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Ethnic Enclaves and Welfare Cultures: Quasi-Experimental Evidence

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ABSTRACT

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Quasi-Experimental Evidence[*]

We examine peer effects in welfare use among immigrants to Sweden by exploiting a governmental refugee placement policy. We distinguish between the quantity of contacts – the number of individuals of the same ethnicity – and the quality of contacts – welfare use among members of the ethnic group. OLS regressions suggest that both these factors are positively related to individual welfare use. Instrumental variables estimations yield the conclusion that only the quality of contacts matter. An increase of the fraction of the ethnic group on welfare by 10 percent raises the individual probability of welfare use by almost 7 percent.

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1   Introduction

In all industrialized countries there is concentration of the poor, particularly immigrants, in metropolitan areas. Universally, there is also great concern that this kind of segregation may be detrimental for individual outcomes. In the US, this concern has led to the design of policies such as "Moving to opportunity" in Boston and the Gautreaux program in Chicago.[1] Practically all developed countries also implement various forms of "neighborhood development programs".

The potentially detrimental effects of segregation have also been a concern in the Swedish policy discussion. High and rising welfare use among immigrants and other disadvantaged groups rose to the fore of policy agenda during the economic downturn of the 1990s.[2] Some observers argued that part of the increase in welfare participation can be attributed to the fact that the "stigma" of welfare use decreases along with the overall increase in the number of individuals on welfare; see Lindbeck (1997) for instance.

A feed-back from overall welfare use to individual welfare participation is an example of a social network effect. The effect of social networks on individual behavior has been a topic among sociologists for long. Economists have only recently become interested in this question, however. There are substantial difficulties in establishing that a particular empirical regularity reflects a causal mechanism running from the characteristics of the social network to individual behavior. For instance, it is a stylized fact that the participation of immigrants in various public support systems is greater in immigrant dense areas; however, it is clearly premature to infer that the higher participation rate is caused by living in these areas. Such inference suffers from what Manski (1993) calls the "reflection problem". The essence of this

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[1] See Katz et al. (2001) for an analysis of the Moving to Opportunity and Rosenbaum (1995) for an analysis of the Gautreaux program.
[2] Most of the time we use the US term "welfare" instead of using "social assistance", which is the more correct, although more cumbersome, terminology.

problem is that omitted individual and neighborhood characteristics may imply that individual behavior and the behavior of the neighborhood as a whole are reflections of one another. The reflection problem leads to a correlation between individual and group behavior, which does not have a causal interpretation.

The purpose of this paper is to examine whether immigrant welfare use is causally affected by ethnic concentration and welfare use among their ethnic peers. To estimate these effects we utilize an immigrant policy initiative in Sweden. During the late 1980s and early 1990s, Swedish immigration authorities distributed refugee immigrants across neighborhoods in a way that we argue was independent of unobserved individual characteristics. We have used this policy experiment in earlier work dealing with the earnings effect of living in ethnic enclaves; see Edin et al. (2003).

We also build on the recent work by Bertrand et al. (2000). They utilize the fact that ethnic concentration varies within neighborhood. Under the assumption that immigrants interact mainly with their ethnic peers, they can thus eliminate the influence of unobserved neighborhood characteristics common across individuals. Since the main effects of ethnic concentration (quantity) and ethnic welfare use (quality) may be biased because of sorting on unobserved ability, the authors focus on the quantity-quality interaction. This strategy makes a lot of sense, but may still have some limitations. We think the major drawback is that Bertrand et al. are unable to obtain unbiased estimates on all effects of interest.[3]

The Swedish government policy that we use concerned the initial location of refugee immigrants. This policy was viable between 1985 and 1991. Government authorities placed

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[3] Since Bertrand et al. cannot obtain unbiased estimates on the main effects of quantity and quality, their estimates may be missing the most important part of the social network effect. Moreover, it is not entirely clear that the coefficient on the quantity-quality interaction is unbiased. There are two problems in this respect. First, differential selection across ethnicities may introduce a bias in the coefficient on the interaction term; second, even if differential selection across ethnicities is not an issue, the coefficient on the interaction is only unbiased subject to the functional form assumption that quantity enters the outcome equation linearly. A final potential problem with their strategy is that it is not clear that the coefficient has the interpretation that the authors ascribe to it. In the strict sense, the quantity-quality interaction is just a parsimonious way of allowing the coefficient on quantity to vary across ethnicities

refugees in localities that were deemed suitable according to certain criteria. In practice, the availability of housing was the all-important factor. Our maintained hypothesis is that, because of the policy, the initial location of immigrants is independent of unobserved individual characteristics. Hence, this "quasi-experiment" enables us to reexamine the question of the importance of ethnic networks in welfare participation. Because of the nature of the data we can credibly estimate the main effects of ethnic ties per se (quantity) and welfare use among the ethnic peers (quality). Throughout we hold the observed and unobserved (time-invariant) characteristics of neighborhoods and ethnicities constant.
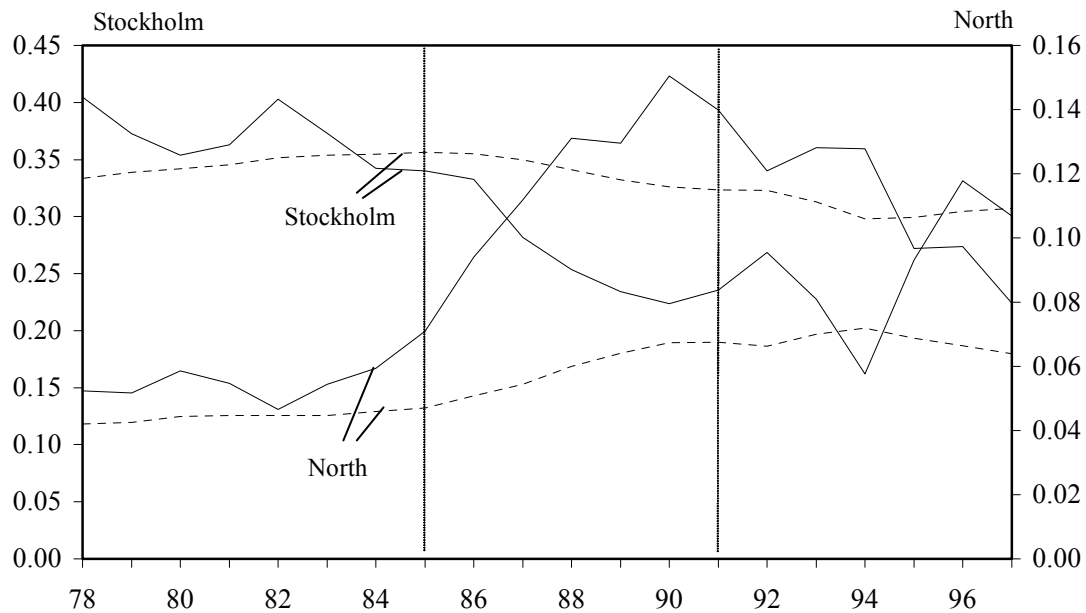
Figure 1: Share of non-OECD immigrant inflow (solid) and stock (dashed) located in Stockholm and in the north of Sweden, 1978–1997.
Notes: "Stockholm" refers to the county of Stockholm and "North" to the six northernmost counties of Sweden. Authors' calculations using the LINDA immigrant sample.

The government settlement policy had real consequences for immigrant location. This is illustrated in Figure 1, where we plot the share of the immigrant inflow and the immigrant stock that resides in Stockholm and the north of Sweden respectively. Prior to 1985, refugees

were allowed to settle in a neighborhood of their own liking. In 1985, the immigrant shares in Stockholm and the north of Sweden stood at 36 and 5 percent respectively. By 1991, the share living in Stockholm had been reduced by more than 3 percentage points, while the share residing in the north increased by 2 points. Thus, the policy initiative clearly increased the dispersion of immigrants across Sweden.

