J Popul Econ (2004) 17:133–155
DOI 10.1007/s00148-003-0143-4

Journal of Population Economics

Settlement policies and the economic success of immigrants

Per-Anders Edin[1], Peter Fredriksson[1], Olof Åslund[2]

[1] Department of Economics, Uppsala University, and Institute for Labour Market Policy Evaluation (IFAU), Box 513, SE-751 20 Uppsala, Sweden (Fax: +46-18-4711478; e-mail: {per-anders.edin; peter.fredriksson}@nek.uu.se)
[2] Institute for Labour Market Policy Evaluation (IFAU), Box 513, SE-751 20 Uppsala, Sweden (Fax: +46-18-4717071; e-mail: olof.aslund@ifau.uu.se)

Received: 18 January 2001/Accepted: 3 September 2002

Abstract. Many countries use settlement policies to direct the inflow of immigrants away from immigrant dense areas. We evaluate a reform of Swedish immigration policy that featured the dispersion of refugee immigrants, but also a change in the approach to labor market integration. We focus on how immigrants fared because of the policy. The evaluation indicates that immigrants experienced substantial long run losses. The bulk of the effect stems from a common component that affected immigrants regardless of location. We interpret the common component as being related to a shift in policy focus, from labor market assimilation to income support.

JEL classification: J15, J18, R23

Key words: Immigration, settlement policies, labor market outcomes

1. Introduction

The past couple of decades have seen an acceleration of immigration to developed countries. Concomitantly, there has been a shift in the ethnic

---

All correspondence to Per-Anders Edin. We thank two anonymous referees, Magnus Löfström, seminar participants at the Institute for Labour Market Policy Evaluation (IFAU), Uppsala University, Stockholm University, the Swedish Institute for Social Research (SOFI), the Research Institute of Industrial Economics (IUI), and the CEPR conference on "Marginal Labour Markets in Metropolitan Areas" for valuable comments and Lisa Fredriksson for expert data assistance. We are also grateful to Sven Hjelmskog, Roland Jansson, Stig Kattilakoski, Christina Lindblom, Anders Nilsson, Kristina Sterne, and Lena Axelsson of the Immigration Board, and Anna Gralberg of the Ministry of Culture, who generously found time to answer our questions. This research has been partly financed through a grant from the Swedish Council for Work Life Research (RALF). *Responsible editor:* Christoph M. Schmidt.

composition of the immigrant inflow and an associated decline in the labor market performance of recently arrived immigrants compared to natives in these countries; see, e.g., Borjas (1999). These developments have put immigration policies high on the political agenda.

One kind of immigration policy imposes restrictions on where newly arrived immigrants can settle. We call such policies *settlement policies*. Many countries practice (or have practiced) settlement policies; examples include the UK, Germany, and Sweden.[1] Under the new UK Asylum and Immigration Bill, refugee immigrants are placed outside London and Southeast England – the two regions where most previous immigrants reside. Germany imposes severe restrictions on where refugee immigrants can settle: unless having found a paid job, people must stay in a part of the country assigned by the government. In Denmark as well as the Netherlands, authorities try to disperse immigrants by obliging all municipalities to provide dwellings for a certain number of refugees (Dutch Refugee Council 1999). In addition, local dispersal policies have been used within European metropolitan areas (Musterd et al. 1997). Sweden is another example, where a new system for refugee immigrant reception was introduced in the mid 1980s. One aspect of the system was that asylum seekers were placed in regions outside the metropolitan areas to a greater extent. Thus, settlement policies are commonly employed, and a vital ingredient of the policies is the attempt to reduce immigrant concentration in big city areas.

Broadly, two arguments have been put forward to rationalize the placement of new immigrants outside immigrant dense regions. The first argument is based on the idea that living in immigrant enclaves stalls immigrant assimilation by decreasing the rate of host country skill accumulation. The second argument is based on the perception that immigrants impose a fiscal and social burden on the host regions. Placing immigrants outside the major cities thus distributes the perceived cost more equally across the host country.

In this paper we examine the versatility of the first type of argument. In particular we ask the question: How did the Swedish settlement policy affect the economic outcomes of the immigrants subjected to the policy? The Swedish experience may be instructive for countries pursuing, or thinking of implementing, similar kinds of policies.

Immigrant enclaves are not necessarily harmful for immigrants living there. The enclave may present the immigrant with more opportunities for gainful "trade" in the goods and labor markets; e.g., Lazear (1999). Also, ethnic neighborhoods may constitute an environment where the immigrant is exposed to less discrimination. Moreover, an ethnic "network" may improve labor market outcomes by providing information on job opportunities. Of course, network effects may also operate in the opposite direction by, e.g., disseminating information on welfare use; see Bertrand et al. (2000). One can also tell stories based on human capital externalities. According to one version, the quality of the enclave (the average stock of human capital) determines whether segregation is good or bad; e.g., Borjas (1998). In sum, the effect of living in an immigrant dense region is *a priori* ambiguous.

Many empirical studies find a negative *association* between the economic outcomes of ethnic minorities and neighborhood characteristics such as segregation, immigrant density or ethnic concentration; see Kain (1992) and Ihlanfeldt and Sjoquist (1998) for surveys. Whether these estimates should be treated as *causal* effects is another issue, since the majority of these studies take local characteristics as exogenous when they are arguably endogenous. In related work we have examined the severity of this problem; see Edin et al. (2003).[2] There we used the settlement policy as an exogenous source of variation arguing that initial placements were independent of unobserved individual characteristics. We found a substantial downward bias in estimates that do not account for sorting; in fact, estimates that were free of sorting bias suggested that an increase in ethnic concentration improved the labor market outcomes for low skilled refugee immigrants. Thus, from an empirical point of view, the effects of redirecting the inflow of immigrants away from enclaves are far from clear-cut.

There was more to the policy shift than trying to increase the dispersion of immigrants in Sweden. Prior to 1985, the integration of refugee immigrants had a direct connection to labor market policy since handling refugee issues was the responsibility of the Labor Market Board. This link was broken in 1985, when the Immigration Board became responsible for refugees. One of the implemented changes was that immigrants were, by default, placed on welfare for an introductory period of about 18 months.[3] Thus, it seems that the new policy effectively shifted the focus from labor market integration to income support. Given that there were two facets of the policy shift, we try to decompose the effects of the shift into its component parts: one associated with redirecting the inflow of immigrants from immigrant dense regions; and another which we interpret as being associated with the shift in the approach to integration policy.

Our results can briefly be summarized as follows. The overall effect of the policy was that immigrants suffered substantial long-run earnings losses. We estimate an earnings loss of about 25% for those subjected to the policy. In addition, idleness increased by one third and welfare receipt by almost 50%. The decomposition analysis suggests that the bulk of this loss is due to the shift in the approach to integration policies. With respect to the settlement policy *per se*, we find that had individuals stayed on in the assigned municipalities their labor market prospects would have been decidedly worse. However, individuals moved out of regions with bad employment prospects, so that the initial effects were undone.

The remainder of the paper is outlined as follows. Section 2 gives a description of the institutional setting. In Sect. 3, we present the evaluation framework. Section 4 turns to the empirical analyses. We use longitudinal micro data derived from the database LINDA (see Edin and Fredriksson 2000). In Sect. 5 we evaluate the policy in terms of three outcomes: earnings, idleness, and welfare dependence. Section 6 concludes.

2. Background

Immigration to Sweden for labor market reasons virtually ceased in the mid 1970s. Since then immigration from non-Nordic countries is mainly related to asylum reasons. Refugees applying for asylum in Sweden are granted a residence permit in case of a favorable decision. Upon receiving a residence permit, refugee immigrants are, as a rule, treated as Swedish citizens; the sole exception from this rule is the right to vote in the national elections. So, work permits and residence permits go together and immigrants are immediately eligible for transfers such as social assistance (welfare).

