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Ethnic enclaves and the economic success of immigrants - evidence from a natural experiment

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Ethnic Enclaves and the Economic Success of Immigrants – Evidence from a Natural Experiment*

by

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Abstract

Recent immigrants tend to locate in ethnic "enclaves" within metropolitan areas. The economic consequence of living in such enclaves is still an unresolved issue. We use an immigrant policy initiative in Sweden, when government authorities distributed refugee immigrants across locales in a way that may be considered exogenous. This policy initiative provides a unique natural experiment, which allows us to estimate the causal effect on labor market outcomes of living in enclaves. We find substantive evidence of sorting across locations. When sorting is taken into account, living in enclaves improves labor market outcomes; for instance, the earnings gain associated with a standard deviation increase in ethnic concentration is in the order of four to five percent.

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1. Introduction

In most countries, immigrants tend to be spatially concentrated. In the US, for instance, recent immigrants often reside in ethnic enclaves, usually located in metropolitan areas; see LaLonde and Topel (1991). Another example is Sweden, where the share of the foreign-born population living in the three largest metropolitan areas outstripped the share of the native population by 18 percentage points in 1997.¹ This paper deals with the economic consequences of living in these enclaves.

There are many potential explanations for this location pattern:² economic incentives may dictate that new immigrants use the established networks of previous immigrants; it may also reflect the importance of ethnic ties *per se*, discrimination in the housing market, or the rational response to imperfect information.³

A fairly large, and predominantly American, literature has examined the individual consequences of living in enclaves (or “ghettos”/“neighborhoods”). Cutler and Glaeser (1997), for instance, find that blacks living in segregated areas have significantly worse outcomes than blacks in integrated areas. The earnings effects are sizable: a one standard deviation increase of segregation reduces the earnings of blacks relative to whites by 7–9 percent.

The early studies concerning the effects of segregation on individual outcomes – Kain (1968) is the seminal paper – estimated the effects treating the ethnic composition of an area within a city as exogenous. The results from this type of studies, however, are susceptible to Tiebout bias, arising because individuals choose in which community to reside. As Evans *et al.* (1992) illustrate, statistically significant neighborhood (“peer group”) effects may disappear if proper account is taken to the fact that there is scope for choosing the neighborhood.⁴

Later studies, e.g. Cutler and Glaeser (1997), have utilized the variation across

¹ The 25 largest metropolitan areas of the US hosted 75% of the immigrant population and 40% of the native population. 53% of the immigrant population and 35% of the native population lived in the three largest metropolitan areas of Sweden in 1997.

² Bartel (1989) has shown that immigrants in the US choose to reside in regions where there are other immigrants. Moreover, their location decisions are less sensitive to wage variations in comparison to the native population.

³ In the absence of other information on job market opportunities, new immigrants may use the location of previous immigrants as an indicator of labor market prospects.

⁴ Manski (1993) has even questioned whether it is possible to identify the peer group effect.

metropolitan areas, arguing that Tiebout sorting is less problematic in this case.⁵ Yet another approach to this issue is to use parental choices of neighborhoods, where the assumption is that this choice is exogenous with respect to the outcome of the offspring; Borjas (1995) is an example.

Although we agree that these later approaches may have reduced simultaneity problems, we still think that the validity of the implicit exogeneity assumptions is an open question. In the US, for instance, moves for non-housing reasons constitute almost 40 percent of total mobility; Greenwood (1997). The assumption of no sorting across local labor markets means that these moves are not the response of, say, individuals whose labor market prospects are threatened by an influx of immigrants. Indeed the US evidence suggests that increased immigration mostly hurt previous immigrants rather than natives; see LaLonde and Topel (1997). Therefore, we believe that the individual consequences of living in enclaves are still an unresolved issue.

We take a different approach. Our exogenous source of variation comes from a Swedish government policy concerning the initial location of refugee immigrants. This policy was viable between 1985 and 1991. Government authorities placed refugees in localities that were deemed suitable according to certain criteria. Initially, these criteria were supposed to be related to factors like educational and labor market opportunities. In practice, however, the availability of housing seems to have been the all-important factor. Our maintained hypothesis is that the policy change implied that the initial location of immigrants was independent of unobservable individual characteristics. Hence, this “natural experiment” enables us to reexamine the question of the economic consequences of living in enclaves.⁶

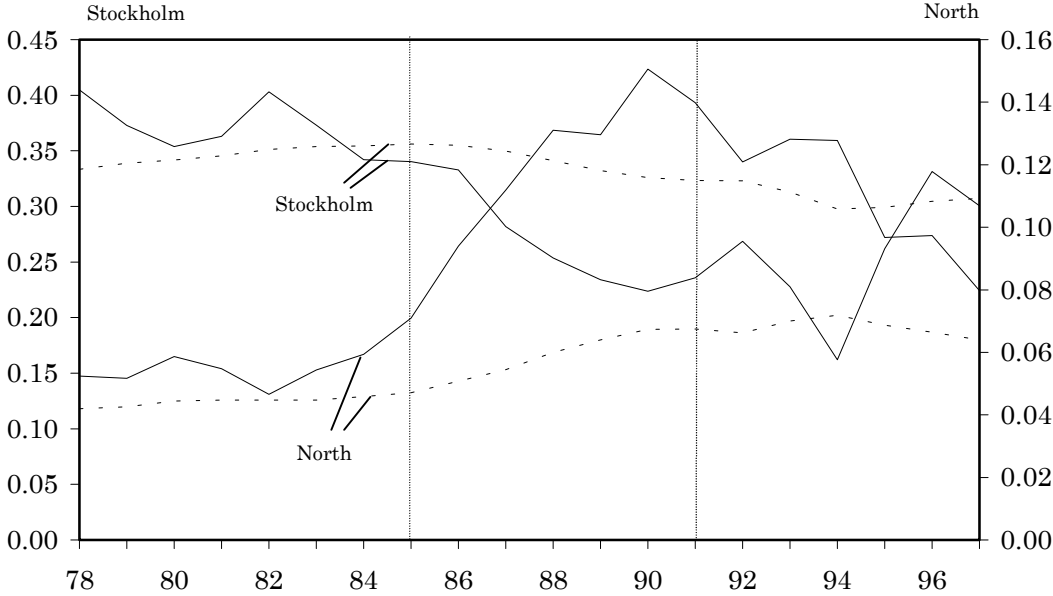
The government settlement policy had real consequences for immigrant location. This is illustrated in Figure 1, which plots the share of the immigrant inflow and stock that resides in Stockholm and the north of Sweden respectively. Prior to 1985, refugees were allowed to settle in a neighborhood of their 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

⁵ Bertrand *et al.* (2000), Dustmann and Preston (1998), and Gabriel and Rosenthal (1999) make analogous arguments.

⁶ Katz *et al.* (2000) uses a similar approach.

share residing in the north increased by 2 percentages. Thus, the policy initiative clearly increased the dispersion of immigrants across Sweden.

Figure 1: Share of non-OECD immigrant inflow (solid) and stock (dashed) located in Stockholm and in the North of Sweden respectively, 1978–1997.



Notes: “Stockholm” refers to the county of Stockholm, “North” to the six northernmost counties of Sweden. Own calculations using the LINDA immigrant sample.

Our results can briefly be summarized as follows. We find pervasive evidence of sorting across local labor markets. The coefficients on municipality characteristics, which is the level at which our measures of, e.g., enclaves pertain, differ rather drastically between estimates that account for sorting and those that do not. For example, estimates that suffer from sorting bias associate an (insignificant) earnings *loss* of 1.0 percent with a standard deviation increase in ethnic concentration. Our baseline estimates that do not suffer from this problem suggest a significant earnings *increase* of 4.2 percent.

The remainder of the paper is outlined as follows. By way of background, section 2 compares the Swedish and US experience with respect to immigration and ethnic concentration. Section 3 gives a description of the institutional setting and discusses whether we can treat the policy shift in 1985 as a natural experiment. In section 4, we outline a simple framework that we use as a guide to specification and interpretation. Sections 5 and 6 turn to the empirical analyses. We focus on two outcome measures:

earnings and idleness. We use longitudinal micro data derived from the database LINDA; see Edin and Fredriksson (2000). Section 7 concludes.

2. Immigration and clustering: Sweden and the US

The purpose of this section is to put immigration to Sweden into perspective. To accomplish this objective we compare the Swedish immigration experience during the past thirty years with that of the US. The US is a natural point of reference since it is the most extensively documented country in the literature. The Swedish figures are calculated from LINDA. The US numbers are mostly taken from Borjas (1999).

