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ABSTRACT

Settlement Policies and the Economic Success of Immigrants*

Many developed countries, e.g. the UK, Germany and Sweden, use or have used settlement policies to direct the inflow of new immigrants away from immigrant-dense metropolitan areas. We evaluate a reform of Swedish immigration policy that featured dispersion of refugee immigrants across the country, but also a change in the approach to labour market integration. We focus exclusively on how immigrants fared because of the policy. The results indicate that immigrants experienced fairly substantial long-run losses because of the policy. We also find that only a smaller share of this effect was associated with the dispersion of immigrants across regions. The larger share of the impact appears to stem from a common component that affected immigrants regardless of where they were located. Our somewhat speculative reading of this result is that it can be traced to a shift in emphasis of integration policy from a policy focusing on labour market assimilation to one of income support.

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NON-TECHNICAL SUMMARY

The past couple of decades have seen an acceleration of immigration to developed countries. Concomitantly, there has been a shift in the ethnic composition of the immigrant inflow and an associated decline in the labour market performance of recently arrived immigrants compared to natives in these countries. 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. The settlement policies we observe across countries often feature an attempt to reduce immigrant concentration in metropolitan 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 at which immigrants acquire skills specific to the host country. 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 1985, the Swedish Immigration Board took over the responsibility for refugee reception from the Labour Market Board and a new system for refugee reception was introduced. Under the new policy, immigrants could no longer choose their initial place of residence after receiving a residence permit; instead, they were provided with housing in a municipality assigned by the government. Another aspect of the reform was a shift in the focus of integration policy – away from labour market integration to an increased reliance on income support. In the new system, refugee immigrants were granted welfare for an introductory period of at least 18 months.

This study examines how labour market outcomes were affected by the policy. We focus exclusively on individuals subjected to the policy and concentrate on the long-run effects. We ask two principal questions: (i) what was the total effect of the reform? (ii) How much of the total effect can be attributed to the change in the geographic distribution of immigrants?

To answer the first question, we need to control for overall time effects. The period when we measure outcomes was a very turbulent time in Sweden; the aggregate unemployment rate went from less than 2% to more than 9% between 1989 and 1993. To handle the time effects, we use a difference-in-difference approach, where the difference in outcomes between individuals subjected to the policy and refugee immigrants arriving prior to the reform is related to changes in a suitable comparison group.

To answer the second question, we need estimates of how different regions affect outcomes. Due to sorting problems, we cannot (in general) consistently estimate the impact of regional characteristics. We use an instrumental variables approach where we exploit the assignment of program participants to municipalities as an exogenous source of variation when estimating the impact of regions.

Our results suggest that the policy adversely affected outcomes in the longer run. Eight years after immigration, we find that the policy reduced earnings by 25% (among those who had earnings), increased the rate of idleness (i.e. people neither working nor studying) by one-third, and led to a 40% rise in welfare receipt.

When we decompose the effect of the reform into a common part and a part connected with the dispersion across regions, we find that less than half of the long-run effect originated in the dispersion component. Had people stayed on in their assigned municipalities, however, the outcomes would have been substantially worse. Because programme participants relocated to better municipalities, many of the adverse effects were avoided. We find some indications that the effects of the distribution component of the policy can be traced to placement in regions with high unemployment and absence of ethnic networks.

Most of the adverse impact thus came from a common component that affected people regardless of where they were located. A somewhat speculative reading of this result is that the weaker link between refugee reception and integration into the labour market, and increased focus on income support, had long-lasting negative effects on individual outcomes.

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

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 composition of the immigrant inflow and an associated decline in the labor market performance of recently arrived immigrants compared to natives in these countries. 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.*, 2000). 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 at which immigrants acquire skills specific to the host country. The second argument is based on the perception that immigrants impose a fiscal and social burden on the host

[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 some kind of restrictions are imposed on the residence of new immigrants.

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.