Our results can be summarized as follows. To gauge the importance of sorting bias we begin by running simple OLS regressions. Quantity as well as quality of the network is positively related to individual welfare participation. The IV approach, using characteristics of the initial location as instruments for the characteristics of the neighborhood 9-10 years later, reduces the coefficient on the size of the enclave to zero while the effect of the quality of the network more than doubles in size. In contrast to Bertrand et al. (2000), we do not find that the interaction between quantity and quality is important. The estimate on our measure of the quality of contacts implies that the individual probability of welfare participation rises by almost 7 percent in response to an increase of the welfare use in the ethnic group by 10 percent. The result that what matters is enclave characteristics rather than the size is consistent with Edin et al. (2003). Edin et al. found that there is a positive return to living in an enclave for low-skilled refugee immigrants and that there is no effect in the refugee population as a whole. Moreover, they concluded that the earnings gain of living in an enclave increases with the general labor market position of one's own ethnic group.

We also run separate regressions for households defined by size, presence of kids, and earnings potential. The relative effect of the quality of contacts is more or less invariant to different definitions of the estimating sample. Since the characteristics that we consider are intimately related to welfare eligibility, the results imply that the welfare culture has similar effects across households with different predicted eligibility status.

The remainder of the paper is outlined as follows. By way of background, section 2 gives a description of the institutional setting, discusses whether we can treat the policy shift in 1985 as a quasi-experiment, and describes the basic features of the Swedish welfare (social assistance) system. Section 3 describes the data. In section 4, we report estimates of the effects of networks on welfare use. We also investigate whether there is evidence on coefficient heterogeneity. Section 5 concludes.

2   Background

Relative to the size of the country Sweden has a substantial immigrant population. The share of the foreign born in the total population stood at 11.5 percent in 2001. As in all developed countries, there is concentration in the residential pattern of immigrants – the probability of residing in an "ethnic neighborhood" is high.[4] Further, immigrants from developing countries are more likely to live in an ethnic neighborhood. Over the past thirty years the majority of immigrants have been refugees or tied movers.

The second half of the 1980s and first half of the 1990s saw a massive increase in immigration. Concomitantly, there was a rapid increase in the immigrant share of the social assistance caseload. During the 1990s, the immigrant share of the caseload has hovered around a third and the immigrant share of the total budget around 50 percent. This contrasts markedly with the situation around 1970 when immigrants were as likely as the native born to receive welfare (Lundh et al, 2002). In 2000, the incidence of welfare stood at 4 percent in the native born population, 6 percent in the non-refugee immigrant population, and 32 percent in the refugee immigrant population. Thus, high welfare use among immigrants is largely tied to

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[4] If we define an ethnic neighborhood as a neighborhood where the share of the ethnic group residing in the neighborhood is at least twice as large as the share of the ethnic group in the population we find that 42 percent of the average first generation immigrant lived in an ethnic neighborhood in 1997.

the prevalence of welfare receipt in the refugee immigrant population. Different motives for immigration and a shift in the integration policy (described below) presumably contribute to this fact.

Refugee immigrants constitute the group of interest in our analysis, and we now present the Swedish refugee policy in more detail. Unfortunately, there is very little documentation about the practical implementation of the placement policy. Therefore, part of the information is based on interviews with placement officers and other officials of the Immigration Board. Following the presentation of the placement policy, we give some additional details on the Swedish social assistance system.

2.1   The Swedish refugee placement policy

The placement policy was introduced in 1985.[5] The Immigration Board was then given the responsibility of assigning refugee immigrants to an initial municipality of residence. The Board was to place all political immigrants, but not those who arrived for family reunification reasons.

The introduction of the placement policy was a reaction to immigrant concentration in large cities. The idea was to distribute asylum seekers over a larger number of municipalities that had suitable characteristics for reception, such as educational and labor market opportunities. At first, the intention was to sign contracts with about 60 municipalities, but due to the increasing number of asylum seekers in the late 1980s, a larger number became involved; in 1989, 277 out of Sweden's (then) 284 municipalities participated. The criteria that initially were supposed to govern placement were abandoned. Instead, the availability of housing became the all-important factor. Employment opportunities were scant in many of the municipalities where refugees were placed. To rationalize placement in areas with poor

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[5] This section draws on The Committee on Immigration Policy (1996) and The Immigration Board (1997).

employment prospects the integration process was divided into two periods: an introductory period of 18 months when the immigrant participated in Swedish language courses and lived off welfare; then integration into the labor market commenced.[6]

Formally, the policy of assigning refugees to municipalities was in place from 1985 to 1994. The strictest application of the assignment policy was between 1987 and 1991. For our purposes, this is the most attractive time period, since there were few degrees of freedom for the individual immigrant to choose the initial place of residence. During 1987–91, the placement rate, i.e., the fraction of refugee immigrants assigned an initial municipality of residence by the Immigration Board, was close to 90 percent.

In an earlier paper (see Edin et al., 2003) we argued that the settlement policy provides an exogenous source of variation that identifies the causal effect of neighborhood characteristics. The essence of the argument is that the placement rate was high (in particular during 1987–91), the housing market was booming (making it difficult to find vacant housing in attractive areas), and there was no interaction between local officers and the refugee in question.

To substantiate this argument further, let us briefly describe the handling of a typical asylum seeker from the border to the final placement. An asylum seeker was placed in a refugee center pending a decision from the immigration authorities. Refugee centers were distributed all over Sweden and there was no correlation between the port of entry and the location of the center. However, immigrants were sorted by native language when placed in centers.

When it came to the municipal placement, weight was given to immigrant preferences. Most immigrants, of course, applied for residence in the traditional immigrant cities of

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[6] In Edin et al. (2004) we evaluate the consequences for the refugees of the policy shift occurring in 1985. The policy shift had two components: (i) dispersal of refugees across the country; and (ii) increased reliance on income support. We show that the overall effect of the policy shift was negative for the refugees subjected to the policy and that the increased focus on income support contributed mostly to this negative effect.

Stockholm, Gothenburg and Malmö. However, there were very few housing vacancies in these locations, in particular during the second half of the 1980s when the housing market was booming. When the number of applicants exceeded the number of available slots, municipal officers may have selected the "best" immigrants. There was no interaction between municipal officers and refugees, so the selection was purely in terms of observed characteristics; language, formal qualifications, and family size seem to have been the governing criteria. Preferences were given to highly educated individuals and individuals that spoke the same language as some members of the resident immigrant stock. Single individuals were particularly difficult to place, since small apartments were extremely scarce.

On the basis of the above description, we think that it is realistic to treat the neighborhood assignment as exogenous with respect to the random components of the outcomes of interest, *conditional on observed characteristics*.

We base our empirical work on immigrants receiving their residence permit in 1990 and 1991. The reason for choosing these two years is that we have access to population micro data during 1990—2000. Thus, the immigrants will have been in Sweden for 9—10 years at the last point when we can observe outcomes.

2.2   The Swedish social assistance system

In comparison with other countries, the Swedish social assistance (SA) system is quite generous in terms of the levels of the benefits (Franzén, 2003). Anyone who has the right to live in Sweden can also become eligible for SA. There are no restrictions in terms of, e.g., household type or time spent in Sweden.

SA is "the last resort" of the Swedish welfare system. In contrast with e.g. unemployment benefits, the receipt of SA is means-tested and may require that assets are realized. Since 1998 there is a national norm concerning the monthly income level guaranteed by SA. The norm is differentiated with respect to the number of household members and their ages. The

regulations also grant compensation for other "reasonable" costs (e.g. housing and commuting), which results in a substantial amount of discretion. The discretion also applies to whether the individual is required to realize assets before receiving SA. In general, the implementation appears to be stricter for long-term than for short-term SA receipt (The National Board of Health and Welfare, 2002).