Relative to the size of each country, the immigrant stock in Sweden is greater than that of the US – a country that is sometimes referred to as “a nation of immigrants”. In 1997, 11 percent of the Swedish population was foreign-born. By comparison, 10 percent of the US population was foreign-born in 1998. The growth of the immigrant population has, however, been somewhat lower in Sweden during the past thirty years: while the immigrant share of the US more than doubled between 1970 and 1998, it grew by around 60 percent in Sweden between 1970 and 1997.⁷

The past thirty years has seen a radical shift in the ethnic composition of immigration to Sweden. In 1970, immigrants of Nordic descent constituted 60 percent of the foreign-born population; by 1997, the share of Nordic immigrants had been halved. Since the mid-1980s immigration is predominantly for political reasons.

Sweden shares the experience of a shift in the ethnic composition of immigrants with many other industrialized countries. Over two thirds of legal immigration to the US was from Europe or Canada during the 1950s. By the 1990s less than 17 percent were of European or Canadian origin.

Associated with the shift in the ethnic composition of the immigrant stock is a decline in the relative skill of the foreign-born population. Male immigrant earnings declined from 95 percent relative to male native earnings in 1970 to 88 percent in 1997.⁸ During the same time period, relative earnings of a male immigrant of Nordic descent increased

⁷ The growth rate of the immigrant to population ratio was dramatically higher during the 1960s, when it grew by 65% in a single decade.

⁸ Concomitantly, relative rates of non-participation among male immigrants increased from 1.5 times to 2.3 times the rate of native non-participation. When calculating relative immigrant earnings we applied a lower earnings limit corresponding to the minimum amount of earnings that qualifies to the earnings related part of the public pension system. In 1997, this amount was 36,300 SEK.

from 92 to 97 percent. In the US, there has been a similar decline in the relative skill of immigrants: in 1960 the average immigrant male earned 4 percent more than the corresponding native; by 1998, this had been turned into an earnings deficit of 23 percent.

As noted in the introduction, immigrants are concentrated to metropolitan areas to a larger extent than natives. In 1997, 53 percent of immigrants lived in the three largest local labor markets in Sweden (Stockholm, Göteborg, and Malmö), which host only 35 percent of the native population.⁹ By comparison, the 25 largest metropolitan areas of the US hosted 75 percent of the immigrant population and 40 percent of the native population; see LaLonde and Topel (1991). In 1998, almost three-quarters of immigrants lived in only six US states (Borjas, 2000).

It is well known that immigrants to the US tend to live in ethnic enclaves. Borjas (1998) has calculated a simple measure of the probability of residing in an “ethnic neighborhood”. An ethnic neighborhood is defined as a neighborhood where the share of the ethnic group in the resident population is at least twice as large as the share of the ethnic group in the US population. According to this measure, 48 percent of an average member of an ethnic group resided in an enclave in 1979; see Borjas (1999). Ethnic concentration seems to be particularly high among immigrants from non-industrialized countries: e.g. Mexicans, Puerto Ricans, and Cubans.¹⁰

We have calculated this measure for our sample of first generation immigrants. It turns out that 42 percent of the average first generation immigrant lives in an ethnic neighborhood in 1997. Among the top ten source countries, the probability of living in an enclave is particularly high among immigrants from Turkey, Iraq, and Iran.¹¹ Thus, ethnic concentration is a feature of the Swedish immigration experience. Further, immigrants from developing countries are more likely to reside in enclaves.¹²

⁹ The definition of a local labor market is roughly comparable to a US Standard Metropolitan Statistical Area (SMSA).

¹⁰ An ethnic group is defined in terms ethnic ancestry and, hence, many generations of immigrants. Neighborhoods are defined in terms of area zip codes. The calculations are based on the NLSY.

¹¹ The top ten source countries in 1997 (ordered by their size) were Finland, the former Yugoslavia, Iran, Norway, Poland, Denmark, (East and West) Germany, Iraq, Turkey, and the former Soviet Union.

¹² For the purpose of this calculation, neighborhoods were defined in terms of parishes. The average size of a parish approximately corresponds to the average size of a US Census tract.

3. The placement policy

The objective of this section is to give the reader a practical sense about the workings of the placement policy. For our purposes we want the actual placement to be independent of any *unobservable* characteristics in our outcome equations. To what extent is this true? Were some individuals more likely placed than others?

Unfortunately, there is very little documentation about the practical implementation of the placement policy. Therefore, we have partly approached the issues by interviewing placement officers and other officials of the Immigration Board.

We begin by giving a brief account of the institutional setting. We then proceed to describe the handling of a typical asylum seeker from the border to the final placement.

3.1 The institutional setting¹³

In 1985, handling refugee issues in Sweden became the formal responsibility of the Swedish Immigration Board, and the national government took a more important role in the handling of refugee immigrants.¹⁴ The Immigration Board assigned immigrants to a municipality of residence. Municipal authorities, in turn, assigned immigrants to an apartment. Reception in the municipalities was regulated in agreements between the Board and the municipality in question. After receiving a residence permit, the refugee was to stay in the municipality for an introductory period of about 18 months.¹⁵ This initial phase, among other things, involved introductory courses in Swedish.

Two aspects of the assignment strategy should be noted. First, the strategy only pertained to the initial location. There were no restrictions against relocating if individuals could find a place on their own. However, mobility implied the loss of eligibility for some of the special introduction activities granted in the assigned municipality. In particular, the immigrant had to await a new place in a language course. Second, not all political immigrants became enrolled in the Immigration Board's asylum reception. During 1985–91, a fifth of the inflow of political immigrants was family members who traveled directly to a municipality, where the remainder of the

¹³ This section draws primarily on The Committee on Immigration Policy (1996) and The Immigration Board (1997).

¹⁴ In practice, the Immigration Board started handling refugee issues during a trial period in the autumn of 1984.

¹⁵ The length of the introduction period appears to have varied across municipalities and years; in many cases it was considerably longer.

family resided.

The reform was a reaction to the concentration of immigrants to large cities that had taken place. 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 284 municipalities participated. It was considered a virtue if every Swedish municipality took its share of immigrants. The advantages of smaller communities in terms of closeness between people were emphasized, but the factors that initially were supposed to govern the choice of location were more or less abandoned. Instead, the availability of housing became the deciding factor.

Formally, the policy of assigning refugees to municipalities was in place from 1985 to 1994. In 1994, a new law was passed that gave immigrants the right to choose the *initial* place of residence provided that they could find an apartment on their own.¹⁶ However, the strictness of the placement policy gradually eroded during 1992–94, when there was an immigration peak caused by the war in Bosnia-Herzegovina. For our purposes, the post-1991 period is less attractive, since it contained larger degrees of freedom for the individual immigrant to choose the initial place of residence.

The strictest application of the assignment policy was between 1987 and 1991. In 1988, a new law was passed which required “extraordinary reasons” for all others than family members to get the right to stay in a municipality instead of a refugee center while waiting for a residence permit.¹⁷ In effect, it seems that the law formalized a stricter practice, which had been introduced in 1987. 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.

3.2 A typical case of asylum – the placement policy in practice 1987–91

An asylum seeker was placed in a refugee center while waiting for a decision from the

¹⁶ From then on more than 50 percent of the immigrants have used this opportunity. The Immigration Board has placed the remainder of the immigrants.

¹⁷ This was a tightening of regulations in the following sense. Prior to the change, refugees could stay in a municipality of their own choice while waiting for a residence permit and, in general, the chance of being assigned the municipality of residence was greater than being assigned another municipality.

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.

There was a long wait for a residence permit. The mean duration between entry into Sweden and the receipt of a permit (conditional on receipt) varied between 3 and 12 months during 1987–91; see Rooth (1999). Notice that whether individuals were subjected to the placement policy or not depended solely on when they received their residence permits. So an individual entering in 1986, but receiving the permit in 1987, was placed according to the practice in 1987. There was a much shorter wait for a municipal placement after receiving the permit, partly because placement officers had explicit goals in terms of the duration of this spell.

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 Stockholm, Göteborg and Malmö. There were very few apartment vacancies in these locations, however, 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 observable characteristics; language, formal qualifications, and family size seem to have been the governing criteria. When the municipalities could “cream skim”, they selected 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.

After having been assigned to an apartment, immigrants’ main source of income was welfare (i.e. social assistance). They could live on welfare while participating in introductory Swedish courses. Receipt of welfare was not conditional on residing in the assigned municipality and the central government reimbursed the local governments for their welfare expenditures. So there was little incentive for the immigrants to stay on in an assigned municipality, if they could realize their preferred choice. The main individual cost, apart from moving costs, consisted of delayed enrollment in introductory language courses.