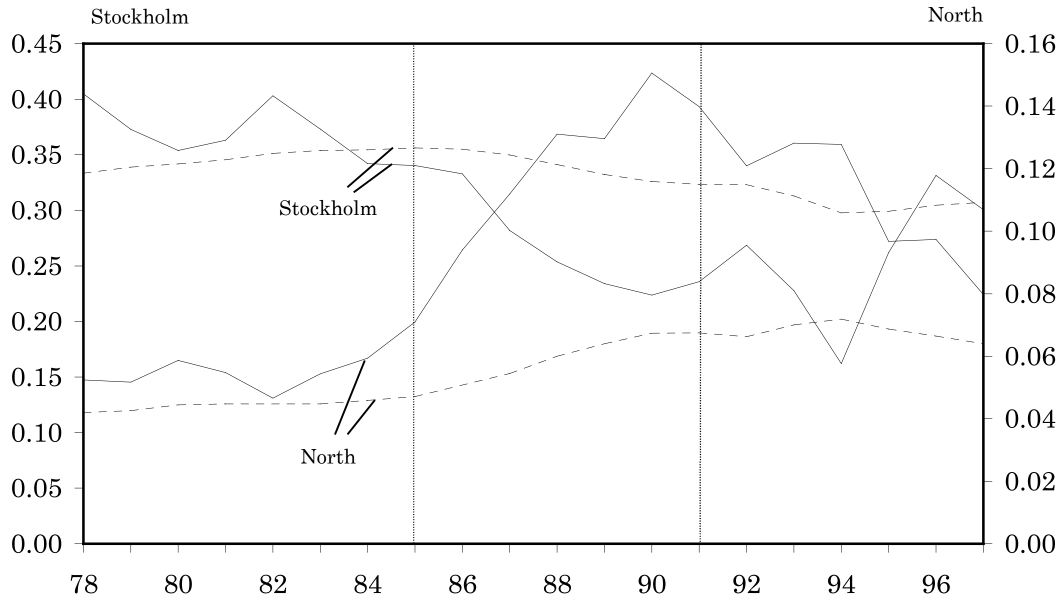
Immigrant enclaves are not necessarily harmful for immigrants living there. The enclave may present the immigrant with more opportunities for gainful "trade" in the goods and labor markets; e.g. Lazear (1999). Also, ethnic neighborhoods may constitute an environment where the immigrant is exposed to 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* is another issue, since the majority of these studies take local characteristics as exogenous. Such an approach is, of course, plagued by Tiebout sorting bias. In related work we have examined the severity of this problem; see Edin *et al.* (2000a).[2] We used the settlement policy as an exogenous source of variation arguing that initial placements were independent of unobserved individual characteristics. We found that there was 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 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.

[2] Borjas (1995), Cutler and Glaeser (1997), and Katz *et al.* (2000) are other examples of attempts to handle the sorting bias.

For a settlement policy to have real effects in the long run, the long-run distribution of immigrants in the receiving country must be affected. The Swedish policy had real consequences for immigrant location, as illustrated in Figure 1. It plots the share of the immigrant inflow and stock residing in Stockholm and the North of Sweden respectively. As indicated by the vertical dashed lines, the policy was viable between 1985 and 1991. Prior to 1985, most immigrants were essentially free to choose a region of residence. In the mid 1980s there was a drastic shift of the inflow towards the North. More importantly, the stocks were also affected. The share of this group of immigrants living in the north of Sweden increased from 5 percent in 1985 to reach 7 percent in 1991. The mirror image of this development is a reduction of the share living in Stockholm (from 36 percent in 1985 to 32 percent in 1991). Thus, the policy initiative clearly increased the dispersion of immigrants across the country.

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.

There was more to the policy shift than inducing an increase in 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 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 fairly substantial long-run earnings losses. Among those with earnings, we estimate an earnings loss of about 25 percent for those subjected to the policy. In addition, idleness increased by one third, and welfare receipt by approximately 40 percent. 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, in the longer run 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 section 3, we present the evaluation framework. We think of an individual's outcome as being determined by a common effect and an effect derived from the characteristics of the region. Section 4 turns to the empirical analyses. We use longitudinal micro data derived from the database LINDA (see Edin and Fredriksson, 2000) and focus on three outcomes: earnings, idleness, and welfare dependency. We begin by estimating the overall effect of the policy without conditioning on regional characteristics. We then estimate the causal effect of regional characteristics, by using the quasi-experiment provided by the assignment policy. These estimates allow us to get at the component of the policy stemming from the fact that individuals were exposed to different regional environments. Section 5 offers concluding remarks.

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

2. The institutional setting

This section has two objectives. First, we describe the major institutional changes that were implemented in 1985 when the settlement policy was introduced. Second, we ask the question: Can we regard the settlement policy as a natural experiment?

2.1 An overview[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 state of affairs meant that there was a natural focus on labor market integration and immigration policy was linked to labor market policy.

Immigration for labor market reasons from non-Nordic countries virtually ceased in the mid 1970s. Since then immigration from these countries is mainly for political reasons. Partly as a consequence of this change, 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.

Before the reform, a majority of refugee immigrants traveled directly to a municipality and applied for asylum there. Remaining refugee immigrants were so called quota refugees who stayed in 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, Göteborg or Malmö (Ministry of Labor, 1981, 1983).

After the reform, all refugee immigrants, apart from reunification immigrants, were subjected to the settlement policy. 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.