SA receipt is also more common in some household types than in others: about one quarter of all single females with kids received SA in 2001. At the same time point, the fraction of all households receiving SA was 6 percent. More than 30 percent of the recipients were long-term dependent—i.e. received SA for at least ten months during 2001. Since 1985, SA is used for supporting newly-arrived refugees and tied movers. As already mentioned, SA is much more prevalent in parts of the immigrant population than in other groups. Excluding recent refugees, payments to immigrant households accounted for close to fifty percent of the money spent on SA.[7] A contributing reason to long-term dependence particularly among immigrants is non-eligibility for other types of welfare payments, e.g. unemployment insurance. About one third of those who received SA in 2001 were unemployed but not eligible for unemployment insurance at some time point (The National Board of Health and Welfare, 2002).

3   Data and identification strategy

In this section we describe the data and the sample selections (section 3.1). Then we present the characteristics of individuals assigned to particular neighborhoods and discuss our identification strategy (section 3.2).

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[7] Here, an immigrant household is defined as a household where both adult members (if applicable) are foreign-born.

3.1   Data and sample selection

The empirical analysis is based on population micro data collected by the IFAU (Institute for Labour Market Policy Evaluation) – the IFAU data base. The data base consists of a collection of register information such as information from the income tax registers and population registers. The data are available from 1990 until 2000. The great advantage of using these data is that we can calculate measures of the characteristics of the population from each source country at the neighborhood level. These measures are free of the small sample error that will plague all estimates deduced from samples of the population. We can thus calculate measures of neighborhood characteristics for much finer geographical units than in our previous work.[8]

A potential drawback of the data is that small source countries are aggregated with other source countries for confidentiality reasons. In the appendix we present the origin countries, or the country aggregates, of the individuals included in the analysis. For the most part we think that these aggregations of countries are rather innocuous.[9] We have also examined whether these aggregations pose a problem for us by estimating regressions including only countries that we can identify uniquely. The results reported in section 4 only change marginally.

All individual variables in our analysis are based on register information. Welfare receipt and some of the other characteristics (gender, age, education, and family composition) are obtained from the income tax registers, which also contain information on country of birth and year of immigration from the population registers. Throughout we use ethnicity as a short

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[8] Due to measurement error considerations, neighborhoods were identified at the municipality level in Edin et al. (2003) and we used the number of foreign citizens rather than the number of individuals from a particular source country.
[9] Some aggregations are clearly unfortunate, such as aggregating Israel with the rest of the Middle East. But note that individuals born in Israel represent a very small share of the sample (0.5 % according to the estimates in Table A.3) so they will have little influence on the estimates.

hand for country of origin, or country aggregate, although it is the latter information that the data contain.

Neighborhood characteristics are measured at the parish level.[10] In 2000, there were 2,482 parishes and the median parish had 590 inhabitants of working age. However, the median *individual* lived in a parish with 8,660 inhabitants. For our refugee sample this figure is even higher: 14,148 in 2000. Descriptive statistics, along with the definition of some of the key variables, are reported in the Appendix.

We cannot identify refugee immigrants directly from our data. Instead we identify them by country of origin. As a general rule we include immigrants from countries outside Western Europe that were not members of the OECD as of 1985. The only exception from this rule is Turkey, which is included since it was the origin of a substantial inflow of refugee immigrants during the period.

We exclude persons belonging to a household with an adult already residing in Sweden, since they were likely to have immigrated as family members and, consequently, were not placed. We base our analysis on individuals aged 18–55 at the time of entry into Sweden. Lastly, we focus on the immigration waves during 1990–91 for reasons outlined above.

Imposing all of these restrictions we are left with a sample of 22,556 individuals. There are 889 parishes of assignment. Most immigrants are of Middle Eastern (e.g. Iran and Iraq) or African origin (e.g. Ethiopia and Somalia); see Table A3 for more details.

Another feature of the data that may be relevant for our analysis is that we observe the region of residence at the end of the year. Thus, the observed initial location may differ from the actual initial placement if individuals move during their first year. This introduces a measurement error in initial placement. In Edin et al. (2003) we investigated the importance

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[10] Refugee immigrants were assigned to an apartment so, in principle, there is scope for defining even smaller "neighborhoods". However, the parish is the smallest geographical unit that we can identify in the data.

of this problem by using a weighting scheme based on aggregate data on municipal refugee reception from the Immigration Board. The weighted estimates were very similar to the non-weighted ones, suggesting that the measurement error is not a big concern. Unfortunately, we are unable to perform a similar analysis in this paper, since there is no information on refugee reception at the parish level.

3.2 Issues related to the identification strategy

Our identification strategy relies on the presumption that the characteristics of the initial placement are independent of unobserved individual characteristics given the observed ones. In table 1, we present some information related to this assumption. The table reports the mean characteristics of individuals that were assigned to neighborhoods characterized by a high and low degree of welfare dependence, respectively. To get this division of the data, we first regressed the share of the ethnic group on welfare in the neighborhood on a set of ethnicity dummies. Then we divided the residual distribution into quartiles. The top quartile is referred to as highly welfare dependent neighborhoods, while the bottom quartile is comprised of neighborhoods where welfare dependence is low.

Table 1 shows that individuals are systematically sorted into neighborhoods. Bigger families and less educated individuals were more likely to be assigned to neighborhoods where welfare dependence was high. This sorting on observed characteristics may cause concern. Still, the argument we are making is that assignment was random conditional on observed characteristics and placement was explicitly conditional on education and family size. We have also probed deeper into this issue and looked at the distribution of characteristics that were not explicitly linked to placement across the two types of neighborhoods conditional on family size and education. It turns out that there are no differences in terms of marital status and age. Gender is still systematically different across the two kinds of neighborhoods, however. Finally, note, that we can estimate separate

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regressions for different demographic groups; this is of great interest in its own right, but it is also a way to validate the conditional independence argument that we are making. Remember that, e.g., singles individuals were very difficult to place since vacant small apartments were extremely scarce. This suggests that it is very unlikely that singles were able to influence placement, i.e., they were most likely "exogenously placed".

Table 1. Individual characteristics by type of neighborhood assignment

| | Neighborhood where welfare dependence is… | |
	high	low
Female	0.42	0.47
Age	30.7	30.6
	(8.0)	(8.4)
Years of schooling at assignment	10.5	11.1
	(2.8)	(2.8)
Years of schooling in 2000	11.6	11.9
	(3.1)	(3.2)
Married and cohabiting	0.46	0.32
# kids	1.3	0.8
	(1.6)	(1.3)

Notes: The neighborhood classification is based on the variation within ethnicity. Characteristics that are time-varying are generally measured at the time of assignment. The exception from this rule is education where there is a substantial amount of missing values at the time of entry.

It may also be of interest to have an idea about the kind of neighborhoods the refugees were assigned to. In table 2 we compare the neighborhood of assignment with the typical neighborhood of residence in the Swedish population. Table 2 shows that the refugees were assigned to more populous areas, with a greater share of immigrants as well as welfare recipients, and a lower employment rate. The differences relative to the overall population concurs with our understanding of how the placement policy was implemented. Still, it is worth noting that the differences across neighborhoods are much smaller than if we would have based the comparison on the residential distribution of refugees after 9-10 years in Sweden (i.e. in 2000) to that of the overall population at the same point in time.

Given that the characteristics of the initial placement are exogenous (at least conditionally) we can potentially use them as instruments for the characteristics of the neighborhood 9-10

years later. This implies that the predictive power of the instruments derives mainly from individuals who stayed on in the assigned residences and it is this group of individuals that we primarily identify the effects for.

Table 2. Characteristics of assigned neighborhoods, means

	Refugees	Overall population
Size of neighborhood (population)	10,614	9,687
Immigrant density (%)	15	12
Share of welfare recipients (%)	8	6
Employment rate (%)	77	79

Notes: The neighborhood characteristics are measured at the time of assignment.