3.3 The placement policy as a natural experiment

On the basis of the above description, we think that it is realistic to treat the municipal assignment as exogenous with respect to the random components of the outcomes of interest, *conditional on observed characteristics*. For highly qualified individuals this assumption is potentially more problematic. Cream skimming on the part of municipal officers suggests that the high-skilled may have been able to realize their preferred option.¹⁸

The strictest application of the assignment policy was between 1987 and 1991. We have chosen to base our empirical work on placements during the 1987–89 period. The last year is an obvious choice, since the probability of being “exogenously” placed is increasing in the total inflow of residence permits and the tightness of the housing market. There was a hike in the number of new residence permits in 1989, and the housing market peaked in that year. Given the choice of 1989 we can follow individuals for a maximum of eight years. To increase the size of the sample we added two additional years. We chose 1987 and 1988 since we wanted to follow individuals over time for as long as possible.¹⁹

4. A stylized framework

The purpose of this section is to present a simple model, which we think is a useful framework of thought. Although we examine different outcomes in our empirical analysis we focus here on the simultaneous determination of location and earnings. We begin by examining the bias of the OLS estimator. We then present the conditions that render different estimation approaches consistent. Throughout we keep the conditioning on observed characteristics (apart from location) implicit. To illustrate our main points we adapt the schooling model of Card (1999) to our setting.

4.1 The bias of ordinary least squares

Consider an immigrant who derives utility from living with other immigrants and the

¹⁸ Below, we provide some evidence on this issue. On the whole, rates of post-placement mobility do not suggest that the highly qualified were more likely to exercise their preferred option when being assigned to a municipality.

¹⁹ As a guide to the selection of years we calculated the ratio of the inflow of residence permits and the stock of vacant public rental apartments. This ratio stood at 10 in 1989; in 1988 and 1990, it equaled 4, and in 1987 and 1991, 2.

consumption of goods. The immigrant maximizes utility by making a location choice, where each location is characterized by some measure of immigrant density (m). For simplicity we assume that there is a continuum of location choice where m spans the real line between zero and unity: $m \in [0,1]$. Utility is given by

$$U_i = u_i(m_i) + \ln y_i \quad (1)$$

The objective of the individual is to maximize (1) subject to a market opportunity locus:

$$\ln y_i = \alpha_i + \beta_i m_i \quad (2)$$

where α_i reflects general aptitudes in the labor market and β_i is the marginal return of living in an enclave – or the aptitude for enclaving. The first-order condition is $u'_i + \beta_i = 0$. To proceed, assume that the marginal utility of living in an enclave is linear in m , i.e., $u'_i = \mu_i - km$, where k is a positive constant. With this assumption we have that the optimal choice of m satisfies²⁰

$$m_i^* = \frac{\mu_i + \beta_i}{k} \quad (3)$$

Now, let us revert to the earnings equation. Rewrite this relationship as follows:

$$\ln y_i = \alpha + \beta m_i + (\alpha_i - \alpha) + (\beta_i - \beta)m_i = \alpha + \beta m_i + \eta_i \quad (4)$$

Our interest concerns the parameter β – the average return to living in an enclave. (Throughout we choose the convention that non-indexed variables are population averages.) Consider the OLS estimate of equation (4). The probability limit of the OLS estimate, b_{OLS} , is

$$\text{plim } b_{OLS} = \beta + \lambda_0 + \lambda_1 m \quad (5)$$

The parameters λ_j are theoretical regression coefficients: $\lambda_0 = \text{cov}(\alpha_i, m_i)/\text{var}(m_i)$ and

²⁰ In order to avoid digressing into details we do not discuss the possibility of corner solutions.

$\lambda_1 = \text{cov}(\beta_i, m_i) / \text{var}(m_i)$, where m_i is given by (3). Assuming that the underlying random variables, α_i , β_i , and μ_i , have some joint and symmetric distribution, the explicit expressions for λ_j are²¹

$$\lambda_0 = k \frac{\sigma_{\alpha\mu} + \sigma_{\alpha\beta}}{\sigma_{\mu}^2 + \sigma_{\beta}^2 + 2\sigma_{\beta\mu}} \quad (6)$$

$$\lambda_1 = k \frac{\sigma_{\beta}^2 + \sigma_{\beta\mu}}{\sigma_{\mu}^2 + \sigma_{\beta}^2 + 2\sigma_{\beta\mu}}$$

The coefficient λ_0 signifies ability bias, while λ_1 is related to bias because of self-selection.

In general, we cannot say much about the sign of the bias, but it is instructive to walk through some special cases. We will consider four such cases: (a) homogeneity in the return to enclaving $\beta_i = \beta$; (b) location decisions are based on expectations of income but *ex post* returns differ across individuals, i.e. $\beta_i = \beta$ in (3); (c) preference homogeneity $\mu_i = \mu$; and (d) preferences are independent of aptitudes $\sigma_{\beta\mu} = \sigma_{\alpha\mu} = 0$.

(a) $\beta_i = \beta$. The bias of OLS then equals

$$bias = k \frac{\sigma_{\alpha\mu}}{\sigma_{\mu}^2} \quad (7a)$$

The sign of the bias depends on the correlation between general aptitudes and preferences. Absent any such correlation OLS is unbiased; random variation of preferences then effectively trace out the structural earnings equation (2). In general, of course, $\text{sign}\{bias\} = \text{sign}\{\sigma_{\alpha\mu}\}$, but it is difficult to have a concrete prior about $\sigma_{\alpha\mu}$.

(b) *Ex ante* homogeneity but *ex post* heterogeneity in β_i . This case is similar to (a) in the sense that the bias of OLS is a function of the statistical dependence between preferences and the returns in the labor market:

$$bias = \frac{k}{\sigma_{\mu}^2} [\sigma_{\alpha\mu} + \sigma_{\beta\mu} m] \quad (7b)$$

²¹ Symmetry is imposed since then we do not have to worry about moments of the third order.

(c) $\mu_i = \mu$. The bias of OLS equals

$$bias = k \left[m + \rho \frac{\sigma_\alpha}{\sigma_\beta} \right] \quad (7c)$$

where ρ is the correlation between ability and the monetary pay-off of living in an enclave. If there is no such correlation OLS is biased upwards; those who gain most from living in enclaves choose to do so; the bias is thus purely due to self-selection. However, we believe that $\rho < 0$ is a realistic assumption, i.e., those who do poorly on the labor market for unobservable reasons are those who with the highest return to enclaving. One argument for this belief would be that network effects are more important for individuals of low general aptitude. If $\rho < 0$, OLS may plausibly be biased downwards.

(d) $\sigma_{\beta\mu} = \sigma_{\alpha\mu} = 0$. In this case the bias expression is very similar to (7c):

$$bias = k \frac{\sigma_\beta^2}{\sigma_\beta^2 + \sigma_\mu^2} \left[m + \rho \frac{\sigma_\alpha}{\sigma_\beta} \right] \quad (7d)$$

i.e., the bias is proportional to, but lower than in, case (c).

Suppose that we can obtain a consistent estimate of β . Can we say anything useful about the sign of the unknown covariances? If $\sigma_\beta^2 > (-\sigma_{\beta\mu})$, the bias due to self-selection (λ_1) is positive. Since this seems like an innocuous assumption, we will impose it. As it turns out, there is one informative and one uninformative case. The informative case is when OLS is downward biased, since then $\text{sign}\{\lambda_0\} = \text{sign}\{\sigma_{\alpha\mu} + \sigma_{\alpha\beta}\} < 0$. If $\sigma_{\alpha\beta} = 0$, this implies that those of less general aptitude derive greater utility from enclaving. If $\sigma_{\alpha\mu} = 0$, low-ability individuals have a greater return to living in an enclave.

4.2 On the estimation of the outcome equations

What kind of assumptions do we need in order to estimate the average return to living in an enclave consistently? In practice, we have two estimation alternatives – an IV or a control function approach. For our instrument to be of any use it is clear that it must be independent of the random coefficients in (4). Let us denote our instrument, the initial

placement, by m_i^0 . Then we assume that

$$E[(\alpha_i - \alpha)m_i^0] = E[(\beta_i - \beta)m_i^0] = 0 \quad (8)$$

We also impose an exclusion restriction in the sense that only variables associated with the current location have an effect on earnings.

Given the above assumptions, two questions arise: What does (linear) instrumental variables estimate? Under what conditions does IV estimate the average return to living in an enclave consistently? These questions have given rise to a scholarly debate; the essence of the debate can be found in Angrist *et al.* (1996) and Heckman (1997).