The remainder of this section describes the institutional setting prior to the reform and the major institutional changes that were implemented in 1985 when the settlement policy was introduced.

2.1. *Refugee reception before the reform*[4]

Prior to 1985, the Swedish Labor Market Board handled refugee issues. Presumably this was a remnant from the time when most immigrants entered Sweden for labor market reasons. Nonetheless, this meant that there was a natural focus on labor market integration and immigration policy was linked to labor market policy.

A majority of the refugee immigrants traveled directly to a municipality and applied for asylum there. The rest of the refugee immigrants were so called quota refugees who stayed in refugee centers before moving to a municipality.[5] The Labor Market Board was responsible for organizing housing (if necessary) and assisting refugees in finding suitable training or employment. It seems that all refugee immigrants could influence the choice of initial residence; in any case, a vast majority arrived in the regions of Stockholm, Gothenburg or Malmo (Ministry of Labor 1981, 1983).

The transition from basic language training, via additional courses in Swedish, to work or training was relatively smooth. Immigrants who had traveled directly to a municipality waited 2–5 months after receiving their residence permit before moving on to work or training.

2.2. *Refugee reception after the reform*

The responsibility for handling refugee issues was transferred to the Swedish Immigration Board. Formally the change took place in 1985, but there was a trial period in the autumn of 1984.

After the reform, all refugee immigrants, apart from reunification immigrants, were subjected to the settlement policy. In principle, an asylum seeker was placed in a refugee center, which were distributed all over Sweden, while waiting for a decision from the immigration authorities. After receiving a residence permit, the Immigration Board assigned immigrants to a municipality of residence; during this process there was no interaction between refugees and municipal officers. 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. At first the intention was to sign contracts with 60 (out of Sweden's 284) municipalities that had suitable characteristics for reception, such as educational and labor market opportunities. Due to the increasing number of asylum seekers in the second half of the 1980s, a larger number of municipalities became involved; in 1989, 277 out of Sweden's 284 municipalities participated. The factors that were supposed to govern placement were abandoned from the start. Instead, the availability of housing was the deciding factor.

Employment opportunities were scant in the majority of municipalities that became involved in asylum reception. To rationalize placement in regions with poor employment prospects, the Immigration Board divided the integration process into two periods: an introductory period lasting for 18 months when the immigrant participated in Swedish courses and lived off welfare;[6] after the introductory period, integration into the labor market commenced.

The move to a strict settlement policy was a reaction to the concentration of immigrants to large cities that had taken place. The immigrants were to stay in the assigned municipalities during the entire introductory period. However, there were no restrictions against relocating if individuals could find a place on their own. The only real cost to the immigrant consisted of a wait for a new place in a language course. Receipt of welfare was not conditional on residing in the assigned municipality and the central government reimbursed the local governments for their welfare expenditures. From 1985 through 1990, municipalities were reimbursed for their actual expenditures, so there was little incentive for local governments to pursue a policy that produced early assimilation into employment. This changed in 1991 when municipalities were given a fixed amount per refugee.

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.[7] However, the strictness of the placement policy gradually eroded during 1992–1994, when there was an immigration peak caused by the war in Bosnia-Herzegovina. The post-1991 period is less attractive for our purposes, 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.[8] In effect, it seems that the law formalized a stricter practice, which had been introduced in 1987. During 1987–1991, the placement rate, i.e. the fraction of refugee immigrants assigned an initial municipality of residence by the Immigration Board, was close to 90%.

In a companion paper (see Edin et al. 2003) we argue the settlement policy provides an exogenous source of variation that identifies the causal effect of regional characteristics. The essence of the argument is that the placement rate was high (in particular during 1987–1991), the housing market was booming (making it difficult to find vacant housing in attractive areas), and there was no interaction between representatives of the municipality and the refugee in question. Therefore, 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.[9]

The municipal placement policy is an obvious difference between the pre- and post-reform periods. Our reading of the facts is that the reform also shifted the policy focus away from labor market assimilation to an increased reliance on income support. The reform meant that direct link to the employment offices providing job search assistance, subsidized employment, or training programs was broken. Partly as a result of this, the integration into the labor market became less smooth and contained longer spells of inactivity. While immigrants arriving prior to the reform got some attachment to the labor market fairly quickly, individuals arriving after the reform were granted (or subjected to) a long initial period of welfare receipt.

To conclude, on our reading the reform brought two major changes: the placement policy where the government assigned the initial place of residence, and a shift in focus away from labor market integration in favor of income support.

3. The evaluation framework

In order to set the stage for the empirical analysis, we devote this section to making clear what we can estimate and the kind of restrictions that are necessary to estimate the parameters of interest.[10] The problem is that we think that location matters for outcomes, but due to sorting we cannot directly obtain consistent estimates of the impact of regional characteristics. In this section we therefore ask two questions. What does an estimate of the overall effect of the policy – one derived without conditioning on regional characteristics – actually measure? Which restrictions must be satisfied for this estimate to be meaningful?

We are also interested in decomposing the estimate of the overall effect of the policy. Our approach is to combine a difference-in-differences estimate of the overall effect with an estimate of the distribution effect. The distribution effect relates to the fact that immigrant arrival cohorts were subjected to different regional environments because of the placement policy. The residual in this decomposition is by construction the common effect of the policy. The distribution component can be estimated for the refugees arriving during the strict settlement policy since the initial placement of these individuals provides exogenous variation that identifies the effects of regional characteristics.

For purpose of concreteness, let us consider the determination of earnings for immigrants (indexed by m). For expositional reasons let us introduce the following assumptions. First, let the conditioning on exogenous individual characteristics be implicit. Second, assume that there are only two regions; associate a dummy variable d with one of them and let the characteristics of this region relative to the other be denoted z_τ. Third, suppose that the individual return to regional characteristics is constant and equal to β^m.

Consider a simple structural earnings equation for an immigrant i in arrival cohort $\tau = 0, p$ ($\tau = p$ for those who arrived during the strict placement policy)

$$\ln y_{i\tau}^m = a_\tau^m + \beta^m z_\tau d_{i\tau} + \alpha_{i\tau}^m \tag{1}$$

where $\alpha_{i\tau}^m$ denotes unobservable ability relative to the mean outcome (a_τ^m) in cohort τ. The estimation of Eq. (1) is complicated by the fact that individuals choose where to reside. In Edin et al. (2003), we considered the sorting bias that may arise in this setting. In the simple framework we are examining here, the bias arises to the extent that there is a covariance between $\alpha_{i\tau}$ and regional characteristics z_τ. To be concrete, suppose for the moment that z_τ reflects immigrant density; then, if high ability individuals choose to live outside immigrant enclaves, the OLS estimate of β^m is biased downwards – and vice versa.

Assuming that there are no time effects, one can in principle use Eq. (1) to form a before-and-after estimator. However, given the time period we are considering this is an unattractive assumption. We measure the outcomes in 1995–1997 for those subjected to the policy, and in 1989–1991 for pre-reform immigrants. In the beginning of the 1990s the unemployment rate in Sweden skyrocketed from less than 2% in 1989 to over 9% in 1993.

To eliminate the time effect, suppose instead that there exists a proper comparison group (superindexed *c*). By analogy with Eq. (1), the earnings equation for the comparison group is given by

$$\ln y_{i\tau}^{c} = a_{\tau}^{c} + \beta^{c} z_{\tau} d_{i\tau} + \alpha_{i\tau}^{c} \tag{2}$$

If we had some exogenous information identifying the coefficient on regional characteristics for all groups, we could use Eqs. (1) and (2) to form a difference-in-differences estimator of the common effect of the policy. This estimator would be given by $\pi = (a_p^m - a_0^m) - (a_p^c - a_0^c)$, if the change in unobserved characteristics is the same in the two groups, where $(a_p^c - a_0^c)$ identifies the pure time effect.