What are the characteristics of those who stayed relative to those who moved? How many moved on to another neighborhood? In Table A2 we present descriptive statistics by mobility status. We first note that 74 percent of the sample has moved to another parish within nine to ten years after entering Sweden. The rate of mobility may seem high; however, it is a generic feature of the Swedish immigration experience that there is substantial mobility out of the initial location; see Åslund (2000). Despite the high rate of mobility out of the assigned parish, our instruments—i.e. the characteristics of the assignment—have substantial predictive power in the first stage regressions; we return to this issue in the presentation of the estimates.

The differences in individual characteristics are broadly in line with what one would expect in any analysis of mobility. Movers are more likely to be young and single, and have more education than stayers. There are some differences across source regions. Refugee immigrants from the African Horn and Iran tend to move to a greater extent than immigrants from other regions.

In terms of the local characteristics 9-10 years after immigration, movers tend to live in neighborhoods where there is a greater number of fellow countrymen and a greater prevalence of welfare receipt among these countrymen. Nevertheless, it is interesting to note that movers

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reduced their exposure to welfare receiving countrymen by moving (not shown in table); this exposure was thus even higher in the neighborhood of assignment.

Using the characteristics of the initial placement as instruments in an IV approach also requires the assumption that these characteristics are excludable from the outcome equation when conditioning on the characteristics of the current residence. If this exclusion restriction does not hold we have misspecified the equation for *movers*. Therefore, it is useful to look at the timing of the moves for the sub-population of movers. It turns out that the vast majority of movers leave the assigned neighborhood shortly after assignment. 80 percent of movers left the initial placement within three years and 90 percent of movers have left for another municipality within five years. The exclusion restriction we are making thus amounts to saying that the history of neighborhood characteristics, say, five years back, has no direct effect on the outcome of interest. We think this is a rather palatable assumption and hence we focus on the IV results in the empirical analysis. Nevertheless, we also discuss alternative ways of interpreting the evidence, which do not hinge on the validity of the exclusion restriction.[11]

4   The effects of ethnic enclaves and welfare cultures

There is much work – especially in sociology (e.g. Portes, 1987) – that emphasizes network effects. According to this literature, there is an upside and a downside to living in an enclave. On the one hand, the enclave may represent a network that increases the opportunities for gainful trade in the labor market and disseminates valuable information on, e.g., job opportunities. On the other hand, the enclave may fail to provide positive role models (Wilson, 1987) and may provide information on welfare eligibility (Bertrand et al., 2000).

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[11] If initial exposure matters on top of current exposure, i.e., if there is "scarring", the exclusion restriction is not correct; see Åslund and Roth (2003) for an analysis of scarring in this context.

Before proceeding to the results it is useful to make clear what kind of mechanisms the network effects we are estimating will capture. Although we argue that our estimates do not suffer from the simultaneity bias usually encountered in the literature, we do not attempt to distinguish between endogenous and exogenous social interactions.[12] Thus, an effect of the quality of the network on individual welfare receipt may reflect the attitudes, behavior, and the information of the network, as well as the exogenous characteristics of the network.

In the setting we are considering, however, the network will probably not function as a provider of information on the workings of the welfare system in general. The reason is that an ingredient of the placement policy was that all refugee immigrants were placed on welfare by default. So the refugees that we are considering should already know how the system in general worked. Nevertheless, information may still be part of the story. There is a considerable amount of discretion on the part of case workers. So, information on local variations in the implementation of the rules may well be provided by the network.

Another distinguishing feature of our study is that we are looking at how the welfare use of new entrants into the country is affected by the stock already residing in the country. These new immigrants may be particularly susceptible to social interactions and network effects. New entrants are probably more susceptible to the characteristics and behavior of their peer group than individuals that have been on the market for some time.

The remainder of this section is devoted to our estimation results. Section 4.1 presents a set of baseline estimates. We examine whether individual welfare dependence is causally affected by ethnic concentration and welfare use in the ethnic group. We view ethnic concentration as a measure of contact availability and welfare use in the ethnic group as a measure of the "quality" of contacts. Section 4.2 relaxes some of the assumptions of the baseline specification. Section 4.3 examines whether the effects of quality and quantity vary by the

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[12] The terminology comes from Manski (1993) and Moffitt (2000).

severity of welfare dependence, and in section 4.4 we examine whether the effects are different in various sub-sets of the population.

4.1   Baseline estimates

As outlined earlier, the basic strategy to free the estimates of simultaneity bias between welfare use and the size and characteristics of the local population is to use the placement policy to obtain instruments for local variables. In effect, we use variables pertaining to the initial (assigned) neighborhood as instruments for neighborhood characteristics nine to ten years later. Our maintained assumption is that the placement policy is independent of unobserved individual characteristics. Moreover, we assume that (initial) location does not have permanent effects on outcomes.

Our basic specification of the outcome equation is the following

$$W_{ipct} = \mathbf{X}_{it}\beta + \alpha \ln W_{pct} + \delta_{p_0} + \delta_c + \delta_{t_0} + \varepsilon_{ipct} \tag{1}$$

where i indexes individuals, p parishes, c countries of origin, and t time. p_0 denotes the initial parish of assignment and t_0 is the year of immigration. The dependent variable equals unity if the individual is a member of a welfare receiving household in $t = 2000$ and zero otherwise; households are classified as being on welfare as long as they have received a positive amount during the year.[13] We standardize for a set of individual characteristics $\mathbf{X}$, containing gender, age, age squared, marital status, education, and dummies for the number of kids in the household. In order to control for omitted variables, the regression includes a full set of country of origin fixed effects (δ_c), immigration year fixed effects (δ_{t_0}), and fixed effects for the assigned parish (δ_{p_0}). We only control for the assigned

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[13] Notice that we estimate a linear probability model rather than a logit or a probit. The main reason is that we want to apply instrumental variables, which is more cumbersome in a non-linear model.

neighborhood, as the coefficients on the resident neighborhood in period t may be endogenous because of sorting and instrumenting the full set of parish effects is simply too taxing on the data.

Welfare use for individual i of ethnic group c is also related to the number of (other) welfare recipients among immigrants from the particular ethnic group in parish p (W_{pct}).[14] Notice that the coefficient on the number of welfare recipients in the ethnic group is identified since it varies across ethnic groups within parishes (and across parishes within ethnic group). The number of welfare recipients in the ethnic group reflects both the quality and quantity of contacts. We also decompose this measure and attach separate coefficients on the component parts, i.e., $\alpha \ln W_{pct} = \alpha_W \ln(W/E)_{pct} + \alpha_E \ln E_{pct}$. Using this decomposition we can in principle test whether quantity (E_{pct}) or quality ($W/E)_{pct}$ is most important for individual behavior.

We begin by estimating equation (1) for the full sample. The first stage equations in the IV (2SLS) procedure, amount to regressing, e.g., $\ln W_{cpt}$ on $\ln W_{cp_0t_0}$ and the remaining explanatory variables in (1). Thus, in this instance, we regress the number of welfare recipients from ethnicity c in the individuals current place of residence (p) on the number of welfare recipients from ethnicity c in the assigned neighborhood (p_0) at the time of immigration (t_0). This first stage regression has strong predictive power: the number of welfare recipients in the ethnic group in the assigned parish enters the equation with a coefficient of 0.17 and a t-ratio of 16.06. All first stage regressions that we have run suggest that the instruments have substantial predictive power.

Table 3 reports the results for the full sample where the outcome of interest is the probability of welfare receipt. Columns (1) and (3) report OLS estimates where we treat the characteristics of the neighborhood as exogenous. Column (2) and (4) report the results of the

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[14] In calculating the characteristics of the ethnic group in a particular neighborhood, we exclude the inflow (and their family members), i.e., the individuals in our analysis sample. If the individuals in our analysis sample and the population for which we calculate neighborhood characteristics would be identical, then the mechanics of the linear probability model drives the coefficient on the neighborhood characteristic towards unity. Given that we exclude the inflow there is nothing mechanical about the estimates.