An application of IV to a setting that is similar to ours is given in Angrist (1990). He used the Vietnam draft lottery to estimate the effect of military service on earnings. An IV estimate of the effect of military service on earnings then is a weighted average of individual “treatment effects” for those whose military service status was changed because of the value of instrument – i.e. for those who were “compliers”.²² Notice that: (i) individuals who would have enrolled in the military irrespective of their lottery number do not contribute to identification; (ii) the interpretation of the IV estimand does not hinge on linearity of the relationships of interest; and (iii) the group of compliers need not be representative of the population.

The fact that compliers need not be representative of the population is the basis for Heckman’s criticism of (linear) IV estimation of models with variable treatment effects. He argues that being a complier involves a choice, which partly may be based on the unobservable earnings gains of going into the military. If this is the case, the average causal response among compliers is not representative of the average treatment effect (ATE) in the population. A control function approach, on the other hand, provides consistent estimates of ATE even if there is selection on unobservable earnings gains, subject to imposing some additional structure.

Let us provide more substance to our informal discussion. Given (8), the consistency of IV requires $E[(\beta_i - \beta)m_i^0] = \kappa$, where κ is constant and independent of m_i^0 ; see Wooldridge (1997). Let $m_i = s_i m_i^0 + (1 - s_i) m_i^*$, where $s_i = 1$ if the individual stayed on in

²² For this weighted average to be well-defined a monotonicity assumption is required. Monotonicity means that getting a lottery number that implied eligibility for draft should not decrease the probability of serving in the military and vice versa. If monotonicity holds, the weights sum to unity.

the assigned location, $s_i = 0$ for the complementary event, and m^* is given by (3). Then IV requires that

$$E[(\beta_i - \beta)m_i|m_i^0] = E[(\beta_i - \beta)m_i|m_i^0, s_i = 1]\Pr(s_i = 1|m_i^0) + E[(\beta_i - \beta)m_i|m_i^0, s_i = 0]\Pr(s_i = 0|m_i^0) = \kappa \quad (9)$$

We first note that the probability of staying will generally depend on m_i^0 .²³ Hence, IV needs that the conditional means of (9) are either equal to zero, or that the two terms in (9) are equal in magnitude but of opposite sign. The conditional means will equal zero if there is no sorting on comparative advantage (β_i) or information correlated with it. In terms of the special cases considered in section 4.1, IV estimates the average treatment effect if the return to enclaving is homogeneous in the population (case a), and when there is *ex ante* homogeneity but *ex post* heterogeneity in β (case b), provided that $\sigma_{\beta\mu} = 0$. However, as soon as there is self-selection by comparative advantage (cases c) and d) IV estimation is problematic and requires special assumptions in order to be consistent.

So where does this leave us? Should we use an IV or a control function approach? Our strategy is to use both methods. The advantage of linear IV is its robustness; it gives a weighted average of treatment effects for compliers. Without further restrictions this is all the data can be informative about. If we are willing to impose some additional structure (such as linearity in the outcome equation and a specific error structure) we can apply selection correction methods to estimate ATE. Subject to this additional structure, the implicit behavioral (or informational) assumption of IV is, in principle, testable.²⁴

5. Consequences of the reform: mobility and concentration

In this section we give a quantitative picture of how the policy reform affected subsequent mobility and location patterns. Subsequent mobility is a natural indicator of

²³ The indicator variable s equals $s_i = I\{U_i(m_i^0)(m_i - m_i^0) \leq c_i\}$, where I is the indicator function and c_i denotes the cost of mobility. Using (3) this equals $s_i = I\{R(m_i - m_i^0) \leq c_i\}$. There are two cases where the probability of staying does not depend on the assigned municipality. One is when there are no mobility costs. In this instance, however, the rank condition fails, given our maintained assumption that the initial placement is exogenous to unobserved individual characteristics. The second example is if mobility costs are given by $c_i = o_i(m_i - m_i^0)$ and so $s_i = I\{R \leq o_i\}$.

²⁴ Heckman and Robb (1985) suggest a Hausmann test.

how immigrants perceived their initial placement. We also want to make sure that the policy initiative actually affected residential location in the longer run. If not, the initial placement will not have much predictive value for current region of residence.

5.1 Data and sample selection

The empirical analysis is based on data from the LINDA database. Among other things, LINDA contains a panel of around 20 percent of the foreign-born population. Moreover, the data are cross-sectionally representative. Data are available from 1960 and onwards, and are based on a combination of income tax registers, population censuses and other sources; for more details, see Edin and Fredriksson (2000).

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. Furthermore, persons belonging to a household with either a Swedish-born grown-up or a previous immigrant were excluded, since these individuals were likely to have immigrated as family members and, consequently, are not “program participants”. We also apply an age restriction and base our analysis on individuals aged 18–55 at the time of entry into Sweden. Lastly, we focus on the immigration waves during 1987–89 for reasons outlined above.

Another feature of the data that is relevant for our analysis is that we observe individuals’ region of residence at the end of the year. Thus, the observed initial location may differ from the actual initial placement when individuals move during their first year. This introduces a measurement error in initial placement, an issue we will return to when assessing the stability of our estimates.

5.2 Consequences of the reform

In order to give a quantitative view of how the placement policy affected the location pattern of recent immigrants it is necessary to construct a counterfactual. For this purpose, we choose individuals who are identified as refugee immigrants (according to the above criteria) during the years 1981–83. We use these two samples of immigrants,

1981–83 and 1987–89, to illustrate differences in initial and subsequent location patterns and secondary mobility.

Since we want to use the 1981/83 cohort as an approximation of the counterfactual for the 1987/89 cohort, it is vital that the cohorts are similar in terms of observable and unobservable characteristics. With respect to observable characteristics, there were no important differences in terms of age and education. The representative individual of the 1987/89 cohort was 0.6 years older and had 0.2 years more of imputed years of education.

The difference between the two cohorts in terms of ethnicity is a greater source of concern. It is well known that ethnicity is an important determinant of success in the receiving country; ethnicity is important as it influences language skills and the level of formal education varies by origin country (Borjas, 1994). The chief discrepancy between the two cohorts is that the 1981/83 cohort has more of the mass among immigrants from Eastern Europe. The later cohort, by contrast, has the greatest fraction of immigrants originating from the Middle East.²⁵

To us the differences in terms of region of origin seem substantial. To generate the counterfactual location distribution for the 1987/89 cohort, we reweigh observations in the 1981/83 cohort such that the distribution over region of origin conforms to the 1987/89 cohort. Whenever we talk about differences across cohorts in the sequel, we refer to the differences between the 1987/89 cohort and the weighted 1981/83 cohort.

One indicator of how immigrants perceived the reform is post-immigration mobility. If a consequence of the government policy was that immigrants were placed in regions that they deemed inferior, we should observe greater mobility in the 1987/89 cohort in comparison to the earlier cohort. The prediction that mobility should be greater in the program cohort is clearly contingent on immigrants being able to choose/identify their most preferred region upon arrival. There are plausible reasons why this might not be the case. For one thing, there is probably genuine uncertainty about the regional variation in the pay-off to labor market skills and, hence, the answer to this question is

²⁵ The share of refugee immigrants from Eastern Europe declined from 37 percent (1981/83) to 18 percent (1987/89), while the share from the Middle East increased from 23 to 46 percent. The increase in refugee immigration from the Middle East is mainly due to the war between Iran and Iraq. The large share of Eastern Europeans in the earlier cohort is due to a substantial inflow of immigrants from Poland in 1982, following the Solidarity upheavals.

not obvious.

We start by comparing mobility across the two cohorts; see Table 1. We note that, mobility is substantial in both cohorts. Around 50 percent chose to move from their initial location. Moreover, there are some differences between the cohorts.²⁶ The probability of remaining in the initial location is lower among those who were assigned a municipality by government authorities; the propensities to emigrate are roughly equal, but there is more internal mobility in the 1987/89 cohort.

Table 1: Individuals who stayed, emigrated, and relocated, percent.

	Immigrant cohort	
	1981/83	1987/89
	t and $t+8$	t and $t+8$
Stayed	51.2	46.5
Emigrated	13.8	13.6
Relocated	35.0	39.9

Notes: Refugee immigrants aged 18–55 at immigration. Probability of emigration equals probability of not being in sample (i.e. the figures include deceased). t denotes year of immigration. Observations in the 1981/83 cohort weighted to correspond to the (period t) region-of-origin distribution in the 1987/89 cohort.

Table 2: Location patterns by population density, percent.

	Immigrant cohort			
	1981/83		1987/89	
	t	$t+8$	t	$t+8$
Region 1 (Stockholm)	48.0	52.3	25.0	33.6
Region 2 (Göteborg & Malmö)	15.3	18.0	16.2	25.6
Region 3	29.2	24.3	31.4	29.5
Region 4	6.4	4.1	17.7	8.5
Region 5	0.8	0.9	3.4	1.7
Region 6 (Sparsely populated)	0.3	0.4	6.3	1.1

Notes: Refugee immigrants aged 18–55 at immigration. “Region 1” most densely populated; “Region 6” least densely populated. t denotes year of immigration. Observations in the 1981/83 cohort weighted to correspond to the region-of-origin distribution in the 1987/89 cohort.