For reasons outlined above we do not want to condition on region of residence. The question then is: What does the difference-in-differences estimate without conditioning on *d* measure? To answer this question, average the structural earnings equations separately within group and time period and calculate the difference in difference using these averages. Assuming that any change in unobserved ability over time is equal in both groups, this yields

$$\begin{aligned}\delta = \Delta \ln y^m - \Delta \ln y^c = {} & \underbrace{((a_p^m - a_0^m) - (a_p^c - a_0^c))}_{1} + \underbrace{\beta^m z_p [\Delta d^m - \Delta d^c]}_{2} \\ & + \underbrace{(\beta^m - \beta^c) z_p \Delta d^c}_{3} + \underbrace{\beta^c (z_p - z_0)(d_0^m - d_0^c)}_{4} \\ & + \underbrace{(\beta^m - \beta^c)(z_p - z_0) d_0^m}_{5} \end{aligned} \tag{3}$$

where Δ is the difference operator, and d^l reflects the distribution across regions in group $l = m, c$. Notice that we can estimate the coefficient on $[\Delta d^m - \Delta d^c]$ by applying instrumental variables to the immigrant cohort subjected to the settlement policy.

Equation (3) is written on a form that separates the components of interest from the ones we want to eliminate by imposing restrictions and choosing a suitable comparison group. It is clear that the first and the second component are key elements of the policy change. As we have argued, there was a common component in the policy shift; the first component measures this effect. Also, there was a regional dimension to the policy since immigrants were dispersed across different regions – this distribution effect is measured by the second component in Eq. (3). We refer to the sum of the common and distribution effect as the overall effect of the policy. The remaining components should not be attributed to the policy.[11]

In the following proposition, we detail the conditions that the comparison group must satisfy in order for the "raw" difference-in-differences estimator (δ) to give a sensible estimate of the overall effect of the policy.

Proposition. A proper comparison group should have the following characteristics relative to the refugee immigrants: (i) the pure time effect should be the same; (ii) the changes in unobserved characteristics (if any) should be equal; (iii) the return to regional characteristics should be the same ($\beta^m = \beta^c$); and (iv) the distribution across regions prior to treatment should be equal ($d_0^m = d_0^c$). Under conditions (i)–(iv) the difference-in-differences estimator of the overall effect is given by[12]

$$\delta = ((a_p^m - a_0^m) - (a_p^c - a_0^c)) + \beta^m z_p[\Delta d^m - \Delta d^c] = \pi + \gamma[\Delta d^m - \Delta d^c] \quad (4)$$

The first condition suggests that we should look for comparison groups along the observable skill dimension. The fourth condition arises because we do not want to condition on region of residence. It may disqualify many groups as comparisons simply because we know that residence patterns among, e.g., natives and immigrants are very different. Empirically, the fourth condition is likely to be of some importance since it eliminates the influence of the fourth component in (3). We know for a fact that, e.g., the dispersion of regional unemployment increases (i.e. $z_p - z_0$ changes) along with the overall rate of unemployment. So the time effects that render the before-and-after estimator implausible will also reduce the plausibility of estimates derived using a comparison group that does not satisfy the fourth criterion. When we return to the selection of comparison groups in the next section, we discuss whether these four conditions are likely to be fulfilled.

Given that the conditions in the proposition are satisfied, we can meaningfully decompose the overall effect of the policy into a common effect, π, and a dispersion effect $\gamma[\Delta d^m - \Delta d^c]$. The decomposition is made possible by the fact that the settlement policy gives exogenous variation in regions of residence that can be used to identify η. We can thus apply an instrumental variables approach to the cohort that was subjected to the policy to estimate the coefficient on the regional dummy. It is important to realize that these estimates are free from sorting bias if one believes in the quasi-experimental nature of the data. In the next section, Eq. (4) and the criteria for identifying it, will guide our choice of empirical strategy and comparison group.

4. An empirical prelude

The section begins by describing the data, sample selections, and the characteristics of the treated and "control group". Then we move on to discuss the choice of comparison group.

4.1. Data and sample selection

The empirical analysis is based on the LINDA database. LINDA contains two panels: one of around 20% of the foreign-born population, and another of approximately 3% of the total Swedish population. 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.[13] As a general rule we include immigrants from countries that were not members of the OECD as of 1985 and countries outside Western Europe. The only exception from this rule is Turkey, which is included since it was the origin of a substantial inflow of refugee

Table 1. Descriptive statistics by immigrant cohort

	Immigrant cohort	
	1981/83	1987/89
Panel a)		
Age	37.55	38.12
	(8.46)	(8.29)
Years of schooling	10.83	11.16
	(2.95)	(2.99)
Female	0.479	0.445
Region of origin		
Eastern Europe	0.373	0.180
Africa	0.092	0.116
Middle East	0.233	0.457
Asia	0.142	0.083
South America	0.160	0.164
Panel b)		
Big city area (at time of arrival, t)	0.62	0.41
Big city area (8 years later, $t+8$)	0.69	0.59
Immigrant dense area (t)	0.61	0.38
Immigrant dense area ($t+8$)	0.64	0.55
High unemployment area (t)	0.12	0.18
High unemployment area ($t+8$)	0.12	0.13
# individuals	2,679	9,883

Notes: Years of schooling are imputed from highest degree attained. Individuals with missing information on education were given the same number of years of schooling as those with less than 9 years of schooling. The first time we observe education is in 1990. Therefore, education is measured 9 years post immigration for those arriving in 1981 and 8 years after immigration for later arrival cohorts. Panel b) presents means of indicator variables. Big city area refers to the Stockholm, Gothenburg or Malmo metropolitan areas. Immigrant dense areas are municipalities where the immigrant share is at least twice as large as the immigrant share of the population. High unemployment areas refer to municipalities where unemployment is in the highest quartile of the municipal unemployment distribution. Area characteristics are defined with respect to a specific point in time (1986) for both cohorts.

immigrants during the period. We exclude persons belonging to a household with an adult already residing in Sweden, since these individuals were likely to have immigrated as family members and, consequently, were not "treated". 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 1987–1989, when placement rates were of the order of 90%.

4.2. The characteristics of the treated and the control group

In order to evaluate the reform it is necessary to construct a counterfactual. For this purpose, we use individuals who are identified as refugee immigrants (according to the above criteria) during the years 1981–1983.

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 observed and unobserved characteristics.

In panel a) of Table 1, we compare the cohorts in terms of a set of standard individual characteristics: age, schooling, gender, and ethnicity.

There are no important differences in terms of age and gender. Also, the cohorts have very similar amounts of schooling: the 1987/89 cohort has 0.3 additional schooling years on average. The difference between the two cohorts in terms of ethnicity may be a greater source of concern. The chief discrepancy between the two cohorts is that the 1981/83 cohort has more of the mass among immigrants from Eastern Europe, while the later cohort has the greatest fraction of immigrants originating in the Middle East.

For the regression analysis, the difference in terms of ethnicity is not a problem as we can condition on the country of origin. However, the calculation of the distribution component requires the counterfactual location distribution. Therefore, we generate the counterfactual distribution by reweighing observations in the 1981/83 cohort such that the distribution over region of origin conforms to the 1987/89 cohort. Whenever we talk about *location differences* across cohorts in the sequel, we refer to the differences between the 1987/89 cohort and the weighted 1981/83 cohort.

In panel b) of Table 1 we illustrate some of the consequences of introducing municipal placement. It reports a set of characteristics of the municipality of residence at the time of arrival and eight years later for the two cohorts. At the time of arrival, those subjected to the placement policy have a substantially higher probability of residing outside the metropolitan areas, are more likely to live in high unemployment regions, and are substantially less likely to live in immigrant dense areas. So, at least initially, the policy objective of reducing immigrant concentration in big city areas was fulfilled. This policy objective may have come at a significant cost, however, as the probability of being exposed to a high unemployment environment also increased.