IV procedure outlined above. Column (5), finally, reports the reduced form results, i.e., the results from the regression of individual welfare use at time t on, *inter alia*, the characteristics of the *assigned* neighborhood at time t_0.

Table 3. Estimates for the full sample. Dependent variable: welfare use

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) Reduced Form
ln(# welfare recipients from same ethnic group)	.032	.010			
	(.003)	(.023)			
ln(fraction of welfare recipients in ethnic group)			.056	.263	.021
			(.006)	(.087)	(.007)
ln(# individuals from same ethnic group)			.028	−.004	−.004
			(.003)	(.023)	(.004)
# individuals	22,556	22,556	22,556	22,556	22,556
Mean of the dependent variable	0.390	0.390	0.390	0.390	0.390
Standard error of regression	.43	.43	.43	.45	.44

Notes: Standard errors in parentheses. All regressions control for (a quadratic in) age, marital status, cohabitant status, dummies for #kids in the household, educational attainment, country of birth fixed effect, neighborhood fixed effects, and immigration year fixed effects. In Table A4 we report the full set of estimates for the specifications in columns (3) and (4). IV estimation is by 2SLS using the characteristics of the assigned neighborhoods as instruments for the characteristics in 2000. Characteristics of the neighborhood refer to time t in columns (1)-(4) and to time t_0 in column (5). Robust variance estimates, allowing for correlation across individuals residing in the same parish.

According to the OLS estimates in column (1) there is a positive association between the number of welfare recipients in the neighborhood and individual welfare use. A doubling of the number of recipients from the individual's ethnic group raises welfare dependence by three percentage points (see column 1). According to the corresponding IV estimate, the OLS coefficient appears to be upward biased.

The bias of OLS is more striking when we turn to the decompositions in columns (3) and (4), where the IV estimates are significantly different from the OLS estimates. Nevertheless, the qualitative conclusion is similar is one respect: Both sets of estimates imply that the quality of contacts is more important than contact availability. Moreover, there is a downward bias in the OLS estimate on the quality of contacts and an upward bias in the coefficient on

contact availability. In fact, there is literally no effect of contact availability in economic as well as statistical terms.[15]

The fact that there is a positive sorting bias in the OLS estimate on ethnic concentration is consistent with our findings in Edin et al. (2003): according to the IV-estimates there was no effect on earnings when all skill groups were pooled together and a positive effect for the low-skilled; the corresponding OLS estimates were all negative.

The sorting pattern implied by the comparison of the OLS and IV coefficients on $\ln(W/E)_{pct}$ may seem surprising. We argued earlier that the reflection problem causes an upward bias in the simple OLS estimate. Now, since equation (1) includes neighborhood and country of origin fixed effects it is more sophisticated than commonly encountered specifications, where simultaneity concerns apply directly. Excluding these fixed effects, the OLS estimate on the fraction of welfare recipients from the ethnic group is almost double the size of the IV estimate in column (4).

Nevertheless, it is relevant to ask: What kind of sorting story motivates the increase in the IV estimate relative to the OLS estimate on the fraction of welfare recipients? Our interpretation goes as follows. Think of a world where sorting bias is driven by the behavior of the low-skilled (in the unobserved sense) and suppose the main motivation for these individuals is to gain employment (or stay off welfare).[16] For these low-ability individuals it

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[15] We have subjected the baseline specification to a number of sensitivity checks. One of the most important ones concerns the possibility of a remaining correlation between unobserved individual characteristics and the initial neighborhood characteristics that we use as instruments. Although we have taken due care to exclude the individuals in the sample and their household members when calculating neighborhood characteristics, one may be concerned that the behavior of the sampled individuals influences the surrounding environment. This is a social interactions model implying that there is feedback from the studied individuals onto the remaining members of the network. To investigate whether this was an issue, we restricted the analysis to individuals entering the country in 1991. As instruments we used neighborhood characteristics in 1990. The point estimates of the network effects were actually slightly higher using this approach than in our baseline approach. Thus endogenous feedback from the studied individuals to the neighborhood characteristics is not a concern. Furthermore, the results are not sensitive to outliers and the functional form. Using the actual share of welfare recipients (rather than the log of the share) produces the same qualitative results.

[16] The characteristics of the neighborhood may be particularly important for low-skill individuals. High-skill individuals presumably make their way in any kind of environment. Notice also that this kind of mobility pattern is consistent with what we observe in the data. Those who moved entered neighborhoods where welfare receipt in the ethnic group was lower than in the assigned neighborhood.

is then rational to opt for a high-skill neighborhood because such a neighborhood increases employment opportunities and reduces the risk of ending up on welfare (see Edin et al., 2003). To substantiate this story we performed the following exercise. As a measure of the quality of the network we introduced the fraction of employed members in the ethnic group rather than the share of the group on welfare. Then we re-estimated the equations using OLS as well as IV. The results were consistent with those reported in Table 3. In both the OLS and the IV the share of employed members of the ethnic group entered the equations significantly negative. Moreover, the OLS coefficient is biased downward in absolute value by a factor of almost five, suggesting that the low-skilled indeed sort into high-skilled neighborhoods.[17]

The magnitudes of the social network effects reported in Table 3 strike us as large. The IV estimates in column (4) imply that individual welfare receipt increases by 2.6 percentage points in response to 10 percent increase in the share of the ethnic group on welfare; this corresponds to a relative increase of 6.7 percent. Alternatively, we can evaluate the estimates at a point corresponding to the typical variation available in the data. To do that, we calculated the weighted average of the standard deviation in the fraction of welfare recipients within ethnicity across neighborhoods. This calculation implies that in response to a standard deviation increase in the (log of the) fraction of welfare recipients (0.55) the individual probability of welfare receipt rises by 14.5 percentage points.

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[17] A relevant question to ask is the following: What happens if one controls for the ethnic employment rate in addition to the share of the ethnic group on welfare? The answer to this question is that the employment rate and the welfare participation rate in the initial assignment both have independent and statistically significant effects on welfare use 9-10 years later. Relative to the reduced form in column (5), the coefficient on ethnic welfare use is reduced by almost 25 percent. We prefer the parsimonious specification in column (5) since the extended specification does not really facilitate interpretation. The employment rate and the welfare participation rate are so intimately related and both measures could reflect basically the same thing. Even if controlling for the ethnic employment rate would reduce the coefficient on ethnic welfare use to zero, this does not imply that behavior and information are unimportant for individual welfare use.

4.2   Relaxing some assumptions of the baseline specification

As noted earlier, the IV estimates require the exclusion restriction that the characteristics of the initial placement do not have permanent effects on the outcome. The reduced form estimates, reported in column (5), impose no such restriction. They can be seen as a test of whether the characteristics and behavior of the surrounding environment matter for individual outcomes. Given the estimates reported in column (4), it is unsurprising to see that the size of the enclave has no effect on individual welfare use – what matters is the share of welfare recipients in the ethnic community.

The "reduced form" relationship between individual welfare receipt in 2000 and the characteristics of the assigned neighborhood can also be interpreted in the context of the refugee placement policy. It seems that exposing individuals to welfare-intense environments initially made them more likely to become long-term welfare recipients. Placing individuals in a neighborhood where welfare receipt in the ethnic group was ten percent higher than normal increased the individual probability of "long-run" welfare use by 0.2 percentage points.