Thus, post-immigration mobility seems to be high; this is true for both cohorts. To what kinds of regions did the immigrants move? We investigate this question in Table 2. As we have noted, the policy reform was a reaction to the concentration of the foreign-born to metropolitan areas, primarily Stockholm, Göteborg, and Malmö. As a consequence of the reform, we should expect a shift in the initial location pattern in favor of sparsely populated areas, often located in the northern part of Sweden.

²⁶ To examine whether these numbers are driven by an overall increase in the probability of relocation, we have also calculated difference-in-difference estimates relative to a sample of natives. The difference between the cohorts is moderated *slightly*. Given the similarity between the before-and-after and difference-in-difference estimates, we report the former in Tables 1 and 2.

Table 2, which tabulates region of residence according to population density, shows that the distribution of initial location across the two cohorts is radically different. There is concentration over time in both cohorts, although much more pronounced in the 1987/89 cohort. Nevertheless, there is far from total convergence of the two distributions. Thus, it seems that the reform did have lasting effects on location. A tabulation of municipalities by geographical location conveys a similar message, with one proviso: mobility is not just from a desolated North to the populous South, but also to the regional centers in the north of Sweden.

So, in line with our expectations, there is more mobility and concentration among immigrants who were assigned a municipality by government authorities.²⁷ Nonetheless, two additional facts are striking to us. First, there is also a lot of mobility in the cohort that was supposedly free to choose, suggesting that informational problems may be of some importance. Second, eight years after entry to Sweden, the post-reform distribution of immigrants has far from converged to the pre-reform distribution of immigrants.

An important question is whether some groups were less likely to be placed than other groups. Our account of the workings of the assignment policy suggests that less qualified individuals may have been able to realize their preferred choice to a lesser extent than the highly skilled. An analogous argument holds for singles.

One natural avenue to examine these hypotheses is to look at migration propensities by education and family status. If the above hypotheses are correct we should expect greater mobility among the less educated than among the highly educated as a consequence of the reform, and likewise for singles in comparison to couples. With respect to education we find no evidence in favor of the hypothesis – on the contrary: difference-in-difference estimates suggest that the reform increased the relative relocation rates of individuals with a university degree. Moreover, we found no differences according to family or marital status.

²⁷ In related work we have examined subsequent mobility in closer detail. It turns out that much of the raw difference in mobility between the two cohorts disappears when it is standardized with respect to individual characteristics; see Åslund (2000).

6. The effects of living in enclaves

In this section we estimate the effect that living in ethnic enclaves has on economic outcomes. We begin by offering a brief account of the literature on why enclaves could affect outcomes. Then we present a set of baseline estimates that deliver the gist of the results. Finally, we subject the baseline estimates to a comprehensive set of specification checks. We wish to emphasize from the start that the conclusion from these checks is that, if anything, the baseline estimates understate the value of enclaves.

6.1 Why does living in enclaves affect outcomes?

The purpose of this section is to give a brief review of the literature on why enclaves influence the outcomes of individuals living there. We consider four types of explanations: (i) slower rate of acquisition of host country skills; (ii) “network effects”; (iii) “spatial mismatch”; and (iv) human capital externalities.²⁸ Although we present each explanation separately, they are not mutually exclusive.

The hypothesis that the enclave *decreases the rate of host country skill acquisition* seems to have been among the prime motives for the reform that we are utilizing. According to this view, the ethnic enclave provides less interaction with natives and reduces the incentives for acquiring, e.g., the language skills that are necessary to succeed in the national labor market. Thus, the enclave hinders the move to better jobs and reduces earnings in the longer run.

More of a positive view is contained in stories that emphasize *network effects*. The enclave represents a network that increases the opportunities for gainful trade in the labor market; e.g. Portes (1987) and Lazear (1999). Further, the network disseminates valuable information on, e.g., job opportunities, and constitutes an environment where the immigrant is less exposed to the discrimination encountered elsewhere on the labor market. The enclave would thus improve labor market outcomes, in particular for recent immigrants and for individuals who have difficulty integrating into the labor market. Of course, the enclave may also provide information on matters that are not conducive to

²⁸ To this list one could potentially add relative factor supplies and compensating differentials. This story would go as follows. If the typical immigrant has preferences for living with members of his own ethnic group, then he is willing to pay a price for living in that area. The price corresponds to the movement along the labor demand curve as labor supply increases. The equilibrium sorting of individuals will feature a negative correlation between wages and ethnic concentration. The correlation is simply due to preferences and does not have a causal interpretation. For this reason, this story is not included above.

success in the labor market, such as welfare eligibility; e.g., Bertrand *et al.* (2000).

The *spatial mismatch* hypothesis emphasizes discrimination in the housing market; see Ihlanfeldt and Sjoquist (1998). Since immigrants face restrictions in the housing market they are *forced* to segregate in an enclave. The enclave, in turn, may be distant from areas that provide employment opportunities. Therefore, individuals living in the enclave will fare worse than otherwise similar immigrants who have managed to escape housing market discrimination. In this view, it is not the enclave as such that hampers success in the labor market, but rather that the enclave is located away from employment opportunities.

The stories based on *human capital externalities* are also based on residential segregation. In this instance, however, segregation is not necessarily bad – it all depends on the quality of the enclave, e.g. the stock of human capital; see Cutler and Glaeser (1997) and Borjas (1998). If residential segregation implies that skilled members of an ethnic group live in the enclave, and individuals primarily interact with members of their own ethnic group, then disadvantaged members such as recent immigrants gain from living in the enclave.

The conclusion from this brief review is that the causal effect of living in an enclave is ambiguous in sign. The net effect on outcomes is thus an empirical question. To determine the net effect, we estimate what must be interpreted as reduced form relationships between measures of labor market outcomes and, among other things, the size of the enclave. We will thus not be able to test any of the above hypotheses. However, we argue that our estimates have a causal interpretation.

6.2 Baseline estimates

In this section we provide estimates of the effects of segregation across municipalities in a cohort of recent immigrants. We investigate to what extent the share of immigrants (foreign citizens) in a municipality, and the ethnicity of these immigrants, matter for the economic outcome of recent immigrants. Since immigrants can choose municipality of residence, municipal variables cannot be assumed exogenous. Therefore, we will make use of the settlement policy introduced in 1985 to obtain instruments for municipal variables. In effect, we use variables pertaining to the initial (assigned) municipality as instruments for municipal variables eight years later. Our maintained presumption

throughout is that the placement policy is independent of unobserved individual characteristics. Moreover, we assume that location does not have permanent effects on outcomes.

We employ the following baseline specification

$$outcome_{ij(t+8)}^k = \bar{\mathbf{a}}' \mathbf{X}_{i(t+8)} + \beta_e \ln e_{j(t+8)}^k + \beta_m \ln m_{j(t+8)}^k + \beta_n \ln n_{j(t+8)} + \delta u_{j(t+8)} + \varepsilon_{ij(t+8)}^k \quad (10)$$

We focus on two outcomes – log earnings and idleness – and standardize for a set of individual characteristics \mathbf{X} , containing gender, age, age squared, marital status, education, ethnicity, and year of immigration. The outcome for individual i of ethnic group k is related to four municipal variables (municipalities are indexed by j): $e_{j(t+8)}^k$ denotes the size of the ethnic group; $m_{j(t+8)}^k = \sum_{\ell \neq k} e_{j(t+8)}^\ell$ the number of immigrants of other ethnicities than k ; $n_{j(t+8)}$ the size of the native (i.e. Swedish-born) population; and $u_{j(t+8)}$ the municipal unemployment rate (percent of the population aged 16–64). We introduce the levels of the population variables in logs, since this provides a more flexible specification than the perhaps more standard approach of using population shares.²⁹

We are primarily interested in the effect of changes in the *composition* of the municipal population, i.e. changes in $e_{j(t+8)}^k$ and $m_{j(t+8)}^k$ holding population ($p_j \equiv e_j^k + m_j^k + n_j$) constant. For instance, the elasticity of earnings (y) with respect to ethnic and immigrant concentration, respectively, equals

$$\eta^e \equiv \left. \frac{d \ln y}{d \ln e} \right|_{d \ln p=0} = \hat{\beta}_e - \hat{\beta}_n \frac{e}{n} \quad (11a)$$

$$\eta^m \equiv \left. \frac{d \ln y}{d \ln m} \right|_{d \ln p=0} = \hat{\beta}_m - \hat{\beta}_n \frac{m}{n} \quad (11b)$$

and analogously for the probability of being idle.³⁰ For all practical purposes, the adjustment in (11a) is immaterial, since (e/n) is such a small number (0.004 on average). The adjustment in (11b) is material, however, because m/n equals 0.087 on

²⁹ In our empirical analysis, the specification in terms of levels fit the data better than the specification in shares. Also, the qualitative results are similar in both specifications.