At first the intention was to sign contracts with 60 municipalities (out of Sweden's 284 municipalities) that had suitable characteristics for reception, such as educational

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

[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%.

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 standard 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 1992–94, 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.

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[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 percent of the immigrants have used this opportunity. The Immigration Board has placed the remainder of the immigrants.

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 1987–91, 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.

The municipal placement policy is an obvious difference between the pre- and post-reform periods. Other differences may be harder to pinpoint, but are important for understanding the overall effect of the program.[9] Our reading of the facts is that the reform shifted the policy focus away from labor market assimilation to an increased reliance on income support. Before the reform, employment officers were responsible for the arrangements concerning housing, education, and jobs. After the reform, ”refugee coordinators”, employed by the municipalities, handled these responsibilities. The direct link to the employment offices providing job search assistance, subsidized employment, or training programs were thus broken. In practice, there was insufficient cooperation between the coordinators and the employment offices. Partly as a result of this, the transition from basic language training, via additional courses in Swedish, to labor market training or a job, became less smooth and contained longer spells of inactivity. In a similar vein, the integration into the labor market was postponed relative to the time of arrival. Individuals arriving after the reform were granted (or subjected to) a long initial period (at least 18 months) of welfare receipt. This period was much shorter for immigrants arriving in previous years: quota refugees spent 4–6 months in the center before moving to a municipality to work or participate in labor market training; immigrants who had traveled directly to a municipality waited 2–3 months after receiving their residence permit before moving on to work or training.[10]

[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] Our information about the pre-reform period comes mainly from Ministry of Labor (1981, 1983) and officials at the Immigration Board.
[10] Quota refugees living in centers did not receive individual social assistance; instead, the Labor Market Board paid for costs of living. Immigrants living outside the centers received social assistance from the municipalities. The central government then reimbursed municipalities for the amount paid plus a standard addition of 25 percent of the amount to cover costs for public services.

To conclude, we think it is fair to say that 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.

2.2 Can the settlement policy be regarded as a natural experiment?

In a companion paper (see Edin *et al.* (2000a)) we argued that the settlement policy provides an exogenous source of variation that identifies the causal effect of regional characteristics.

If the policy is to provide a quasi-experiment, actual placement should be independent of any *unobservable* characteristics in the outcome equations. To what extent is this true? Were some individuals more likely placed than others? To answer these questions we interviewed placement officers and other officials of the Immigration Board. The following is a description of the practical implementation of the policy.

An asylum seeker was placed in a refugee center while waiting for a decision from the immigration authorities. Refugee centers were distributed all over Sweden, and the port of entry did not influence to which center the individual was directed.

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). 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 selected the "best" immigrants. Notice that there was no interaction between municipal officers and refugees. The selection was, hence, 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.

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 can potentially be problematic. Cream-skimming on the part of municipal officers suggests that high-skilled may have been able to realize their preferred option.[11]

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 estimating the parameters of interest. The basic problem is that we think that location matters for outcomes, but due to sorting we cannot consistently estimate 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 basic approach is to combine a difference-in-difference 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. To simplify the exposition, we introduce the following assumptions. First, let the

[11] In Edin *et al*. (2000a), 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.

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.[12]

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 equation (1) is complicated by the fact that individuals choose where to reside. In Edin *et al.* (2000a), 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.

Let us pool the two cross sections by defining the dummy variable p such that $p = 1$ if $\tau = p$

$$\ln y_i^m = a^m + \phi^m p_i + \kappa_1^m d_i + \kappa_2^m p_i d_i + \alpha_i^m \tag{2}$$

where $\phi^m = [a_p^m - a_0^m + E(\alpha_{ip}^m) - E(\alpha_{i0}^m)]$. If we have some exogenous variation identifying the coefficients on d, and assuming that the two cohorts are generically of equal mean ability ($E(\alpha_{ip}^m) = E(\alpha_{i0}^m)$), one can in principle use equation (2) to evaluate the settlement policy. The estimate of ϕ^m would then capture a common program effect.