If we look at the cohorts eight years later it is evident that the difference between the two cohorts in terms of residence characteristics has decreased markedly. In particular, it seems that the 1987/89 cohort escaped high unemployment areas to a greater extent; the difference between the two cohorts has almost disappeared after eight years in the host country. It is also noteworthy that the two distributions have not converged. The probability of residing in metropolitan areas and immigrant dense areas is still around 10 percentage points lower for the 1987/89 cohort relative to 1981/83 cohort.

The last issue we want to discuss with the aid of Table 1 concerns the implications of the size of the inflow for the location distribution of refugee immigrants. The last row shows that the inflow during 1987/89 was 3–4 times larger than the inflow during the earlier period. Is the municipal distribution of the 81/83 cohort an appropriate counterfactual location distribution? It is possible that the increased inflow of immigrants would have changed the distribution also in absence of the reform. However, this problem is what the difference-in-differences between the distributions in Eq. (4) is meant to handle. The distribution of the 81/83 cohort plus the change within an appropriate comparison group is arguably the best approximation we can get of what the distribution would have been if the program had not been implemented.

Will changes in unobserved heterogeneity bias the evaluation?

It is difficult of course to assess whether unobserved heterogeneity will bias our policy evaluation. There are two issues here: first, independently of the

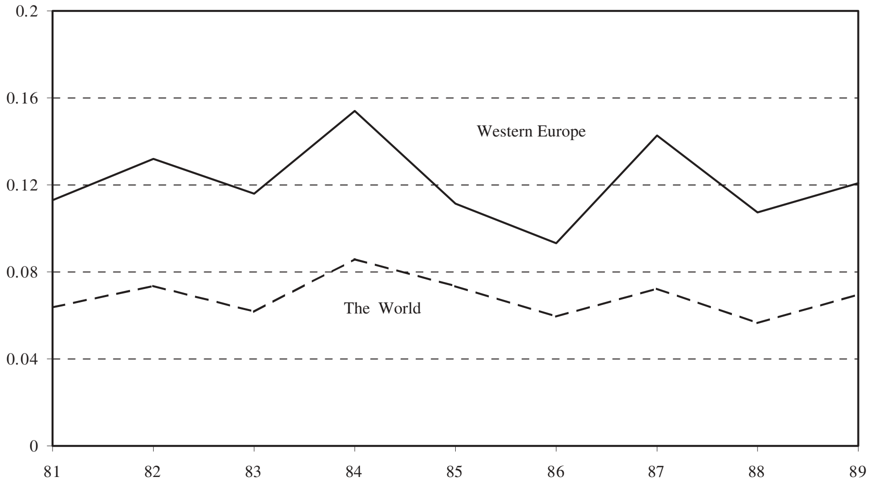

Fig. 1. Asylum seekers in Sweden as a fraction of asylum seekers in other regions, 1981–1989. *Notes:* Asylum seekers according to UNHCR (2000). Sweden is not included in "Western Europe"

reform there may be a decline in cohort quality over time; second, because of the reform there may be selection of refugees with unobserved characteristics that are not conducive to labor market success.

With respect to the first question, Borjas (2000b) argues that national origin is *the* crucial factor behind changes in cohort quality in the US. Since our data include information on country of origin we can control for the major influence of changes in cohort quality over time.

With respect to the second question, one might worry that the reform changed the selection of immigrants in terms of their disposition to work since it had an emphasis on income support and implied less pressure to enter the labor market quickly. Let us present three facts suggesting that this is not a major issue.

First, pre-reform refugees also had the possibility to live off welfare. Once granted a residence permit, they were eligible for social assistance in the same way as other residents. If there is a group of asylum seekers who base their destination choices on welfare opportunities, they probably found Sweden equally appealing before the reform.

Second, the share of asylum seekers going to Sweden did not change during the 1980s. The decade saw an overall increase in refugee migration, but as Fig. 1 shows the relative number of people seeking asylum in Sweden remained about the same during the entire decade.

Third, observed skills did not decrease. It is plausible that the highly educated are less inclined to live off welfare (see, e.g., Hansen and Lofstrom 1999); if selection was a problem we would expect the education level of refugees to fall after the reform. According to Table 1, however, average years of schooling was more or less equal in the two cohorts; this result also holds when we condition on country of origin.

Based on the facts presented in this section we conclude that unobserved heterogeneity is not likely to be a major problem.

4.3. The choice of comparison group

In Sect. 3 we identified properties that a comparison group must have to make a meaningful analysis possible. We have considered three groups: low-skilled natives; OECD immigrants during 1981–1983 and 1987–1989; and non-OECD immigrants during 1975–1980.[14] The suitability of these groups depends on how well they conform to the conditions described in the proposition of Sect. 3. Since we have discussed condition (ii) already, we focus on the remaining conditions here.

Let us start with condition (i): a common time effect. This property is vital to credibly estimate the overall effect of the policy. Finding a group with identical time effects as refugee immigrants is of course a delicate matter, but it makes sense to think that the susceptibility to macro events is related to the amount of host country skills. The logic for considering the above comparison groups is based on this consideration. For instance, immigrants from the OECD countries arriving in the same time periods as the treated and control groups is the proper comparison group if the susceptibility to the cycle primarily is related to the time in the host country. If immigrants from certain regions fare worse than others during economic recessions, on the other hand, then the 1975/80 cohorts of non-OECD immigrants should be comparable with the samples of refugee immigrants.[15]

The fourth condition of the proposition stipulates that the distributions across regions prior to the reform should be equal. This condition potentially disqualifies natives, as immigrants (in particular low-skilled immigrants) are much more prone to live in metropolitan areas than natives. On the basis of this consideration, the preferred comparison group consists of immigrants from non-OECD countries.[16]

It is *a priori* plausible that the 1975/80 cohorts of non-OECD immigrants best meet the third condition, i.e., that there is equality of the return to local characteristics. In Edin et al. (2003), we find that members of high-skilled ethnic groups gained by being surrounded by a greater number of individuals from their own country. It is quite likely that this effect is different for natives and perhaps also for OECD immigrants.

There are additional reasons to prefer prior immigrants arriving from countries outside the OECD. One may worry that the size of the inflow itself affects outcomes. This supply effect is only a problem if recent arrivals are affected differently than the rest of the refugee immigrant population; otherwise, the 1975/80 comparison group should experience the same change.[17] Finally, relative to OECD immigrants, there is also the advantage of having a larger number of observations.

In sum, then, we primarily base our results on using immigrants from refugee countries arriving 1975–1980 as our comparison group. In some cases we present results using the OECD group. For reasons given above low-skilled natives is not a proper comparison group.

5. Policy evaluation

The purpose of this section is to evaluate the reform in terms of the economic outcomes of the participants. First, we investigate the overall effect of the policy, then we estimate the distribution effect of the policy and, finally, we decompose the overall effect into a common effect and a (geographical) dispersion effect.

5.1. The overall effect of the policy

In this section, we set out to estimate the total effect of the reform on the economic outcomes of the participants. We use a difference-in-differences approach and relate the 87/89 and 81/83 immigrant cohorts to the comparison groups discussed in Sect. 4.2. We measure outcomes eight years after immigration. The reason for choosing outcomes such a long time after immigration is that we are interested in the long-run impact on participants. Also, the policy did not aim at initial labor market success, since it prescribed language training and other introduction activities rather than immediate entry on the labor market. Note, though, that we have also looked at outcomes four and six years after arrival. The estimates are very similar.[18]

The re-direction of the inflow away from big-city regions was an important part of the policy change. Partly, this was motivated by a belief that regions matter for outcomes. However, conditioning on region of residence will potentially result in estimates suffering from sorting bias. Under the conditions outlined in the proposition we can still estimate a sensible overall program effect.