Moreover, the reduced form also lends itself to a structural interpretation. Suppose we were interested in the question: What is the long-run effect (cumulated over 9-10 years) of placing individuals in environments with marginally higher welfare use?[18] If one is interested in this question one should correct the estimates for the fact that not all individuals complied with the assigned treatment. In fact, 74 percent moved on to a different neighborhood than the assigned one. The answer to the above question is thus obtained by dividing the reduced form coefficient by the probability of remaining in the assigned neighborhood (0.26). So, if ethnic welfare use was ten percent higher than normal in the assigned neighborhood, the long-run effect—cumulated over 9–10 years—is to increase the individual probability of welfare use by 0.8 percentage points.

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[18] Notice that this is a different question that the one implicitly posed in equation (1). In equation (1) we purport to estimate the contemporaneous relationship between neighborhood characteristics and individual welfare use.

The baseline specification is based on the rather extreme assumption that it is only the current characteristics of the ethnic community that matters for current welfare receipt. Of course, we are not literal believers in this assumption – it seems plausible that the history of neighborhood characteristics is also of some importance for welfare receipt at time t. We have examined whether history matters in a variety of ways. The results of these analyses suggest the history of neighborhood characteristics has some influence but their importance declines rather rapidly.[19]

We have also investigated the possibility that the interaction of quality and quantity is important; c.f. Bertrand et al. (2000). In other words, does the impact of frequent welfare receipt in the ethnic group vary with the size of the group? Adding the interaction between ln(size of the ethnic group) and ln(fraction of welfare recipients in the ethnic group) to specification (4) gives an estimate of 0.014 (0.034) on the interaction term, suggesting that the answer to this question is no. The main effect of quality is far more important than the interaction between contact availability and quality.

4.3 Do the neighborhood effects differ depending on the severity of welfare dependence?

We have so far used a strict measure of welfare receipt: if the individual (household) receives any amount of welfare at any time during a particular year, he or she is classified as a welfare recipient. Since it is possible that neighborhood effects differ depending on the severity of welfare dependence, table 4 presents results for alternative thresholds for welfare receipt. The

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[19] For instance, we have estimated models assuming that the history is *as* important as the current characteristics. Under this assumption, we get a specification where it is the cumulative exposure to varying enclave characteristics that matters for welfare receipt at time t. Maintaining the assumption that the initial exposure is excludable from the outcome equation, conditional on cumulative exposure, we can estimate the alternative specification using standard IV. We have estimated such models for a variety of lags included in the measure of cumulative exposure. Cumulating exposure two years back, we get an IV estimate on the share of welfare recipients of 0.09.

alternative thresholds are based on the ratio of received welfare to the eligibility limit for the household in question, i.e. the income guaranteed by the welfare system.[20]

Table 4. Alternative thresholds for welfare use

Threshold (welfare/eligibility threshold)	Baseline	(1) 0.1	(2) 0.2	(3) 0.3	(4) 0.4
ln(fraction of welfare recipients in ethnic group)	.263	.227	.189	.179	.155
	(.085)	(.081)	(.076)	(.077)	(.064)
ln(# individuals from same ethnic group)	−.004	−.002	.001	.011	.015
	(.023)	(.022)	(.021)	(.020)	(.018)
# individuals	22,556	22,556	22,556	22,556	22,556
Standard error of regression	.44	.43	.41	.39	.36
Mean of the dependent variable	.390	.324	.267	.218	.174
Relative effect of "enclave quality"	.674	.701	.708	.821	.891
Fraction of "not eligible" receiving welfare	.210	.127	.078	.049	.031
Fraction of "eligible" receiving welfare	.676	.636	.568	.486	.400

Notes: In specification (1), an individual is classified as welfare recipient if the amount received is more than 0.1 of the eligibility limit for his/her household. In specifications (2)–(4) this threshold is set to 0.2, 0.3, and 0.4 respectively. Standard errors in parentheses. All regressions control for (a quadratic in) age, marital status, cohabitant status, dummies for #kids in the household, educational attainment, country of birth fixed effect, neighborhood fixed effects, and immigration year fixed effects. IV estimation is by 2SLS using the characteristics of the assigned neighborhoods as instruments for the characteristics in 2000. Robust variance estimates, allowing for correlation across individuals residing in the same parish. Note that the alternative threshold for welfare receipt pertains only to the dependent variable. The explanatory variables are the same in all specifications.

The first column of the table contains the baseline estimates, whereas specifications (1)–(4) use increasingly higher thresholds for welfare receipt. The estimated effects are quite similar regardless of the threshold. There is a tendency to smaller estimates with higher thresholds in the absolute sense. Alternatively, one may wish to normalize the coefficients by the mean of the dependent variable which suggests that the estimates are increasing in the relative sense (third row from bottom). However, none of the coefficients are significantly different from one another. As in the baseline case, enclave size appears to be unrelated to the outcomes. Thus, these results suggest that network effects are just as pertinent for severe welfare dependence as they are for overall welfare use.

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[20] The eligibility limit is calculated from the national social assistance standard combined with information on household composition. It also includes estimated housing costs. The accuracy of the eligibility measure is discussed further in section 4.4.

4.4   Do the effects vary across groups?

In this section we report estimates for different sub-groups. There are three main reasons for doing this. First, welfare eligibility is determined by household economic status and welfare generosity is a function of household characteristics: young single mothers may have easier access to welfare than a couple without kids.[21] Second, we know from the description of the placement policy that the argument for exogenous placement is stronger for certain demographic groups. Third, there is also a methodological twist to running separate regressions for different sub-groups. Wooldridge (2003) has recently shown that conventional two-stage least squares estimates (2SLS), including the interactions between the endogenous variable(s) and the exogenous characteristics, consistently estimates an average treatment effect (if there is treatment heterogeneity) under weaker conditions than the "plug-in" estimators proposed by Heckman and Vytlacil (1998).[22] Running separate regressions for groups with certain traits is a convenient way of allowing for interactions between the trait in question and the endogenous variable.

We estimated the specification of column (4) in Table 3 for sub-samples divided along two dimensions: (i) singles and cohabitants; (ii) kids present in the household or not. All in all, this yielded nine sub-samples (including the full sample and sub-samples divided only in one dimension). Welfare use is prevalent in all kinds of households; for instance, 30 percent of singles without kids received welfare payments during 2000. This number is much higher for singles with kids (54 percent), but, in contrast to the US, welfare use is clearly not restricted to single mothers only.

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[21] Note, though, that there is no necessary link between household type and the importance of the peer effects studied here.
[22] Heckman and Vytlacil (1998) criticized applying standard 2SLS to a setting where there is treatment heterogeneity, because such an approach either requires that there is no heterogeneity or that individuals do not act on heterogeneity.

Table 5 presents a selection of these regression results. It reports the estimates for the full sample, for households were there is at least one kid present, for households containing only one adult member (but kids may be present), and for singles with kids. The main message of the estimations is that the characteristics of the enclave have similar effects across different types of households. In all kinds of households it seems that the quality of contacts is what matters for welfare use. Although, the point estimates on quality differs somewhat, the relative effects presented in the bottom row of the table are practically constant and hover around 0.7. The lack of statistical significance for singles with kids is presumably driven by sample size relative to the vast number of fixed effects that we are estimating.

Table 5: Estimates by household type. Dependent variable: welfare use

	Full sample	Kids in household	Singles	Singles with kids
ln(fraction of welfare recipients in ethnic group)	.263	.295	.251	.382
	(.085)	(.115)	(.129)	(.265)
ln(# individuals from same ethnic group)	−.004	−.023	−.013	−.043
	(.023)	(.030)	(.039)	(.071)
# individuals	22,556	14,266	9,565	2,815
Standard error of regression	.44	.45	.45	.47
Mean of the dependent variable	.390	.442	.368	.536
Relative effect of "enclave quality"	.674	.667	.682	.713

Notes: Standard errors in parentheses. In general, the regressions control for (a quadratic in) age, marital status, cohabitant status, dummies for #kids in the household, educational attainment, country of birth fixed effect, neighborhood fixed effects, and immigration year fixed effects. All regressions are estimated by 2SLS using the characteristics of the assigned neighborhoods as instruments for the characteristics in 2000. Robust variance estimates, allowing for correlation across individuals residing in the same parish.