The simple before-and-after estimator outlined in (2) rests on the implausible assumption that there are no time effects. This is a particularly untenable assumption given the time period that we are considering. We measure the outcomes in 1995–97 for those subjected to the policy, and in 1989–91 for pre-reform immigrants. In the beginning of the 1990s the unemployment rate in Sweden skyrocketed from less than 2

[12] In Edin *et al.* (2000a), we compared an IV estimator and a control function estimator. The results were very similar across the two approaches, suggesting either that the return is constant across individuals, or that individuals do not make location decisions based on the individual return or information correlated with it.

percent in 1989 to over 9 percent in 1993. Suppose that there exists a proper comparison group (superindexed *c*) in the sense that the time effect is the same for this group as for refugee immigrants, and assume for now that there is no change in mean unobservable ability over time. Define a dummy *m* equaling unity if the individual is a refugee immigrant and pool the two groups into a single regression

$$\ln y_i = a + \lambda_1 m_i + \lambda_2 p_i + \pi p_i m_i + \mu_1 d_i + \mu_2 m_i d_i + \mu_3 p_i d_i + \mu_4 p_i m_i d_i + \alpha_i \tag{3}$$

The crucial parameter in (3) is the coefficient on $p_i m_i$, i.e., the difference-in-difference estimator $\pi = (a_p^m - a_0^m) - (a_p^c - a_0^c)$, which gives the common program effect if the comparison group properly identifies the pure time effect $(a_p^c - a_0^c)$.

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

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

where Δ is the difference operator, and d^k reflects the distribution across regions in group *k*. 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 (4) 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, which simply equals π in equation (3), 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 equation (4).

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.[13]

In the following proposition, we detail the conditions that the comparison group must satisfy in order for the "raw" difference-in-difference 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 common time effect should be the same; (*ii*) the changes in unobservable ability (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 estimator in (4) simplifies to

$$\delta = \left((a_p^m - a_0^m) - (a_p^c - a_0^c)\right) + \beta^m z_p[\Delta d^m - \Delta d^c] = \pi + \gamma[\Delta d^m - \Delta d^c] \qquad (5)$$

From a practical point of view the most important obstacles are conditions (*i*) and (*iv*). 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 (4). 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 mainly focus on these two conditions. However, we also discuss whether equality of the return to regional characteristics – condition (*iii*) – is a reasonable assumption. Moreover, in the empirical

[13] One can also argue that part of the fifth component should be included in an evaluation of the policy success; to our minds, however, the effect of the policy on the characteristics of the region is likely to have been of minor importance.

analysis we consider problems caused by differential changes in unobservable cohort quality – condition (*ii*).

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 γ. 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, equation (5) and the criteria for identifying it, will guide our choice of empirical strategy and comparison group.

4. Empirical analysis

We begin the empirical analysis by describing the data, and the choice of counterfactual and comparison group. Then we take a brief look at the impact the reform had on the geographic distribution and internal mobility of refugee immigrants. After that we proceed to the evaluation of the reform in terms of economic outcomes of the participants. First, we investigate the overall effect of the policy, and then we decompose this effect into a common effect and a dispersion effect.

4.1 Data and sample selection

The empirical analysis is based on data from the LINDA database. LINDA contains two panels: one of around 20 percent of the foreign-born population, and another of approximately 3 percent 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. 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 immigrants during the period. Furthermore, persons belonging to a household with either a Swedish-born grown-up or a person who immigrated in a previous year were excluded, 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 1987–89, when placement rates were of the order of 90 percent.

4.2 The choice of counterfactual and comparison 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 1981–83.

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 gender.[14] In Tables 1 and 2, we compare the cohorts in terms of education and ethnicity. Table 1 begins by tabulating immigrants by educational level. While about 30 percent of the "pre-program" cohort has a short high school education, the program cohort is more evenly spread out across educational levels. To us these differences do not raise great concern, however. In fact, if we impute years of education the cohorts are very similar; the 1987/89 cohort has 0.2 additional schooling years on average.

The difference between the two cohorts in terms of ethnicity is a greater source of concern; see Table 2. 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 in the Middle East.[15]

[14] The representative individual of the 1987/89 cohort was 0.58 years older and the probability of being male was 0.34 percent greater in comparison to the average individual in the 1981/83 cohort.
[15] 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.

Table 1: Immigrant cohort by education, percent.

	Immigrant cohort	
	1981/83	1987/89
Missing	8.7	4.9
< 9 years	17.1	16.5
9–10 years	10.8	18.5
High school ≤ 2 years	28.8	16.8
High school > 2 years	13.5	18.5
University < 3 years	10.2	13.4
University ≥ 3 years	10.9	11.4
# individuals	2,679	9,883

Notes: Refugee immigrants aged 18–55 at immigration. 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.

Table 2: Immigrant cohort by region of origin, percent.

	Immigrant cohort	
	1981/83	1987/89
Eastern Europe	37.3	18.0
Africa	9.2	11.6
Middle East	23.3	45.7
Asia	14.2	8.3
South America	16.0	16.4
# individuals	2,679	9,883

Notes: Refugee immigrants aged 18–55 at immigration. Measured 8 years after immigration.

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 *location differences* across cohorts in the sequel, we refer to the differences between the 1987/89 cohort and the weighted 1981/83 cohort.