³⁰ If we want to get at the latter elasticity we need to divide by the corresponding average probability.

average.³¹

Motivated by the discussion in section 4, we estimate (10) with a variety of estimation methods. As we have explained, these methods differ with respect to the underlying assumptions about the sorting process. To conserve space, we just report estimates of local unemployment and the elasticities defined in (11a) and (11b) here. The full set of estimates for OLS and IV are available in the appendix; see Table A2. Hence, we will not comment on the coefficients on individual characteristics. Let us just note that they are very similar across specifications.

Earnings

Table 3 reports the results of the basic specification for earnings. The outcome of interest is defined conditional on having positive earnings. As we go along in Table 3, we add more sophistication but also more structure. Column (1) reports OLS estimates where we treat the four local variables as exogenous. Column (2) gives the results of the IV (2SLS) procedure, which uses the local variables for each immigrant's initial placement (in year t) as instruments for current (year $t+8$) local conditions.³² In column (3), we present the results from a control function approach due to Garen (1984). The value of this approach relative to IV is that it allows $E[(\beta_i - \beta)m_i | m_i^0]$ to depend on m_i^0 .³³ Column (4), finally, presents the result of the Heckman (1979) two-step estimator; we choose the two-step estimator, rather than the FIML estimator, since the former is more robust to departures from bivariate normality.

According to the OLS estimates we are led to believe that ethnic and immigrant concentration do not matter for earnings. Unemployment is the only local variable that is of importance (in the statistical sense) for earnings. A standard deviation increase in ethnic concentration is associated with an (insignificant) earnings gain of 1.4 percent.³⁴ The IV estimates, however, imply that these conclusions are premature, since the OLS estimates on ethnic and immigrant concentration are downward biased by a factor of

³¹ Summary statistics for the municipal variables and the individual characteristics of the sample are reported in Table A1.

³² Estimates where municipal house prices were used as an additional instrument are almost identical to those reported here.

³³ It adds the assumption that the conditional expectations of the individual specific error terms ($\alpha_i - \alpha$ and $\rho_i - \rho$) can be written as linear functions of the local variables in $t + 8$ and t ; see Card (1999).

³⁴ The standard deviation is calculated within ethnic groups.

three to four.³⁵ Interestingly, the precision of ethnic concentration is not much affected by the IV-procedure. In this instance, a standard deviation rise in ethnic concentration produces a significant earnings increase of 4.2 percent.

Table 3: Basic earnings estimates, standard errors in parentheses.

	OLS	IV (2SLS)	Control function Garen (1984)	Control function Heckman (1979) two-step	
	(1)	(2)	(3)	(4a) Stayers	(4b) Movers
Ethnic concentration (η^e)	.020 (.013)	.058 (.016)	.056 (.017)	.054 (.023)	-.014 (.034)
Immigrant concentration (η^m)	.037 (.043)	.149 (.134)	.121 (.110)	.162 (.088)	.084 (.074)
Local unemployment	-.083 (.018)	-.140 (.047)	-.136 (.053)	-.072 (.022)	-.129 (.029)
# individuals	6,418	6,418	6,418	3,492	2,926

Notes: The estimates on ethnic and immigrant concentration are calculated according to equation (11). The coefficient on local unemployment is the percent change in earnings associated with a percentage point increase in unemployment. In col. (2) period t values in the assigned municipalities are used as instruments for ($t+8$) local variables. In col. (3) we add the residuals from first-stage regression of each of the endogenous right-hand side variables on all exogenous characteristics of the municipality and individual, together with interaction terms between the residuals and the endogenous local variables. The first step in the Heckman two-step procedure includes period t values for the assigned municipalities and individual characteristics. Variance estimates allow for correlation across individuals residing in the same municipality. The standard errors in columns (3) and (4) are not corrected for the first stage estimation.

The estimates on ethnic and immigrant concentration are more or less invariant with respect to variations in the estimation method. The Garen (col. 3) and the Heckman approach (col. 4a) both produce estimates that are very similar to the IV estimates. The only difference pertains to the effect of unemployment, where the Heckman procedure yields decidedly lower point estimates. Thus, for the variables of primary interest, the fact that stayers – who constitute the prime source of identification in the IV approach – may be a selected group does not seem to be a problem.

A comparison between the OLS and IV estimates does not yield a correct picture of the size of the sorting bias. This is so because some (the majority of those still in the country) of our individuals remained in the locations in which they were originally placed. An alternative is to compare movers (col. 4b) with any of the estimates that are consistent. The estimates for movers suffer from sorting bias since there are no instruments for their present location; however, we can correct for the fact that they

³⁵ A Hausman test of the exogeneity of the local variables decisively rejects exogeneity. The test statistic is $F(4, 249) = 7.47$ (degrees of freedom within parentheses), with a p-value of 0.000.

may be a selected group using the location assigned to them. The comparison between movers and, e.g., the IV estimates suggests that the bias due to sorting is especially acute for the estimate on ethnic concentration. According to the estimate in col. (4b) a standard deviation increase in ethnic concentration *decreases* earnings by 1.0 percent.³⁶

A word of caution should accompany the control function estimates in column (4) since they rely on functional form to a great deal. Although our equations are identified without the non-linear transformation of the inverse Mill's ratio, some experimentation with a linear selection term (Olsen, 1980) results in imprecise estimates of the coefficients on the local variables, which suggests that "true" identification is weak.³⁷ Therefore, we drop the selection correction approach from here on and resort to the simpler control function approach of Garen, which seems to be less demanding of the data in this setting.

Throughout the analysis we have assumed that the initial location does not affect current income. An important issue is whether this exclusion restriction is valid. To examine this issue we have performed Sargan tests of overidentifying restrictions on slight variations of our basic model. Since our baseline specification is exactly identified we cannot apply the Sargan procedure directly. Therefore, we amended the instrument set by including the lags of the original instruments. In general, the parameters of interest were not affected by altering the set of instruments. Furthermore, the Sargan-statistics do not reject the estimated models. This result lends support to the identifying assumption that initial location does not have a permanent effect on earnings.

To sum up this subsection, we have two major findings. First, there is pervasive evidence that estimates of "neighborhood" effects that do not account for sorting may be severely biased.³⁸ Our results indicate that high (unobservable) ability immigrants locate outside ethnic enclaves to a greater extent. Second, and perhaps more importantly, immigrants derive a (statistically significant) positive return from living in ethnic enclaves. There is an earnings gain of four percent associated with a standard deviation increase in ethnic concentration. We interpret this result as being consistent

³⁶ A simple OLS regression for movers yields a slightly greater earnings reduction.

³⁷ We have tried adding local variables as well as interactions between individual characteristics and local variables to the selection equation with essentially the same result.

³⁸ In this sense, our results are similar to Evans *et al.* (1992), where the neighborhood referred to schools. Our estimates suggest that sorting is pervasive even when using a more extensive measure of a neighborhood.

with hypotheses stressing that ethnic enclaves may be associated with, e.g., positive network effects.

Idleness

Let us proceed to our other outcome measure: idleness. We define those who had no earnings and did not participate in regular education as “idle”.³⁹ Table 4 reports the results for linear probability models of idleness. Again, we report the results of different estimation approaches; this time, however, we drop the selection correction method for reasons given above.

Table 4: Idleness, standard errors in parentheses.

	OLS	IV (2SLS)	Control function Garen (1984)
	(1)	(2)	(3)
Ethnic concentration (η^e)	-.019 (.009)	-.057 (.014)	-.054 (.014)
Immigrant concentration (η^m)	.113 (.045)	.091 (.111)	.115 (.100)
Local unemployment	.020 (.004)	.032 (.010)	.028 (.010)
# individuals	9,883	9,883	9,883

Notes: The coefficient on local unemployment is the change in the probability associated with a percentage point increase in unemployment. The remaining coefficients are elasticities, defined analogously to equation (11), i.e., they give the percent change in the probability associated with a one percent change in each respective variable. In col. (2) period t values in the assigned municipalities are used as instruments for ($t+8$) local variables. In col. (3) we add the residuals from first-stage regression of each of the endogenous right-hand side variables on all exogenous characteristics of the municipality and individual, together with interaction terms between the residuals and the endogenous local variables. Variance estimates allow for correlation across individuals residing in the same municipality. The standard errors in col. (3) are not corrected for first stage estimation.