We focus on three outcome measures: (the log of) annual earnings, idleness (defined as neither having positive earnings nor being enrolled in education), and welfare receipt (defined as being a member of a welfare receiving household). The difference-in-differences estimates of the total effects of the reform are derived from the following prototype specification:

$$outcome_{ik} = \alpha_k + \phi_0'\mathbf{X}_{ik} + \phi_1'\mathbf{X}_{ik}m_{ik} + \eta_0 m_{ik} + \eta_1 p_{ik} + \delta m_{ik}p_{ik} + \varepsilon_{ik} \tag{5}$$

where $outcome_{ik}$ is the outcome in $t+8$ for individual i from source country k. α_k is a country of origin fixed effect, $\mathbf{X}$ a vector of individual characteristics (gender, age, age squared, marital status, and level of education), m a dummy variable for belonging to either the treated or the control group, and p a dummy for the reform period. In this setup, δ is the parameter of primary interest; it is the difference-in-differences estimator of the overall effect of the new policy. To simplify the decomposition of the overall effect, we estimate linear probability models for the binary outcomes.

Table 2 shows the results for the three outcomes, using two different comparison groups. Starting with earnings, we find that the effects are substantial; according to the estimates, program participants with earnings eight years after arrival suffered an earnings loss of 25–29% due to the policy.

Furthermore, the policy increased the likelihood of being idle. The estimates imply that the probability of idleness is about 6–8 percentage points higher for those subjected to the policy; in relative terms this translates to an increase of about a third.

For welfare receipt, the estimates indicate that program participants were 9–11 percentage points more likely than previous immigrants to receive social assistance eight years after arrival; in relative terms this amounts to an increase in the probability of close to 50%.

Table 2. Difference-in-differences estimates of the overall effect of the policy

	Outcome		
	Log(earnings)	Pr(idle)	Pr(welfare)
Comparison group			
OECD			
Estimate	−0.345	0.079	0.089
Standard error	(0.050)	(0.013)	(0.013)
R-squared	0.12	0.16	0.14
# individuals	13,187	18,279	18,279
Non-OECD immigrants 75/80			
Estimate	−0.285	0.062	0.108
Standard error	(0.035)	(0.009)	(0.010)
R-squared	0.10	0.14	0.11
# individuals	42,587	54,448	54,448

Notes: The regressions also include a dummy for being in the treated or the control group (m), gender, age, age squared, level of education, country of origin, marital status (dummies for (a) being married, (b) having at least one child under 16, and interactions between gender and (a) and (b)), and interactions between m and remaining individual characteristics. The full set of estimates is available in Table A2. For details on the different samples, see Table A1. The comparison group is weighted such that the sample composition over time corresponds to the treated and control group.

Supported by our discussion in Sect. 4.2, we believe that the two comparison groups used in Table 2 capture the relevant time effects in a reasonable way. Of course, we could always eliminate the program effect by comparing with a very restricted group, with especially bad outcomes. To provide a sense for the stability of the estimates, we have also excluded the quartile predicted to have the best outcomes in the two comparison groups; the results from this exercise are very similar to those in Table 2.[19]

There is no doubt that the outcomes of people subjected to the policy were worse than those of previous immigrants in absolute terms. The more difficult task is to separate the program effect from a time effect that would have changed the outcomes also in absence of the policy change. We have presented evidence that regardless of the comparison group, the program had a deteriorating effect on all outcomes. Furthermore, this holds in instances when we exclude high-skilled individuals (in the observed sense) from the comparison groups. Therefore, our conclusion is that the policy change adversely affected the economic outcomes of refugee immigrants.

5.2. Effects of the placement

We now turn to investigating the effects that *placement* had on economic outcomes: Were people located where there were good or bad prospects? In terms of Eq. (4), this analysis considers the distribution component, i.e., the effect of the change in distribution over municipalities on outcomes.

The residential location after some time in the host country is not exogenous. Therefore, we estimate versions of the following regression for the 1987/89 cohort:

$$outcome_{ijk} = \alpha_k + \phi' \mathbf{X}_{ik} + \sum_j \gamma_j d_{ijk} + \varepsilon_{ijk} \tag{6}$$

where *outcome* denotes either log earnings, the probability of being idle, or welfare dependency, α_k is a country of origin fixed effect, $\mathbf{X}$ a vector of individual characteristics (gender, age, age squared, marital status, level of education, and immigration year), and d_{ij} is a dummy variable for residing in municipality j. All variables included in Eq. (6) are measured eight years after immigration. To avoid bias due to sorting we instrument d_{ij}. We instrument the dummy for the individual's current municipality with two dummies: one indicates that the individual was placed in the current municipality, the other that the individual was placed in the county block of the current municipality. The first stage equation is estimated separately for each municipality. To ensure that the municipality effects on outcomes, η_j, could be identified, and reasonably precisely estimated, we required that there should be at least 5 refugee immigrants with positive earnings in our sample that reside in municipality j; if a municipality failed this requirement it was merged with a bordering municipality.[20]

We ask two questions in this part of the analysis. First, to what extent does the change in geographic distribution account for the overall effects of the policy? The answer to this question thus gets at the distribution component of Eq. (4). Second, what would the effect have been if people had stayed in their assigned municipalities?

We evaluate the placement effect in the following way. Let $d^m_{j\tau}$ be the probability of residing in municipality j in time period τ (0 if pre-reform, p if reform) for an individual belonging to the refugee immigrant population (m). Let $d^c_{j\tau}$ be the analogous probability for the comparison group (c). Our measure to answer the above questions is then

$$\sum_j ((d^m_{jp} - d^m_{j0}) - (d^c_{jp} - d^c_{j0}))\hat{\gamma}_j \tag{7}$$

Thus, we use the estimates of γ_j in combination with the differences in distributions of immigrants and the comparison group to calculate weighted averages of the location effects. We calculate two measures of placement success. First, we use the distributions corresponding to the time point when the treated and control groups have been eight years in Sweden; the difference-in-differences between the weighted averages is then the distribution component of equation (4). Second, we use the initial distributions, i.e., those that correspond to the time of arrival for the treated and control groups. We then estimate what the effect would have been if people had remained in their assigned municipalities. Thus we examine whether program participants were located in better or worse regions relative to the choices of previous immigrants.

Table 3 presents the estimates; the calculations use 1975/80 immigrants as the comparison group (estimates on individual characteristics are presented in Table A3). The distribution components relevant for the decomposition analysis are given in the first row. It seems that the distribution component accounts for a limited part of the overall effect of the policy on earnings; see column (1). There is a decrease in earnings of almost 8 percent associated with the change in the distribution caused by the policy. For idleness and welfare it appears as if the distribution component had more of an impact; see columns (2) and (3). There is an increase in idleness of around 3 percentage points as a result of the placement policy; analogously, welfare receipt increased by 4 percentage points.

Table 3. How placement affected outcomes, standard errors in parentheses

	(1) log(earnings)	(2) Pr(idle)	(3) Pr(welfare)
Distribution component of difference-in-differences estimate. Distribution eight years after immigration.	−0.077 (0.036)	0.030 (0.013)	0.039 (0.015)
Distribution component if people had stayed in the assigned municipalities. Initial distribution.	−0.542 (0.277)	0.185 (0.093)	0.268 (0.104)

Notes: The calculations use the difference-in-differences of distributions, with 75/80 immigrants as the comparison group. Observations in 1981/83 cohort weighted to conform to the region-of-origin distribution in the 1987/89 cohort. Estimates for individual variables presented in Table A3. The estimates for idleness and welfare receipt were obtained by applying the linear probability model.

Let us now turn to the second row and the comparison of the initial location effects in the immigrant cohorts. The estimates suggest that the initial location was associated with sizable earnings losses: had the refugees stayed on in the assigned regions their earnings would have been 42 percent lower relative to the control group.