On the more difficult question of whether unobserved heterogeneity will bias our policy evaluation, these aggregate statistics, of course, provide limited information. However, it is reassuring that formal education is so similar across the two cohorts.

The choice of comparison group

In section 3 we identified properties that a comparison group must have to make a meaningful analysis possible. Our strategy is to consider the following comparison groups:

1. Natives
2. Low-skilled natives
3. Young and less educated natives
4. OECD immigrants, 81/83 and 87/89
5. Non-OECD immigrants, 1975/80

The suitability of these groups depends on how well they conform to the conditions described in the proposition of section 3.

Let us start with condition (*i*): a common time effect. This property is vital in order 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. There is reason to believe that the first group (natives) may not experience the same time effects as refugee immigrants; however, we include them as a reference case.

The second and third groups are obtained by restricting the native population in the observable skill dimension. Low-skilled natives are defined as the lowest quartile of the predicted outcome distribution.[16] Young and less educated natives are defined as those who are between 20 and 30 years of age, and have completed less than 11 years of schooling. These two groups at the lower end of the skill distribution may experience similar responses to macro fluctuations as refugee immigrants.

Our fourth potential comparison group is immigrants from the OECD countries arriving in the same time periods as the treated and counterfactual groups. This is the proper comparison if the susceptibility to macro shocks primarily is related to the time in the host country.

The fifth comparison group is based on the idea that immigrants from certain regions may fare worse than others during economic recessions. If this is the case, then the 1975/80 cohorts of non-OECD immigrants should be comparable with the sample of refugee immigrants. Results in Edin *et al.* (2000b) show that most of the economic assimilation of immigrants occurs within a few years after arrival.[17] Since this group

[16] We obtained the predictions by regressing the outcome variable on individual characteristics (see equation (6)) and time controls, and then predicting values using only individual variables.
[17] With an assimilation measure that corresponds to this comparison (allowing assimilation to be affected by return migration), 79 percent of the earnings growth observed in the first ten years among non-OECD immigrants occurs between the first and fourth year; less than 2 percent of the growth comes between the seventh and the tenth year.

has been in Sweden for a minimum of nine years before we first measure outcomes, we do not expect assimilation between 89–91 and 95–97 (when we measure outcomes) to bias the estimated time effects.

In the first panel of Table 3 we substantiate the discussion with some evidence. It presents estimates on time dummies for the years 1982 and 1983 (relative to 1981) in earnings regressions using refugee immigrants and comparison groups in the pre-reform period only. An ideal comparison group has time estimates equal to those in the immigrant group; the estimate on the interaction dummy *time*immigrant* is then zero. Although we cannot reject the hypotheses that the time effects are the same, it is clear that the estimates we get using the OECD or 1975/80 comparison group are closer to zero than those relating to natives, especially for 1983 when time effects are larger.

The fourth condition of the proposition stipulates that the distributions across regions prior to the reform should be equal. We investigate this issue in panel b of Table 3. The panel shows the percentage of the 81/83 immigrants and the comparison groups living in different regions of Sweden. The 1975/80 cohorts of immigrants appear to best meet the fourth criterion. OECD migrants also conform relatively well to refugee immigrants, whereas the distribution of natives is quite different.[18]

There are additional reasons to prefer the 1975/80 cohorts of non-OECD immigrants as the comparison. It is *a priori* plausible that this group best meets the third condition, i.e., there is equality of the return to local characteristics. In Edin *et al.* (2000a), we find that the presence of other immigrants affects outcomes. It is quite likely that this effect is different for natives and perhaps also for OECD immigrants. Relative to OECD immigrants, there is also the advantage of having a larger number of observations; see Table A1a. The 1975/80 immigrants are therefore our primary comparison group, but in some cases we present results using also the OECD group. Some results for the native comparison groups are provided in the appendix.

[18] Reweighing of the native comparison group to conform to the geographic distribution of refugee immigrants appears to be a possible alternative. Some experimentation along this line indicates that the estimated overall effect is very similar with original and reweighed data. However, since other arguments also favor the use of previous immigrant cohorts, we have not investigated this issue further.

Table 3: Suitability of comparison groups.