We noted earlier that singles are the ones that were most likely to be exogenously placed. Therefore, it is reassuring to see that the neighborhood effect estimated for singles is virtually identical to the one estimated for the full sample.

It would be interesting to decompose the neighborhood effect on welfare utilization into the effect on the take-up rate, i.e. welfare use conditional on eligibility, and the effect on welfare eligibility. Although interesting, this is a very hard question. Estimating the effect on take-up by conditioning on eligibility (or income) directly in the equations is likely to be

unproductive. The reason for this is that the individual unobserved components that influence welfare receipt probably also influence income, i.e., income is not exogenous to the availability of welfare. Moreover, any attempt to instrument eligibility (or income) will have to rely on an exclusion restriction that is bound to be arbitrary. Another approach would be to back-out the effect on take-up using estimated effects on welfare use and welfare eligibility. However, there is no information on eligibility in the data and there seems to a good deal of discretion on the part of case-workers in determining the eligibility.

Nevertheless, we have made an attempt to estimate eligibility from observations on annual household income. There are several sources of measurement error in this estimate. First, welfare eligibility is determined by household income during a particular month rather than the full year. Second, we are unable to include exact housing expenditure or other extra costs covered by welfare. Third, we do not observe assets that may hinder welfare eligibility. The two latter problems are related to the discretionary element of the welfare system, which may of course also operate in other dimensions. A flavor of the extent of measurement error was given already in Table 4. With our baseline definition of welfare receipt, 21 percent of those that we classify as "non-eligible" are actually receiving welfare. When we consider more severe forms of welfare dependence the extent of misclassification drops but it does not go away completely; see the last column of table 4.[23]

All in all, whether the neighborhood effects vary by eligibility status is a very hard question to answer. What we can do is ask whether the neighborhood effects vary by earnings potential (i.e. their predicted eligibility status). This is what is done in table 6, where we present estimates by quintiles of the predicted earnings distribution. The basis for the earnings prediction is a regression using the stock of individuals from refugee source countries residing

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[23] We have made an attempt to examine whether welfare eligibility is causally affected by neighborhood characteristics. This analysis seems to convey a similar message as our analysis of welfare use. However, the IV estimates were imprecise, which presumably reflects the substantial difficulties associated with measuring eligibility.

in Sweden in 2000. This earnings regression includes a standard set of covariates (educational attainment, gender, age, age squared, country of birth etc.). The coefficients from the regressions are used to predict earnings in the sample that we are analyzing.[24]

Table 6. Estimates by predicted earnings. Dependent variable: welfare use

Predicted earnings quintile	(1)	(2)	(3)	(4)	(5)
ln(fraction of welfare recipients in ethnic group)	.283	.255	.199	.165	.160
	(.083)	(.087)	(.063)	(.085)	(.062)
ln(# individuals from same ethnic group)	.003	−.038	.025	−.011	.000
	(.022)	(.025)	(.024)	(.028)	(.022)
# individuals	4,512	4,512	4,511	4,512	4,509
Standard error of regression	.49	.51	.49	.48	.43
Mean of the dependent variable	.553	.412	.374	.349	.262
Relative effect of "enclave quality"	.512	.619	.532	.473	.611

Notes: Standard errors in parentheses. All regressions control for (a quadratic in) age, marital status, cohabitant status, dummies for #kids in the household, educational attainment, country of birth fixed effect, neighborhood fixed effects, and immigration year fixed effects. IV estimation is by 2SLS using the characteristics of the assigned neighborhoods as instruments for the characteristics in 2000. Robust variance estimates, allowing for correlation across individuals residing in the same parish. The "relative effect of enclave quality" is the point estimate of the first row divided by the mean of the dependent variable in the respective quintile.

The labor market success of refugee immigrants in Sweden has been rather poor during the 1990s; see Edin et al. (2000). Therefore, one may fear that even a refugee immigrant predicted to be in the top quintile has low earnings relative to natives. However, this is not quite the case. A refugee immigrant predicted to be on the 75th percentile in the analysis sample is ranked on the 40th percentile in the native earnings distribution. So, the observed characteristics of the refugees spans some range in the native earnings distribution. They are not all predicted to be low-income earners.

Table 6 conveys a message that we have seen several times before. It is the quality of the enclave that matters for individual welfare use. The fraction of welfare users in the community has a positive and significant effect on individual welfare use for all skill groups; the size of the enclave has no effect. The magnitude of the estimate on quality declines in

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[24] To avoid omitted variable bias, we included a set of municipality fixed effects in estimating the regressions. However, earnings were predicted as if all individuals resided in the same municipality.

more skilled groups. But the estimates are not significantly different from one another and the relative effects of an increase in quality are remarkably stable. Thus, the predicted eligibility status of the households does not matter much for the effect of neighborhood characteristics on welfare use.

5   Concluding remarks

The main purpose of this paper has been to examine whether the size and characteristics of the enclave has a causal effect on welfare use. To this end, we have made use of an immigrant policy initiative in Sweden, when government authorities distributed refugee immigrants across locales in a way that we argue is exogenous. This policy initiative provides a quasi-experiment, which allows us to handle the endogeneity problem due to the individuals' residential choice. Throughout we have distinguished between the quantity of contacts (the size of the enclave) and quality of contacts (the welfare culture).

The empirical analysis suggests two main conclusions. First, we find that the quality of contacts has a sizable and positive effect on individual welfare use; there is no effect coming from the size of the enclave. Individual welfare use increases by almost 7 percent (2.6 percentage points) in response to an increase in the fraction of welfare dependents in the ethnic group by 10 percent. Second, when we break down the analysis to households with different demographic characteristics and different earnings potential it is again ethnic welfare use in the neighborhood that matters; furthermore, the relative effect of changes in the quality of contacts is broadly the same across groups. The divisions of the sample that we have considered are clearly related to the economic status of the household, which is the determinant of welfare eligibility. The results thus imply that that the neighborhood effects that we are considering do not vary much across households defined by predicted eligibility status.

In sum, the characteristics of the social network seem to have important effects on individual outcomes. In our setting, the quality of the contacts proves to be far more influential than the number of contacts. An interesting issue is why quality is important. As such, the quality of contacts may reflect norms and information (endogenous social interactions) as well as exogenous characteristics of the contacts (exogenous social interactions). Since we have made no attempt to distinguish between endogenous and exogenous social interactions we cannot answer this question.

However, we do think that information is not such a big part of the story in our setting. The policy experiment that we are using had the particular feature of introducing all refugee immigrants to welfare upon arrival. Therefore, it is likely that the enclave loses some of its role as an information provider on, e.g., the rules of the welfare system. This observation suggests that our estimates may represent lower bounds on the effects of the characteristics of the enclave on individual behavior: if there would be an information aspect to the network, the estimates would potentially be larger in size. Having said this, we note that recent research have found little support for the information aspect. Aizer and Currie (2002) find that the presence of network effects cannot be explained by information sharing within members of the network.

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Appendix

Table A1 reports the definitions and primary sources of data for variables used in the empirical analysis. Information on gender, age, and marital status come from the Income tax registers. The definition of these variables should be obvious and are not included.

