigration of individual $i$. | Population register, DST. Author's calculations based on the 100 per cent sample of immigrants. |

Table A.II

Summary statistics (initial values). Means (std. dev.). Part A.

| Variables | Mean | Std. dev. |
|---|---|---|
| Woman | .34 | .47 |
| Age | 27.86 | 7.60 |
| Married | .44 | .50 |
| Young children | .24 | .43 |
| Older children | .34 | .47 |
| *Country of origin:* | | |
| Poland | .03 | .17 |
| Iraq | .16 | .37 |
| Iran | .20 | .40 |
| Vietnam | .10 | .29 |
| Srilanka | .13 | .33 |
| Lebanon | .28 | .45 |
| Ethiopia | .01 | .10 |
| Afghanistan | .02 | .14 |
| Somalia | .06 | .24 |
| Rumania | .02 | .12 |
| Chile | .002 | .04 |
| Immigration year 1986 | .26 | .44 |
| Immigration year 1987 | .13 | .34 |
| Immigration year 1988 | .10 | .30 |
| Immigration year 1989 | .12 | .32 |
| Immigration year 1990 | .09 | .28 |
| Immigration year 1991 | .10 | .30 |
| Immigration year 1992 | .11 | .32 |
| Immigration year 1993 | .09 | .28 |
| ln(ethnic stock) | 3,710.78 | 1964.92 |

Table A.II

Summary statistics (initial values). Means (std. dev.). Part B.

| Variables | Mean | Std. dev. |
|---|---|---|
| Basic schooling | .06 | .23 |
| High school degree | .25 | .43 |
| Professional degree | .13 | .34 |
| Missing information on educational attainment | .56 | .50 |
| *Municipality of residence:* | | |
| Number of inhabitants | 111,669 | 136,851 |
| % immigrants | 3.61 | 6.45 |
| % co-nationals | 5.16 | 6.81 |
| Large | .32 | .47 |
| Medium | .59 | .49 |
| Small | .09 | .29 |
| Number of immigrants | 8,759 | 15,784 |
| Ethnic group size | 265 | 399 |
| Unemployment rate | 10.21 | 2.40 |
| % right-wing votes | 40.21 | 12.16 |
| Number of educ. institutions | 9.14 | 10.03 |
| % county jobs | 26.00 | 25.56 |
| % social housing | 20.35 | 9.96 |
| Number of placed co-nationals in year t | 23.0 | 22.1 |
| Number of placed co-nationals in year t-1 | 13.8 | 19.4 |
| Number of placed co-nationals in year t-2 | 9.7 | 16.7 |
| Number of placed co-nationals in year t-3 | 7.6 | 15.5 |
| Number of placed co-nationals in year t-4 | 5.5 | 13.7 |
| Number of placed co-nationals in year t-5 | 4.2 | 12.8 |
| Number of placed co-nationals in year t-6 | 3.0 | 11.0 |
| Number of placed co-nationals in year t-7 | 1.4 | 6.9 |
| Number of observations | 13,927 | |

Table A.III

Ethnic stock by ethnic group and calendar year.

| Year | 1984 | 1985 | 1986 | 1987 | 1988 | 1989 | 1990 | 1991 | 1992 | 1993 |
|---|---|---|---|---|---|---|---|---|---|---|
| Ethnic group | | | | | | | | | | |
| Poland | 6,953 | 7,748 | 8,244 | 8,847 | 9,451 | 9,926 | 10,400 | 10,819 | 11,111 | 11,392 |
| Iraq | 274 | 792 | 1,170 | 1,439 | 1,914 | 2,474 | 2,929 | 3,477 | 4,634 | 5,677 |
| Iran | 998 | 4,926 | 6,175 | 7,155 | 8,187 | 8,856 | 9,515 | 10,342 | 10,780 | 11,172 |
| Vietnam | 3,797 | 4,017 | 4,331 | 4,625 | 5,144 | 5,867 | 6,642 | 7,425 | 8,316 | 8,838 |
| Sri Lanka | 356 | 776 | 3,102 | 4,308 | 4,601 | 5,129 | 5,417 | 5,789 | 6,253 | 6,735 |
| Lebanon | 391 | 879 | 3,321 | 5,302 | 6,608 | 8,570 | 10,256 | 12,128 | 13,881 | 15,002 |
| Ethiopia | 242 | 330 | 438 | 530 | 557 | 625 | 668 | 710 | 749 | 806 |
| Afghanistan | 101 | 150 | 212 | 245 | 288 | 345 | 440 | 623 | 783 | 969 |
| Somalia | 176 | 189 | 213 | 249 | 329 | 539 | 765 | 1,411 | 2,276 | 3,858 |
| Rumania | 294 | 346 | 383 | 434 | 472 | 798 | 1,042 | 1,209 | 1,328 | 1,427 |
| Chile | 1,136 | 1,131 | 1,129 | 1,159 | 1,151 | 1,171 | 1,203 | 1,215 | 1,221 | 1,218 |

Notes: Refugees from Lebanon are Palestinian refugees with no citizenship. Data source: Longitudinal administrative registers from Statistics Denmark on the immigrant population in Denmark 1984-2000.

Table A.IV

Summary statistics for the dependent variables by ethnic group.

| | Self-employment rate | | Wage-employment rate | |
|---|---|---|---|---|
| Ethnic group: | Men | Women | Men | Women |
| Poland | 0.07 (0.26) | 0.04 (0.20) | 0.45 (0.50) | 0.37 (0.49) |
| Iraq | 0.13 (0.34) | 0.02 (0.13) | 0.19 (0.39) | 0.11 (0.31) |
| Iran | 0.13 (0.34) | 0.04 (0.19) | 0.21 (0.41) | 0.15 (0.36) |
| Vietnam | 0.05 (0.21) | 0.06 (0.25) | 0.43 (0.50) | 0.18 (0.39) |
| Sri Lanka | 0.02 (0.14) | 0.02 (0.14) | 0.52 (0.50) | 0.32 (0.47) |
| Lebanon | 0.10 (0.30) | 0.02 (0.12) | 0.14 (0.34) | 0.05 (0.21) |
| Ethiopia | 0.01 (0.10) | 0 (0) | 0.24 (0.43) | 0.26 (0.44) |
| Afghanistan | 0.17 (0.37) | 0.06 (0.23) | 0.20 (0.40) | 0.10 (0.31) |
| Somalia | 0.02 (0.14) | 0.01 (0.11) | 0.27 (0.45) | 0.08 (0.27) |
| Rumania | 0.10 (0.30) | 0.07 (0.26) | 0.46 (0.50) | 0.43 (0.50) |
| Chile | 0 (0) | 0 (0) | 0.36 (0.50) | 0.50 (0.53) |
| All | 0.09 (0.29) | 0.03 (0.17) | 0.26 (0.44) | 0.15 (0.36) |

Notes: Standard deviations are reported in parentheses.

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