A similar pattern arises for idleness as for earnings. The OLS estimate of the effect of living in ethnic enclaves is biased downward by a factor of three according to the IV and the control function approach. The point estimate implies that a standard deviation increase in ethnic concentration reduces idleness by 4.1 percent (1.2 percentage points). The sorting pattern appears to be analogous to the earnings equation.⁴⁰ Again, the results suggest that individuals with high (unobservable) ability choose locales with less ethnic concentration. This conclusion is reinforced by the fact that a simple OLS

³⁹ Regular education does not include labor market training programs.

⁴⁰ The variation in idleness is mostly driven by variations in employment. Since employment is increasing in ethnic concentration, the fact that we are conditioning on employment in the earnings regression does not cause great concern.

regression for movers suggests that a standard deviation increase in the size of the ethnic enclave is associated with an (insignificant) rise in idleness of 0.6 percent.

6.3 Are the basic earnings estimates stable?

The baseline earnings estimates presented above are associated with some potential problems related to model specification and particular features of the data. We have two main issues in mind. First, the models are estimated using a parsimonious set of local variables. It may be the case that the effect of ethnic concentration just stems from some omitted local variable correlated with ethnic concentration. Second, we do not observe actual placements of refugees. For example, we know that only 90 percent of the refugee immigrants were actually subjected to the placement policy. Furthermore, if refugees move within the year of receiving residence permits, we do not observe their municipality of placement (since we only observe residence at the end of the year). Thus, our instruments are not valid for all observations in our sample.

In Table 5 we assess the stability of the key parameter by various ways of accounting for these problems. We start by investigating whether the effect of ethnic concentration is driven by omitted local characteristics. First, we increased the set of local variables by including a proxy for local prices (house prices) and the share of employment in manufacturing; see row (2). This permutation did not have much effect on the estimates; moreover, the added local variables did not enter significantly in the estimated relationship. Second, we added the mean earnings of non-OECD immigrants with similar results (row (3)). Third, we included a set of (23) county dummies to capture other omitted regional characteristics (row (4)). The results were almost identical to the baseline estimates. This set of additional estimates makes us confident that it really is ethnic concentration that drives our results.

Table 5: Stability of the elasticity of ethnic concentration.

Variation	Elasticity	Standard error
(1) Baseline estimate	.058	(.016)
<u>Alternating the set of controls</u>		
(2) Add house prices and industry structure	.055	(.016)
(3) Add mean earnings in municipality, non-OECD immigrants	.057	(.016)
(4) Add county dummies	.057	(.017)
(5) Delete schooling variable	.046	(.017)
<u>Weighting (to conform to municipal distribution of other data)</u>		
(6) Immigration Board, municipal placement	.083	(.022)
(7) Total inflow of refugees	.073	(.020)
(8) 1-p, where p is pre-reform distribution (81/83 immigrants)	.059	(.017)
<u>Restrictions on the sample</u>		
(9) Delete top 8 percent of initial earnings distribution (to conform to aggregate statistics on immigrant inflow)	.060	(.018)
(10) Only individuals from countries listed as refugee source countries by the Immigration Board	.066	(.024)
(11) 1989 immigrants only (most likely to be exogenously placed)	.056	(.037)
(12) Delete top 10 percent in residual earnings distribution (check if estimate is driven by high ability individuals being able to choose preferred location)	.056	(.017)
(13) Delete top 10 percent in residual earnings distribution on the basis of pre-reform distribution (high ability individuals able to choose, choices similar to previous immigrants')	.067	(.020)

Notes: Entries show the elasticity of earnings with respect to ethnic concentration. The weighting procedure (used in variations under “Weighting”) gives higher weight to observations initially in municipalities where the probability is higher that the individual was placed by the government. All local variables are treated as endogenous. The regressions were estimated by IV (2SLS); the instrumentation is explained in Table 3. For (12) and (13), we predict residuals from an OLS earnings regression like the one in Table A2, but without local variables.

We now turn to the potential problems related to our data. Since we lack information on the individual’s refugee status, it is natural to ask whether this produces a significant bias in our estimates. Of course, we cannot settle this issue completely, but this section reports some evidence suggesting that this problem does not cause great concern.

By comparing the number of individuals in our sample with the total number of allotted residence permits during 1987 to 1989, we conclude that we sample too many individuals. This leads to the suspicion that some of our individuals may have immigrated for other reasons than political and, hence, were not assigned a municipality by the Immigration Board. In particular, some of our individuals may have immigrated for labor market reasons.

We approached this problem in two ways. The first approach is to use information on

the number of received refugee immigrants in each municipality. We reweigh our data such that the distribution of the initial locations of the individuals in our sample corresponds to the distribution of received refugees over municipalities. One weighting procedure is based on the number of refugees covered by grants from the Immigration Board (row (6)). Thereby, we can also address the problem that some individuals may already have relocated from the assigned municipality when we first observe them. As an alternative (row (7)), we use the distribution of initial locations from individual data in another dataset (FLYDATA; see Rooth (1999)). Both these weighted estimates of ethnic concentration are higher than the baseline estimate.

A second way to handle the lack of data on individuals' refugee status is restricting the sample. One idea is that those with the highest earnings during the year of immigration (period t) are least likely to be refugees; this appears a plausible assumption given that refugees could live on welfare during their initial period in Sweden. So, we excluded individuals at the top end of the period t earnings distribution until the number of sampled individuals was conformable with aggregate statistics (row (9)). This exclusion produced a reduction of the earnings sample by 8 percent, leaving the estimate of ethnic concentration essentially unchanged.

Moreover, we have also restricted the sample to individuals arriving from countries listed as refugee source countries by the Immigration Board (row (10)). This restriction eliminates a number of small countries in terms of immigrant inflow and produces a somewhat higher estimate of the effect of ethnic concentration than the baseline estimate. Thus we conclude that, if anything, the fact that we do not observe refugee status tends to bias our estimates of the causal effect of living in enclaves downwards.

A related problem with our data is the fact that we do not observe which individuals were subjected to the placement policy. We know that about ten percent of refugees chose not to participate in the program, and there is a possibility that some refugees were able to influence their placement. We have tried various ways to assess whether this may bias our estimates.⁴¹

Our first attempt to alleviate this problem is based on the assumption that, in the

⁴¹ To test whether the missing information on placement in combination with the estimation approach yields an upward bias in the estimates, we used a "fake" IV procedure. We applied our IV approach to the pre-program cohort (1981/83). OLS and IV applied to this cohort should give the same estimates. This turned out to be the case.

absence of the placement program, refugees would have behaved similarly to previous refugee immigrants. Consequently, we use the pre-program distribution of initial location in weighting the estimates (row (8)). Municipalities that received a large share of immigrants in the 1981/83 cohort, are weighted down relative to municipalities that received few immigrants. The results are not affected by this weighting procedure.

Another way to try to get at the effects of missing information on placement is to restrict the sample to those who received their residence permits in 1989. This was when the placement policy was most restrictive. Also in this case, the point estimates are virtually identical in comparison with the full sample; see row (11).

The above two robustness checks are based on the assumption that the probability of being treated (placed) is independent of unobserved characteristics. This is a strong assumption. A reasonable hypothesis may be that the skilled (in a general sense) are more able to influence their placement than the unskilled. What does this hypothesis imply for our estimates? One approach to this question is to delete the educational dummies from the set of individual controls. The idea is that if the probability of influencing placement is similarly affected by unobserved and observed skills (i.e. schooling), dropping education provides information on the sign of the omitted variable bias. The result of this experiment is that the estimate on ethnic concentration drops somewhat; see row (6). This suggests that there is no upward bias as a result of the fact that those with plenty of unobserved skills may have been able to influence their placement.

Another approach to get a handle of the bias resulting from unobserved skills influencing placement, is to drop the top of the residual earnings distribution. We have explored two variants of this approach. First, we dropped the top ten percent of the overall residual distribution (row (12)). Second, under the assumption that the choices of location would have been similar to previous refugees, we dropped the top ten percent using the pre-reform distribution of municipalities as weights (row (13)). These two sample restrictions gave estimates that were similar and somewhat higher, respectively, compared to the baseline estimates.

In summary, we have performed a large number of robustness checks and find the basic estimates to be stable. If anything, the baseline estimates probably understate the

true effect of ethnic concentration.

6.4 Does the return to living in an enclave vary across groups?

Our approach to answering this question is to interact measures of skill with local characteristics. We have considered interactions with gender, individual education and region of origin. We found no differences across region of origin, nor between males and females.⁴² There were some interesting differences across educational groups however.