	(Refugee) Immigrants	Natives	Young and less educated natives	Low-skilled natives	OECD migrants	Immigrants 1975/80
Panel a: Estimated time effects.						
1982		–.013	–.027	–.016	–.012	–.019
		(.003)	(.007)	(.007)	(.061)	(.018)
1982*immigrant		–.001	.014	.002	–.001	.005
		(.040)	(.042)	(.046)	(.082)	(.056)
1983		–.060	–.151	–.061	–.122	–.131
		(.003)	(.007)	(.007)	(.062)	(.018)
1983*immigrant		–.067	.024	–.066	–.006	.003
		(.042)	(.044)	(.048)	(.085)	(.059)
Panel b: Regional distribution, percent						
Stockholm	46.77	18.29	17.09	14.97	42.23	45.68
Mid (east)	13.21	17.02	17.68	16.68	11.93	12.60
Southeast	4.03	9.21	9.43	10.83	4.45	5.14
South	17.54	14.13	13.66	14.76	11.21	15.17
West	14.33	19.79	20.10	20.82	17.66	17.54
Mid (north)	2.39	10.31	11.26	10.77	5.61	1.83
North (mid)	0.97	4.84	4.82	4.99	1.82	0.74
North (upper)	0.75	6.42	5.97	6.18	5.10	1.31

Notes: Panel a shows estimates from difference-in-difference estimations of log earnings. The dependent variable is earnings eight years after immigration. In addition to the time dummies (immigration year) and interactions time*immigrant, the regression includes individual characteristics; see equation (6). Panel b shows the pre-reform geographic distribution of refugee immigrants (81/83 cohorts) and the comparison groups. To facilitate comparison across comparison groups, the location distributions for refugee immigrants are not weighted to conform to the 87/89 region-of-origin distribution.

4.3 Mobility and concentration

Since the difference in mobility is one indicator of how refugee immigrants perceived the reform, this section compares post-immigration mobility across policy regimes. We base the comparison on a simple before-and-after approach.[19]

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 program cohort in comparison to the earlier cohort. Moreover, the economic consequences of the reform will of course depend on whether they stayed on in the assigned region.

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

[19] In related research we found no obvious time effects in migration and location patterns; see Åslund (2000).

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 not obvious.

Table 4: 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.

We start by comparing mobility across the two cohorts; see Table 4. Eight years after the immigration year, 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. However, regression based comparisons (controlling for gender, age, marital status, and region of origin) do not give strong support to changes in migration propensities (Åslund, 2000).

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 5. 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.

Table 5, which divides region of residence by 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 further look at the residential patterns suggests that mobility is not just from a desolated North to the populous South, but also to the regional centers in the north of Sweden.

Table 5: 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.

These findings show that there were no clear-cut differences in mobility between pre-reform immigrants and program participants. However, there was more concentration among immigrants who were assigned a municipality by government authorities. Two more things should be noted. First, the fact that there is also a lot of mobility in the cohort that was supposedly free to choose, suggests that informational problems may be of some importance. Second, even eight years after entry to Sweden, the post-reform distribution of immigrants has far from converged to the pre-reform distribution of immigrants.

The latter point implies that if there was an overall effect of the reform on economic outcomes, some of it could origin in the change in the long-run dispersion of refugee immigrants. Below, we first investigate the total effect, and then decompose it in order to determine the importance of the distribution component relative to the common component of the policy.

4.4 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-difference approach and relate the 87/89 and 81/83 immigrant cohorts to the comparison groups discussed in section 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.

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.

In the analysis, we focus on three measures of the outcome. The first measure is (the log of) annual earnings; the second is idleness (defined as neither having positive earnings nor being enrolled in education); the third is welfare receipt. Table A2 shows means and standard deviations of these variables eight years after arrival for both immigrant and comparison groups. Program participants have worse labor market outcomes on all accounts. Among those with earnings, people subjected to the policy earn about 60 percent less than previous immigrants. Further, idleness is almost 17 percentage points higher in the 87/89 than in the 81/83 cohort. However, there are clear changes in the earnings and employment variables in the comparison groups as well, particularly concerning idleness for OECD and 75/80 immigrants.

The difference-in-difference estimates of the total effects of the reform are derived from the following prototype specification:

$$outcome_i = \alpha + \boldsymbol{\beta}_0'\mathbf{X}_i + \boldsymbol{\beta}_1'\mathbf{X}_i m_i + \phi_0 m_i + \phi_1 p_i + \delta m_i p_i + \varepsilon_i \tag{6}$$

where $outcome_i$ is the outcome in $t+8$ for individual i. $\mathbf{X}_i$ is a vector of individual characteristics (gender, age, age squared, marital status, level of education, and region of origin), m_i is a dummy variable for refugee immigrants, and p_i is a dummy for the reform period. In this setup, δ is the parameter of primary interest; it is the difference-in-difference estimator of the overall effect of the new policy. To simplify the decomposition of the overall effect, we use the linear probability model for binary outcome variables.

Table 6: Difference-in-difference estimates with alternative comparison groups.