Table A1: Variable definitions and primary sources of data

Variable	Definition	Primary source of data
Individual characteristics		
Welfare receipt	Dummy for the incidence of welfare receipt in the household.	Income tax registers, Statistics Sweden (SCB).
Welfare eligibility	Dummy for being a member of a household eligible for welfare. The income cut-off is the sum of the national norm (varying by the number of adults and kids in the household) and the average municipal rent in public rentals.	Income tax registers, SCB, + additional information.
Education	Highest degree attained.	Education register, SCB.
Kids	Dummies for the number of kids ≤ 15 years of age in the household.	Income tax registers, SCB.
Immigration year	Year of receipt of residence permit.	Population register, SCB.
Country of origin	Immigrant source country; see Table A3 for more details.	Population register, SCB.
Local/group characteristics		
Size of ethnic group (E_{cpt})	Number of individuals from source country c residing in parish p at time t.	Population register, SCB.
Number of welfare recipients from an ethnic group (W_{cpt})	Number of individuals from source country c residing in parish p at time t who are members of a welfare receiving household.	Population register and Income tax register, SCB.
Fraction of welfare recipients in ethnic group	W_{cpt}/E_{cpt}	Population register and Income tax register, SCB.

Table A2 presents means and standard deviations of the variables relevant for the empirical analysis.

Table A2: Summary statistics: Means (Standard deviations).

Variable	All	Stayers	Movers
Local/group characteristics			
ln(# welfare recipients from ethnic group in the neighborhood)	3.787 (1.832)	3.359 (1.768)	3.933 (1.831)
ln(# individuals from ethnic group in the neighborhood)	4.947 (1.607)	4.651 (1.594)	5.048 (1.598)
ln(fraction of neighborhood ethnic group receiving welfare)	−1.160 (0.723)	−1.292 (0.778)	−1.115 (0.698)
Individual characteristics			
Welfare receipt 2000	.390	.350	.404
Welfare receipt in the year of immigration	.719	.717	.720
Eligible for welfare (defined as total income<calculated income limit)	.386	.385	.387
Age	40.052 (8.260)	41.613 (8.794)	39.518 (8.000)
Female	.429	.513	.399
Married or cohabiting	.576	.622	.560
Kid	.632	.675	.618
# kids	1.554 (1.623)	1.628 (1.568)	1.528 (1.640)
Years of schooling	11.895 (3.164)	11.579 (3.246)	11.998 (3.130)
Education: Missing	.054	.083	.044
< 9 years	.139	.172	.128
9–10 years	.139	.147	.136
High school ≤ 2 years	.179	.179	.179
High school > 2 years	.190	.158	.201
University < 3 years	.119	.098	.127
University ≥ 3 years	.179	.164	.185
Immigration year: 1990	.454	.455	.454
1991	.546	.545	.546
Region of origin: Former Yugoslavia	.059	.066	.056
Poland	.030	.041	.027
Baltic states	.005	.007	.005
Eastern Europe 1	.082	.100	.075
Eastern Europe 2	.015	.020	.013
Mexico and Central America	.019	.028	.016
Chile	.027	.047	.020
South America (excluding Chile)	.032	.040	.029
African Horn	.144	.063	.172
North Africa (Arabic countries) and Middle East	.179	.195	.173
Other Africa	.026	.031	.025
Iran	.140	.093	.156
Iraq	.106	.086	.114
Turkey	.034	.042	.031
South East Asia	.069	.098	.059
Other Asia	.034	.043	.030
# individuals	22,556	5,751	16,805

Notes: Unless explicitly stated otherwise, the variables refer to the situation in. "Stayers" are those who stayed on in the assigned neighborhood between the year of immigration and 2000. "Movers" are those who moved between these two time points.

Table A3 provides a list of the "countries" of origin, at the levels of aggregation that we can observe them, of the individuals included in the analysis.

Table A3: Source countries

"Country" of birth	Percent of sample
1. Former Yugoslavia	**5.8**
2. Poland	**3.0**
3. The Baltic states	**0.5**
Estonia	0.4
Latvia and Lithuania	0.1
4. Eastern Europe 1	**8.2**
Rumania	3.5
The former USSR	2.8
Bulgaria	1.8
Albania	0.1
5. Eastern Europe 2	**1.5**
Hungary	0.9
The former Czechoslovakia	0.6
6. Mexico and Central America	**1.9**
El Salvador	1.0
Mexico	0.2
Other countries	0.7
7. Chile	**2.7**
8. Other South America	**3.2**
Peru	1.0
Brazil	0.7
Colombia	0.6
Argentina	0.4
Uruguay	0.3
Other countries	0.3
9. African Horn	**14.4**
Ethiopia	8.3
Somalia	5.9
Sudan and Djibouti	0.2
10. North Africa (Arabic countries) and Middle East	**17.9**
Lebanon	9.7
Syria	3.3
Morocco	1.3
Tunisia	0.9
Egypt	0.7
Algeria	0.5
Israel	0.5
Palestine	0.4
Jordan	0.3
Other countries	0.3
11. Other Africa	**2.6**
Gambia	0.7
Uganda	0.3
Zaire	0.3
Ghana	0.2
Other countries	1.4
12. Iran	**14.0**
13. Iraq	**10.7**
14. Turkey	**3.3**
15. South East Asia	**6.9**

Vietnam		3.2
Thailand		1.9
the Philippines		1.2
Malaysia		0.2
Laos		0.2
Other countries		0.2
16. Other Asia		**3.4**
Sri Lanka		1.0
Bangladesh		0.8
India		0.6
Afghanistan		0.4
Pakistan		0.4
Other countries		0.1
Total (Sum of bold-faced numbers)		**100**

Notes: China and North Korea are not included in our sample despite being refugee source countries. The reason for this is that these countries are lumped together with Japan. The bold face information represents the country of birth information that we have available in the data. For aggregates of countries we also present estimates for individual countries that have been derived by combining the information in the IFAU data base with the information in the LINDA data base; see Edin and Fredriksson (2000) for a description of LINDA. As a general rule we only list individual countries as long as they represent more the 0.2 percent of the inflow.

Table A4 reports the full set of estimates for the specifications in columns (3) and (4) of

table 3.

Table A4: Full set of estimates for specifications in columns (3) and (4) of table 3

	OLS	IV
ln(fraction of welfare recipients in ethnic group)	.056	.263
	(.006)	(.086)
ln(# individuals from same ethnic group)	.028	−.004
	(.003)	(.023)
Age	−.026	−.027
	(.004)	(.004)
Age squared	.040	.042
	(.004)	(.004)
Female	.058	.066
	(.012)	(.013)
Married	.009	.006
	(.012)	(.013)
Married×female	−.046	−.048
	(.017)	(.018)
Married or cohabiting	−.054	−.050
	(.016)	(.017)
(Married or cohabiting)×female	−.055	−.058
	(.020)	(.021)
Kids present in household	.075	.068
	(.016)	(.017)
Kids×female	.018	.014
	(.017)	(.018)
# kids = 2	.030	.029
	(.010)	(.011)

# kids = 3	.137	.134
	(.014)	(.015)
# kids = 4	.252	.247
	(.019)	(.020)
# kids = 5	.341	.320
	(.024)	(.027)
# kids = 6	.426	.412
	(.031)	(.033)
# kids = 7	.502	.482
	(.042)	(.042)
# kids = 8	.317	.287
	(.070)	(.083)
# kids = 9	.536	.518
	(.039)	(.044)
# kids = 10	.477	.427
	(.149)	(.146)
Education, 9-10 years	−.036	−.033
	(.012)	(.013)
Education, high school ≤ 2 years	−.114	−.118
	(.012)	(.013)
Education, high school > 2 years	−.132	−.127
	(.013)	(.014)
Education, university < 3 years	−.129	−.125
	(.013)	(.016)
Education, university ≥ 3 years	−.208	−.199
	(.012)	(.017)
Education, missing	−.077	−.076
	(.017)	(.018)
Immigration year = 1991	.043	.038
	(.007)	(.008)
Country of origin fixed effects	YES	YES
Neigborhood fixed effects	YES	YES
# individuals	22556	22556
Standard error of regression	.43	.45
Mean of dependent variable	.390	.390

Notes: Standard errors in parentheses. IV estimation is by 2SLS using the characteristics of the assigned neighborhoods as instruments for the characteristics in 2000. Robust variance estimates, allowing for correlation across individuals residing in the same parish.