People were initially placed in regions with bad prospects for employment. The difference in idleness obtained when applying the initial distributions is rather dramatic. The estimate suggests that had the refugees stayed on in the assigned municipality the probability of being idle would have been 19 percentage points higher compared to the situation when they could freely choose place of residence.

Column 3 shows that we get a similar result for welfare. The probability of being welfare dependent was 27 percentage points higher in the 87/89 cohort than in the 81/83 cohort using the initial distribution.

For all three outcomes examined, we see that relative to the 1981/83 cohort individuals escaped regions with bad prospects by moving out of assigned municipalities. Most of the rather dramatic differences observed initially have disappeared after eight years in the host country.

Did people relocate to better or worse regions also in absolute terms? To answer this question, we must look at the change in the distribution of each cohort between the initial time of observation and eight years later. To this end, we calculate the difference in the weighted average between the two time points for each cohort. The calculations indicate that there is very little change among pre-reform immigrants, and that the bulk of the decrease in the difference between cohorts stems from program participants moving to better locations. Thus, there is no evidence that immigrants in general move to better regions in terms of labor market prospects, but that this was a phenomenon connected with the placement policy.

Why was the initial location bad for outcomes? In related work, we estimate the causal effects of a set of municipal characteristics on the outcomes of program participants; see Edin et al. (2000a). We find that local unemployment has a large negative effect. Compared to the 81/83 cohort, the 87/89 cohort was assigned to municipalities with higher unemployment; see Table 1. A significant part of the answer probably lies in the fact employment opportunities were scant in many of the assigned municipalities.

Table 4. Decomposition of the overall effect, 1975/80 immigrants comparison group

	Total	Common	Distribution	Common/total (%)
Log earnings	−0.285	−0.208	−0.077	73.0
Idleness	0.062	0.032	0.030	51.6
Welfare	0.108	0.069	0.039	63.9

Notes: "Total" taken from Table 2 and "Distribution" from the first row of Table 3. "Common" is the difference between "Total" and "Distribution".

Taken together, our findings suggest that the long run effects of location on earnings, employment, and welfare receipt were relatively small. However, this is because people moved out of bad regions, not because placement created a distribution that was equal to that chosen by the 81/83 immigrants in terms of outcomes. On the contrary, had people stayed on in the assigned municipalities, their outcomes would have been substantially worse.

5.3. Decomposing the overall effect

Since we have estimated the overall effect and the distribution effect of the policy, we can "back out" an estimate of the common effect of the reform. Table 4 presents the results of this exercise; the comparison group is non-OECD immigrants during 1975–1980.

The decomposition in Table 4 suggests that for all outcomes a majority of the loss is due to the common component. Therefore, we conclude that the bulk of the impact of the reform came from a common effect, rather than from the change in geographic distribution induced by the placement policy. Our interpretation of this result is that the policy shift increased the initial reliance on income support, which had detrimental effects on people arriving under the new policy.

6. Conclusion

The purpose of this paper has been to evaluate a Swedish reform of the refugee immigrant reception system. The reform, which was implemented in the later half of the 1980s, had two facets. First, immigrants were no longer free to choose their initial place of residence; rather they were assigned to less immigrant dense regions. Second, the reform shifted the focus away from labor market assimilation. We have focused exclusively on how refugee immigrants fared because of the reform.

We use three outcome measures: log earnings, idleness, and welfare receipt. The overall effect of the reform was detrimental to all of these outcomes. Our analysis suggests that eight years after arrival earnings were roughly 25% lower because of the new policy; idleness increased by around seven percentage points; and welfare receipt rose by about 10 points.

Because there were two facets of the reform we decompose the overall effect into a common component and a distribution component. The distribution component is associated with the shift in the regional distribution of immigrants across Sweden. The decomposition analysis suggests that more than half of the total effect can be attributed to the common effect. A somewhat speculative reading of this is that the weaker link between refugee reception and integration into the labor market, and increased focus on income support, had long-lasting negative effects on individual outcomes.

The negative effects of the dispersion policy would, however, have been larger if people had stayed on in the assigned municipalities. If they would have stayed, the estimates suggest that the probability of being idle, for example, would have been almost 20 percentage points higher because of the placement policy. Since people moved out of the initial locations, this loss was largely undone, however.

In this Swedish case, the increased long-run dispersion of refugee immigrants that was achieved with the policy came at the expense of individual outcomes in the labor market. An implication of our findings is that with a more careful choice of municipalities, and with a policy more focused on the labor market, this cost could at least to some extent have been avoided. We think this is an important lesson for future policies in Sweden and elsewhere.

Appendix

Table A1. Descriptive statistics, means (standard deviations)

Variable	Refugee immigrants		OECD immigrants		Non-OECD immigrants 75/80	
	81/83	87/89	81/83	87/89	81/83	87/89
Log(earnings)	11.48	10.96	11.63	11.52	11.59	11.40
	(1.09)	(1.51)	(1.08)	(1.28)	(1.02)	(1.31)
Idleness	0.127	0.292	0.154	0.231	0.120	0.234
Welfare receipt	0.215	0.321	0.112	0.114	0.143	0.116
Female	0.479	0.445	0.533	0.448	0.486	0.487
Age	37.549	38.124	37.393	38.372	39.542	42.872
	(8.456)	(8.289)	(8.674)	(8.914)	(7.961)	(8.398)
Married	0.605	0.592	0.521	0.463	0.665	0.609
Kid	0.500	0.514	0.434	0.402	0.575	0.491
Education						
<9 years or missing	0.258	0.214	0.251	0.186	0.273	0.201
9–10 years	0.108	0.185	0.123	0.187	0.123	0.132
High school $\leq$ 2 years	0.288	0.168	0.239	0.217	0.285	0.317
High school $>$ 2 years	0.135	0.185	0.126	0.144	0.111	0.121
University $<$ 3 years	0.102	0.134	0.115	0.124	0.095	0.112
University $\geq$ 3 years	0.109	0.114	0.146	0.141	0.112	0.116
Ethnicity						
Nordic	–	–	0.622	0.644	–	–
Western Europe	–	–	0.312	0.262	–	–
Eastern Europe	0.373	0.180	–	–	0.351	0.338
Africa	0.092	0.116	–	–	0.091	0.087
Middle East	0.233	0.457	–	–	0.277	0.287
Asia	0.142	0.083	0.008	0.007	0.120	0.126
North America	–	–	0.051	0.070	–	–
South America	0.160	0.164	–	–	0.161	0.163
Oceania	–	–	0.007	0.017	–	–

Notes: Idleness = 1 if the individual neither had positive earnings nor was enrolled in education, 0 otherwise. Welfare receipt = 1 if the individual was a member of a welfare receiving household, 0 otherwise. Kid = 1 if there was a kid $\leq$ 15 years of age present in the household, 0 otherwise.