Table 6 presents estimates for various educational groups, analogous to those in Table 3. The effect of ethnic concentration exhibits an interesting pattern. The positive effect of living in an enclave declines monotonously with observable skill until we reach the university level. For individuals with the highest education we also estimate a positive (and significant) effect. It is possible that the latter result is due to cream skimming by municipal officers (although our previous evidence does not suggest that this is a major issue). Due to this potential problem and the relative magnitude of the estimated effects, we are inclined to interpret the results in Table 6 as saying that the least skilled are the ones who gain most from living in ethnic enclaves.⁴³ This is consistent with a story that emphasizes that enclaves are associated with ethnic networks who primarily benefits the least skilled.

⁴² The estimate of the elasticity of earnings with respect to ethnic concentration was 0.055 for males and 0.065 for females, with standard errors of 0.023 and 0.025, respectively.

⁴³ Interestingly, this is also the group that gains the least from language acquisition, according to Berman *et al.* (2000).

Table 6: The effect of local characteristics by education, standard errors in parentheses

	missing & < 9 years	Educational level				
		9–10 years	high school ≤ 2 years	high school > 2 years	university < 3 years	university ≥ 3 years
Ethnic concentration (η^e)	.156 (.039)	.123 (.034)	.037 (.035)	-.067 (.042)	.051 (.044)	.092 (.044)
Immigrant concentration (η^m)	.250 (.210)	-.310 (.162)	.288 (.197)	.225 (.237)	.398 (.386)	-.018 (.353)
Local unemployment	-.021 (.065)	-.211 (.087)	-.129 (.063)	-.150 (.070)	-.229 (.103)	-.070 (.120)
# individuals	1,041	1,170	1,220	1,222	922	843

Notes: All regressions were estimated by IV (2SLS). Local variables are instrumented as explained in Table 3. Robust variance estimates, allowing for correlation across individuals residing in the same municipality.

7. Concluding remarks

The main purpose of this paper has been to provide estimates of the causal effect on economic outcomes of living in an enclave. 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 may be considered as exogenous. This policy initiative provides a unique natural experiment, which allows us handle the endogeneity problem due to individuals' choice of residence.

Our empirical analysis suggests two principal conclusions. First, we find evidence of sorting across locations. There is a substantive downward bias in estimates that do not account for sorting. Second, and perhaps more importantly, when accounting for the endogeneity of residential choice we find that an increase in ethnic concentration gives rise to a (statistically significant) *improvement* in labor market outcomes (earnings and idleness). For instance, the earnings gain associated with a standard deviation increase in ethnic concentration is in the order of four to five percent. Further, these gains appear to be concentrated in the lower end of the observable skill distribution.

We have offered evidence suggesting that ethnic enclaves contribute to an increase in the *level* of earnings. This is consistent with the view that the enclave offers a “warm embrace”, helping immigrants escape the discrimination encountered elsewhere on the labor market. Another issue is whether enclaves also contribute to earnings assimilation, i.e., to the rate of *change* in earnings. It may well be that the warm embrace is too comfortable so that incentives to move upwards on the job-ladder are reduced. Recent evidence presented in Borjas (2000) suggests that this indeed is the

case: enclaves decrease immigrant assimilation. However, this evidence potentially suffers from the Tiebout bias that we have addressed.

Finally, let us note that we have not exploited the full potential of the data. Our analysis has been based on rather broad geographical units. But ethnic segregation may vary a great deal within these broader units. During the years of the settlement policy, refugee immigrants were actually placed directly in an apartment. This fact gives ample scope for utilizing information on finer geographical units, such as housing blocks.

The questions of whether ethnic clustering affects immigrant assimilation and the consequences of housing segregation for individual outcomes are of substantial interest. We have put these issues on our research agenda.

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Appendix

Table A1: Summary statistics

Variable	Total sample (# individuals: 9,883)		Earnings sample (# individuals: 6,418)	
	Mean	Std. Dev.	Mean	Std. Dev.
<u>Local characteristics</u>				
ln (ethnic group) ($\ln e$)	5.252	2.110	5.104	2.111
ln (other immigrants) ($\ln m$)	9.061	1.575	8.954	1.601
ln (natives) ($\ln n$)	11.619	1.226	11.534	1.247
Unemployment rate (percent)	5.885	1.391	5.825	1.404
Ethnic concentration ($e/(n + e + m)$)*10 ² (standard deviation within country)	.3478	.4016 (.2424)	.3304	.3932 (.2401)
Immigrant concentration ($m/(n + e + m)$)*10 ²	7.880	3.275	7.761	3.339
<u>Individual characteristics</u>				
Earnings	--	--	10.955	1.513
Idleness	.292	.455	--	--
Welfare dependency	.321	.467	--	--
Stayer	.538	.499	.544	.498
Female	.445	.497	.447	.497
Age	38.124	8.289	37.437	7.506
Married	.592	.492	.624	.484
Kid	.514	.500	.560	.496
Education: Missing and < 9 years	.214	.410	.162	.369
9–10 years	.185	.388	.182	.386
High school ≤ 2 years	.168	.374	.190	.392
High school > 2 years	.185	.388	.190	.393
University < 3 years	.134	.341	.144	.351
University ≥ 3 years	.114	.317	.131	.338
Region of origin: Eastern Europe	.180	.384	.201	.401
Africa	.116	.320	.117	.321
Middle East	.457	.498	.403	.490
Asia	.083	.277	.089	.284
South America	.164	.370	.190	.393
Immigration year: 1987	.260	.439	.276	.447
1988	.355	.478	.357	.479
1989	.385	.487	.367	.482

Notes: All variables refer to the situation in $t+8$. Entries for local variables under the heading “Total sample” are population weighted. Entries for local variables under the heading “Earnings sample” are employment weighted. The “standard deviation within country” for ethnic concentration is a weighted average of the standard deviations within each country of origin.

Table A2: Basic OLS and IV estimates, standard errors in parentheses.

	ln(earnings) earnings>0		Pr(idle)	
	(1) OLS	(2) IV	(3) OLS	(4) IV
Panel a)				
ln(ethnic group)	.020 (.013)	.057 (.016)	-.006 (.003)	-.017 (.004)
ln(other immigrants)	.030 (.039)	.135 (.121)	.032 (.012)	.026 (.029)
ln(natives)	-.085 (.063)	-.161 (.150)	-.012 (.015)	-.001 (.044)
Unemployment rate	-.083 (.018)	-.140 (.047)	.020 (.004)	.032 (.010)
Panel b)				
Female	.034 (.070)	.032 (.071)	-.003 (.024)	-.002 (.024)
Age	.052 (.022)	.055 (.022)	-.015 (.005)	-.016 (.005)
Age squared (*10 ⁻²)	-.057 (.027)	-.061 (.027)	.030 (.006)	.030 (.006)
Married	.239 (.068)	.244 (.071)	-.077 (.018)	-.076 (.019)
Kid	-.062 (.058)	-.031 (.058)	-.041 (.013)	-.045 (.013)
Married*female	-.128 (.085)	-.105 (.085)	.033 (.021)	.029 (.022)
Kid*female	-.305 (.095)	-.324 (.098)	-.028 (.023)	-.025 (.023)
Education (missing and < 9 years, ref.)				
9–10 years	.059 (.056)	.051 (.055)	-.113 (.014)	-.111 (.014)
High school ≤ 2 years	.184 (.082)	.179 (.082)	-.185 (.013)	-.183 (.013)
High school > 2 years	.191 (.062)	.175 (.064)	-.162 (.014)	-.158 (.015)
University < 3 years	.110 (.065)	.090 (.066)	-.253 (.019)	-.249 (.018)
University ≥ 3 years	.479 (.086)	.464 (.088)	-.238 (.013)	-.236 (.014)
Region of origin (Eastern Europe, ref.)				
Africa	-.197 (.110)	-.211 (.111)	.030 (.020)	.023 (.024)
Middle East	-.544 (.061)	-.592 (.074)	.112 (.012)	.122 (.013)
Asia	-.021 (.085)	-.007 (.088)	.017 (.016)	.006 (.018)
South America	-.036 (.054)	-.050 (.056)	-.056 (.020)	-.054 (.021)
Immigration year (1987, ref.)				
1988	-.102 (.041)	-.119 (.050)	.053 (.009)	.057 (.011)
1989	-.145 (.044)	-.193 (.081)	.101 (.014)	.113 (.014)
# individuals	6,418	6,418	9,883	9,883
R-squared	.063	.054	.120	.118

Notes: IV estimation by 2SLS using values in the assigned municipalities as instruments for ($t+8$) local variables. Robust variance estimates, allowing for correlation across individuals residing in the same municipality.