	Program effect δ	Time effect ϕ_1	# individuals	R-squared
Log earnings				
OECD	–.331	–.164	13,187	.11
	(.047)	(.036)		
Immigrants 75/80	–.263	–.233	42,587	.08
	(.032)	(.013)		
Idleness				
OECD	.066	.082	18,279	.15
	(.013)	(.010)		
Immigrants 75/80	.056	.092	54,448	.12
	(.009)	(.004)		
Welfare				
OECD	.085	–.005	18,279	.12
	(.013)	(.009)		
Immigrants 75/80	.101	–.021	54,448	.10
	(.010)	(.003)		

Notes: OLS parameter estimates, standard errors in parentheses. The regressions also include immigrant status, gender, age, age squared, level of education, region 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 immigrant status and remaining individual characteristics. For details on the different samples, see Tables A1a–c. Results using other comparison groups are presented in Table A3.

Table 6 shows the results for the three outcomes, using two different comparison groups. Starting with earnings, we find that the effects are quite large; according to the estimates, program participants with earnings eight years after arrival suffered approximately a 25-percent loss in earnings due to the policy.

Furthermore, the policy increased the likelihood of being idle. The estimates imply that the probability of idleness is about 5–7 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 9–10 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 more than 40 percent. Note, though, that there is a negative time trend for 75/80 immigrants. This may seem surprising, given the increase in households receiving social assistance in the first half of the 1990s (Arslanogullari, 2000). According to Hansen and Lofstrom (1999), immigrants in Sweden assimilate out of the welfare system; the trend in the 75/80 group may be explained by the fact that this group has spent more time in

Sweden in the 87/89 period.[20] If we relate welfare receipt to natives, the program effects are smaller but still significant; see Table A3.

Supported by Table 3, we believe that the two comparison groups used in Table 6 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 and time effects. To create a "lower bound" for the estimates, we look at the two less skilled native groups; the results are found in Table A3. The parameter estimates are highly significant for all outcomes, although smaller when we use the group of young and less educated. Moreover, we have performed a similar operation on the main comparison groups (OECD and 75/80), where we excluded the quartile predicted to have the best outcomes; the results from these exercises are very similar to those in Table 6.

According to the second condition of the proposition, the change in unobservable ability (cohort quality) over time must be the same among refugee immigrants and the comparison group. Can the results of Table 6 be rationalized by declining cohort quality?[21] Declining cohort quality may be a problem if the data are not sufficiently rich in terms of individual characteristics and the specification is not flexible enough. According to Borjas (2000b), national origin is *the* crucial factor behind changes in cohort quality in the US. Notice in this respect that our findings are robust to substituting the region of origin dummies with controls for country of origin. We have also interacted all individual variables with region of origin, so that e.g. the returns to education are allowed to vary between people from different regions; again, the results do not change.[22]

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,

[20] Differing age structure is another potential explanation (if its effect is not fully captured by the linear and quadratic terms). We have experimented with regressions where data were weighted according to the age structure of the 87/89 refugee immigrants. The results are in general unaffected.
[21] Nonrandom emigration of the 1975/80 immigrants could potentially be a problem. However, results in Edin *et al.* (2000b) show that most emigration occurs shortly after arrival.
[22] We have also estimated the model separately for people from different countries. Not surprisingly there are some variations in the results; however, the negative impact of the program always remains.

this holds in instances when we use comparison groups that we expect (and find) to suffer severely during the recession in the early 1990s. Therefore, our conclusion is that the policy change adversely affected the economic outcomes of refugee immigrants.

4.5 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 equation (5), 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, our strategy is to estimate versions of the following regression for the 1987/89 cohort:

$$outcome_{ij} = \boldsymbol{\beta}'\mathbf{X}_i + \sum_j \gamma_j d_{ij} + \varepsilon_{ij} \tag{7}$$

where *outcome* denotes either log earnings, the probability of being idle, or welfare dependency, **X** is a vector of individual characteristics (gender, age, age squared, marital status, level of education, region of origin, and immigration year), and d_{ij} is a dummy variable for residing in municipality *j*. All variables included in equation (7) 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, γ_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.[23]

[23] 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 percent of the sample lived in a municipality with less than 20 observations.

We ask two questions in this part of the analysis. First, what part of the difference-in-difference in outcomes originates in the change of geographic distribution? The answer to this question gets at the distribution component of equation (5). 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_{jt}^{m} be the probability of residing in municipality *j* in time period *t* (0 if pre-reform, *p* if reform) for an individual belonging to the refugee immigrant population (*m*). Let d_{jt}^{c} be the analogous probability for the comparison group (*c*). Our measure to answer the above questions is then

$$\sum_j \left((d_{jp}^{m} - d_{j0}^{m}) - (d_{jp}^{c} - d_{j0}^{c}) \right) \hat{\gamma}_j \qquad (8)$$

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 counterfactual groups have been eight years in Sweden; the difference-in-difference between the weighted averages is then the distribution component of equation (5). Second, we use the initial distributions, i.e., those that correspond to the time of arrival for the treated and counterfactual 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 7: How the placement affected outcomes, standard errors in parentheses.