Table A2. Estimation results of Eq. (5)

	log(earnings)		Pr(idle)		Pr(welfare)	
	OECD	Im 75/80	OECD	Im 75/80	OECD	Im 75/80
p	−0.172	−0.232	0.075	0.092	0.000	−0.018
	(0.038)	(0.014)	(0.010)	(0.004)	(0.009)	(0.003)
$m * p$	−0.345	−0.285	0.079	0.062	0.089	0.108
	(0.050)	(0.035)	(0.013)	(0.009)	(0.013)	(0.010)
Female	−0.122	−0.065	0.020	−0.000	−0.030	0.005
	(0.061)	(0.030)	(0.017)	(0.009)	(0.014)	(0.007)
$m *$ (Female)	0.121	0.064	−0.027	−0.007	0.092	0.057
	(0.082)	(0.062)	(0.023)	(0.017)	(0.020)	(0.016)
Age	0.035	0.067	−0.018	−0.027	0.010	−0.004
	(0.020)	(0.009)	(0.006)	(0.002)	(0.004)	(0.002)
$m *$ (Age)	0.015	−0.017	−0.001	0.008	−0.022	−0.008
	(0.026)	(0.019)	(0.007)	(0.004)	(0.006)	(0.004)
Age squared ($*10^{-2}$)	−0.041	−0.072	0.028	0.041	−0.011	0.004
	(0.024)	(0.011)	(0.007)	(0.003)	(0.005)	(0.002)
$m *$ (Age squared ($*10^{-2}$))	−0.011	0.020	0.006	−0.007	0.032	0.018
	(0.031)	(0.023)	(0.008)	(0.005)	(0.007)	(0.005)
Married	0.269	0.252	−0.055	−0.083	−0.074	−0.055
	(0.070)	(0.031)	(0.022)	(0.010)	(0.015)	(0.007)
$m *$ (Married)	−0.039	−0.022	−0.029	−0.001	0.040	0.021
	(0.090)	(0.064)	(0.026)	(0.017)	(0.021)	(0.016)
Kid	0.046	−0.102	−0.092	0.099	0.039	0.060
	(0.009)	(0.030)	(0.022)	(0.071)	(0.007)	(0.017)
$m *$ (Kid)	−0.116	0.084	0.053	0.007	0.048	0.069
	(0.090)	(0.062)	(0.026)	(0.016)	(0.022)	(0.016)
Married $*$ female	−0.269	−0.097	0.019	0.034	−0.002	−0.056
	(0.088)	(0.039)	(0.026)	(0.012)	(0.020)	(0.009)
$m *$ (Married $*$ female)	0.146	−0.026	0.019	0.004	−0.090	−0.036
	(0.114)	(0.082)	(0.032)	(0.021)	(0.027)	(0.021)
Kid $*$ female	−0.362	−0.064	−0.006	−0.013	0.016	0.010
	(0.090)	(0.036)	(0.026)	(0.011)	(0.021)	(0.008)

Table A2 (continued)

	log(earnings)		Pr(idle)		Pr(welfare)	
	OECD	Im 75/80	OECD	Im 75/80	OECD	Im 75/80
$m*$(Kid $*$ female)	0.081	−0.217	−0.011	−0.004	−0.018	−0.012
	(0.114)	(0.078)	(0.032)	(0.021)	(0.028)	(0.020)
Education (missing and <9 years, reference)						
9–10 years	−0.035	−0.034	−0.207	−0.094	0.034	−0.014
	(0.072)	(0.030)	(0.020)	(0.009)	(0.017)	(0.007)
$m*$(9–10 years)	0.105	0.104	0.101	−0.013	−0.057	−0.009
	(0.089)	(0.061)	(0.024)	(0.016)	(0.022)	(0.015)
High school $\leq$ 2 years	0.089	0.114	−0.264	−0.142	−0.015	−0.033
	(0.064)	(0.026)	(0.019)	(0.007)	(0.015)	(0.006)
$m*$(High school $\leq$ 2 yrs)	0.097	0.072	0.088	−0.034	−0.081	−0.063
	(0.079)	(0.054)	(0.022)	(0.014)	(0.020)	(0.014)
High school $>$ 2 years	0.184	0.155	−0.265	−0.165	−0.069	−0.075
	(0.072)	(0.032)	(0.020)	(0.009)	(0.015)	(0.007)
$m*$(High school $>$ 2 yrs)	0.027	0.056	0.112	0.012	−0.013	−0.007
	(0.088)	(0.060)	(0.024)	(0.016)	(0.020)	(0.015)
University $<$ 3 years	0.210	0.339	−0.323	−0.213	−0.076	−0.093
	(0.075)	(0.031)	(0.020)	(0.009)	(0.015)	(0.015)
$m*$(University $<$ 3 yrs)	−0.013	−0.142	0.089	−0.020	−0.027	−0.010
	(0.093)	(0.063)	(0.024)	(0.016)	(0.021)	(0.016)
University $\geq$ 3 years	0.577	0.634	−0.319	−0.245	−0.108	−0.119
	(0.071)	(0.031)	(0.019)	(0.009)	(0.013)	(0.006)
$m*$(University $\geq$ 3 yrs)	−0.094	−0.151	0.096	0.022	−0.066	−0.055
	(0.092)	(0.066)	(0.024)	(0.017)	(0.019)	(0.015)
Country of origin dummies	Yes	Yes	Yes	Yes	Yes	Yes
$m*$(Country of origin dummies)	–	Yes	–	Yes	–	Yes
R-squared	0.12	0.10	0.16	0.14	0.14	0.11
# individuals	13,187	42,587	18,279	54,448	18,279	54,448

Notes: Parameter estimates. Heteroscedasticity consistent standard errors in parentheses. m is a dummy for being among the treated or the control group. p is a dummy for the post-reform period. See Table A1 for descriptive statistics.

Table A3. Estimates of the coefficients on individual variables in Eq. (6)

	(1) log(earnings)	(2) Pr(idle)	(3) Pr(welfare)
Female	−0.000	0.006	0.081
	(0.073)	(0.016)	(0.017)
Age	0.062	−0.017	−0.015
	(0.022)	(0.005)	(0.005)
Age squared ($*10^{-2}$)	−0.001	0.000	0.000
	(0.000)	(0.000)	(0.000)
Married	0.220	−0.075	−0.038
	(0.070)	(0.016)	(0.017)
Kid	0.002	−0.045	0.118
	(0.068)	(0.016)	(0.017)
Married * female	−0.084	0.025	−0.096
	(0.091)	(0.021)	(0.022)
Kid * female	−0.332	−0.025	−0.010
	(0.092)	(0.022)	(0.022)
Education (missing and <9 years, reference)			
9–10 years	0.054	−0.110	−0.037
	(0.065)	(0.014)	(0.014)
High school ≤ 2 years	0.206	−0.182	−0.109
	(0.066)	(0.015)	(0.015)
High school > 2 years	0.201	−0.156	−0.098
	(0.067)	(0.015)	(0.015)
University < 3 years	0.135	−0.254	−0.116
	(0.072)	(0.016)	(0.016)
University ≥ 3 years	0.496	−0.244	−0.188
	(0.075)	(0.017)	(0.018)
Immigration year (1987, reference)			
1988	−0.049	0.037	0.013
	(0.048)	(0.011)	(0.012)
1989	−0.017	0.062	0.082
	(0.048)	(0.011)	(0.012)
Country of origin dummies	Yes	Yes	Yes
R-squared	0.11	0.14	0.14
# individuals	6,418	9,883	9,883
# municipalities	168	168	168

Notes: Instrumental variables estimates. Heteroscedasticity consistent standard errors in parentheses. See Table A1 for descriptive statistics.

Endnotes

[1] Until the last few years, Israeli authorities located immigrants outside the major cities. Recently the policy has changed from one of deliberate placement to one of encouragement to settle in development towns; see Hiltermann (1991) for details on the Israel policy toward Russian immigrants. US immigration authorities distribute refugees through private organizations that arrange housing; the dispersion of immigrants across the US is not an explicit objective, however (Borjas 2000a). Belgium is another country where restrictions are imposed on the residence of new immigrants.

[2] Borjas (1995), Cutler and Glaeser (1997), and Katz et al. (2001) are other examples of attempts to handle the sorting bias.

[3] Immigrants arriving prior to the reform were eligible for welfare after receiving their residence permits. The difference here is that practically all immigrants were placed on welfare for an initial period that in many cases appears to have been considerably longer than 18 months.

[4] The main sources for the material presented in Sects. 2.1 and 2.2 are The Committee on Immigration Policy (1996), The Immigration Board (1997), and interviews of placement officers and other officials of the Immigration Board. We conducted the interviews in order to get information about how asylum reception worked in practice.

[5] The absolute number of quota refugees was roughly constant during the 1980s; thus, its share of the total refugee inflow decreased when immigration rose. In the fiscal year 1982/83, quota refugees made up 25% of the inflow (Ministry of Labor 1983); in 1987 their share of the inflow had declined to 10%.

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

[7] From then on more than 50% of the immigrants have used this opportunity. The Immigration Board has placed the remainder of the immigrants.

[8] 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.

[9] We provide more details substantiating this argument in Edin et al. (2003).

[10] See, e.g., Heckman et al. (1999) for an overview of different approaches to the evaluation problem.

[11] The third component relates to the differences in the returns to regional characteristics; the fourth can be labeled the initial distribution component; and the fifth has to do with the change in regional characteristics. One can also argue that the evaluation of the policy success should include the change in regional characteristics (the fifth component). To our minds, however, the effect of the policy on the characteristics of the region is likely to have been of minor importance.

[12] Notice that we have already imposed conditions (i) and (ii) when deriving Eq. (3). Conditions (iii) and (iv) are necessary to get from Eq. (3) to Eq. (4).

[13] This implies that we cannot separate between quota refugees and refugees applying for asylum when arriving in Sweden. Therefore, we evaluate these two groups together. In connection to this notice that: (i) quota refugees constituted a small and declining share of the total inflow; (ii) the policy reform presumably changed the reception of quota refugees as well, although arguably to a lesser extent; and (iii) if anything we probably underestimate the impact of the reform since we cannot separate between the two groups.

[14] See Edin et al. (2000b) for a more detailed discussion.

[15] Since this group has been in Sweden for a minimum of nine years before we measure outcomes, we do not expect assimilation between 89–91 and 95–97 (when we measure outcomes) to bias the estimated time effects. Results in Edin et al. (2000c) show that most of the economic assimilation of immigrants occurs within a few years after arrival. According to their results, 79% of the earnings growth observed in the first ten years among non-OECD immigrants occurs between the first and fourth year; less than 2% of the growth comes between the seventh and the tenth year. For a discussion of immigrant assimilation out of welfare, see Hansen and Lofstrom (1999).

[16] In the beginning of the 1980s the probability of residing in the Stockholm metropolitan area is 47% for recent arrivals of refugee immigrants; it is 46% for refugees arriving during the second half of the 1970s, 42% for OECD immigrants, and only 15% for low-skilled natives. To some extent we could correct for these discrepancies by reweighing the data such that the native comparison group to conform to the geographic distribution of refugee immigrants.

[17] We have also investigated whether the size of the inflow affects outcomes more formally by dropping individuals arriving in 1989 from the treated. The total inflow in 1989 was 77 (54)% larger than in 1987 (1988). The difference-in-differences estimates of the total effects that we are about to present are slightly smaller with this restriction.

[18] The estimates are available on request.

[19] The point estimates of the overall effects increase by about 0.02 for earnings. The estimates for idleness and welfare change by less than 0.01. The estimates are available upon request.

[20] Excluding municipalities resulting from splits during the relevant time period, the maximum number of municipality effects we could have estimated would have been 278. The minimum limit of at least 5 resident immigrants reduces the number of estimable municipality effects to 167. In principle, we also require that 1 assigned individual should remain in the municipality in question; otherwise the assigned municipality will have no predictive power in the first stage regression. The weight given to municipalities with few observations is relatively low; 11% of the sample lived in a municipality with less than 20 observations.

References

Bertrand B, Luttmer EFP, Mullainathan S (2000) Network Effects and Welfare Cultures. *Quarterly Journal of Economics* 115(3):1019–1055

Borjas GJ (1995) Ethnicity, Neighborhoods, and Human-Capital Externalities. *American Economic Review* 85(3):365–390

Borjas GJ (1998) To Ghetto or Not to Ghetto: Ethnicity and Residential Segregation. *Journal of Urban Economics* 44(2):228–253

Borjas GJ (1999) *Heaven's Door – Immigration Policy and the American Economy*. Princeton University Press, Princeton, NJ

Borjas GJ (2000a) Ethnic Enclaves and Immigrant Assimilation. *Swedish Economic Policy Review* 7(2):89–122

Borjas GJ (2000b) The Economic Progress of Immigrants. In: Borjas GJ (ed) *Issues in the Economics of Immigration*. University of Chicago Press, Chicago London, 15–49

Cutler DM, Glaeser EL (1997) Are Ghettos Good or Bad? *Quarterly Journal of Economics* 112(3):827–872

Dutch Refugee Council (1999) *Housing for Refugees in the European Union*. Dutch Refugee Council, Amsterdam

Edin P-A, Fredriksson P (2000) LINDA – Longitudinal INdividual DAta for Sweden. Working Paper 2000:19, Department of Economics, Uppsala University

Edin P-A, Fredriksson P, Åslund O (2000a) Ethnic Enclaves and the Economic Success of Immigrants: Evidence from a Natural Experiment. Working Paper 2000:21, Department of Economics, Uppsala University

Edin P-A, Fredriksson P, Åslund O (2000b) Settlement Policies and the Economic Success of Immigrants. Working Paper 2000:22, Department of Economics, Uppsala University

Edin P-A, Fredriksson P, Åslund O (2003) Ethnic Enclaves and the Economic Success of Immigrants: Evidence from a Natural Experiment. *Quarterly Journal of Economics* 118(1):329–357

Edin P-A, LaLonde RJ, Åslund O (2000c) Emigration of Immigrants and Measures of Immigrant Assimilation: Evidence from Sweden. *Swedish Economic Policy Review* 7(2):163–204

Hansen J, Lofstrom M (1999) Immigrant Assimilation and Welfare Participation: Do Immigrants Assimilate Into or Out-of Welfare. IZA Discussion Paper No. 100

Heckman JJ, Lalonde RJ, Smith JA (1999) The Economics and Econometrics of Active Labor Market Programs. In: Ashenfelter O, Card D (eds) *Handbook of Labor Economics* vol. 3A. North-Holland, Amsterdam, 1865–2097

Hiltermann JR (1991) Settling for War: Soviet Immigration and Israel's Settlement Policy in East Jerusalem. *Journal of Palestine Studies* 20(2):71–85

Ihlanfeldt KR, Sjoquist DL (1998) The Spatial Mismatch Hypothesis: A Review of Recent Studies and Their Implications for Welfare Reform. *Housing Policy Debate* 9(4):849–892

Kain JF (1992) The Spatial Mismatch Hypothesis: Three Decades Later. *Housing Policy Debate* 3(2):371–460

Katz LF, Kling JR, Liebman JB (2001) Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment. *Quarterly Journal of Economics* 116(2):607–654

Lazear EP (1999) Culture and Language. *Journal of Political Economy* 107(6):S95–S126

Musterd S, Ostendorf W, Breebaart M (1997) Segregation in European Cities: Patterns and Policies. *Tijdschrift voor Economische en Sociale Geografie* 88(2):182–187

Ministry of Labor (1981) *Ett lokalt omhändertagande av flyktingar*. Ds A 1981:11, Liber, Stockholm

Ministry of Labor (1983) *Genomförande av ändrad statlig ansvarsfördelning inom flyktingomsorgen*. Ds A 1983:10, Åbergs, Stockholm

The Committee on Immigration Policy (1996) *Sverige, framtiden och mångfalden*. Slutbetänkande från Invandrarpolitiska kommittén, SOU 1996:55, Fritzes, Stockholm

The Immigration Board (1997) *Individuell mångfald: Invandrarverkets utvärdering och analys av det samordnade flyktingmottagandet 1991–1996*. Statens invandrarverk, Norrköping

UNHCR (2000) *Refugees and Others of Concern to UNHCR – 1999 Statistical Overview*. Registration and Statistical Unit, Programme Coordination Section, United Nations High Commissioner for Refugees, Geneva