	(1) log (earnings)		(2) Pr(idle)	(3) Pr(welfare)
	(a)	(b)		
Distribution component of difference-in-difference estimate (equation (5)). Distribution eight years after immigration.	–.0630 (.0356)	.0957 (.0587)	.0295 (.0134)	.0379 (.0149)
Distribution component if people had stayed in the assigned municipalities. Initial distribution.	–.4956 (.2770)	–.2240 (.2883)	.1975 (.0950)	.2745 (.1057)

Notes: The calculations use the difference-in-difference of distributions, with 75/80 immigrants as the comparison group. Estimates in column 1b control for regional ”price” dispersion as explained in the text. 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 A4. The estimates for idleness and welfare receipt were obtained by applying the linear probability model.

Table 7 presents the estimates; the calculations use 1975/80 immigrants as the comparison group (estimates on individual characteristics are presented in Table A4). For simplicity, the estimates for idleness and welfare receipt were obtained by applying the linear probability model. In column 1, we give the estimates on earnings.[24] There are two sets of estimates of the reform: column 1a has no control for regional price dispersion, while column 1b controls for the dispersion of regional house prices. The latter estimates were constructed by regressing the set of local effects $(\boldsymbol{\gamma})$ on a vector of house prices ($\mathbf{p}$). The difference-in-difference in distributions were then applied to the transformed local effects, $(\boldsymbol{\gamma}-\hat{\alpha}\mathbf{p})$ (where $\hat{\alpha}$ is the estimate from the above regression).

From the results in the first row, it seems that the distribution component accounts for a limited part of the overall effect of the policy on earnings. Without control for "prices", the estimates suggest a six percent decrease (t-ratio 1.77); with controls the estimate is positive but not significant.

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 (the t-ratio is 2.29); analogously, welfare receipt increased by 4 percentage points (t-ratio, 2.54).

Let us now turn to the second row and the comparison of initial location effects in the immigrant cohorts. Even though the standard errors are high, 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 39 percent (*t*-ratio, 1.79) lower than in the situation when they could freely chose place of residence; the estimated effect on ”real” earnings is a 20 percent (*t*-ratio, 0.78) loss.

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 20 percentage points higher (the *t*-ratio is 2.08) compared to the situation when they could freely choose place of residence.

[24] Using log earnings, of course, yields estimates that do not take behavior into account. The estimates of the impact of the reform are biased to the extent that selection affects the estimates of our location dummies. The reason for not estimating selection corrected earnings regressions is that it is difficult to come up with the exclusion restrictions necessary to produce credible estimates.

Column 3 shows that we get a similar result for welfare. The probability of being welfare dependent was 27 percentage points higher (*t*-ratio 2.60) 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. For example, the difference in idleness was only –0.037 (standard deviation 0.025) in the 81/83 cohort, and –0.202 (0.102) in the 87/89 cohort. For earnings, the corresponding figures were –0.006 (0.069) and 0.429 (0.301). 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 negative effect, and that the presence of other immigrants, particularly those from the own country, influences the outcome positively. Compared to the 81/83 cohort, the 87/89 immigrants were assigned to municipalities with lower representation of immigrants and higher unemployment. When we try to explain the estimated municipal effects with observable local characteristics, it turns out that the most significant effects come from municipal unemployment (negative) and the size of the group from the own country (positive).[25]

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.

4.6 Decomposing the overall effect

Equation (5) states that under certain conditions, the difference-in-difference in outcomes can be written

$$\delta = \Delta \ln y^m - \Delta \ln y^c = \pi + \gamma[\Delta d^m - \Delta d^c]$$

Since we have estimated the overall effect (δ) and the distribution effect (corresponding to the second term on the right-hand side), we can "back out" an estimate of the common effect of the reform (π). Throughout the decomposition analysis, we use the 1975/80 non-OECD immigrant cohorts as comparison.

Table 8 suggests that about half of the effect on idleness came from the common component of the reform, and the other half originated in the change in distribution over municipalities. For earnings and welfare receipt, the decomposition attributes a majority of the loss to the common component.

Table 8: Decomposition of the overall effect, 1975/80 immigrants comparison group.

	Total	Common	Distribution	Percentage on common part
Log Earnings	–.263	–.200	–.063	75.7
Idleness	.056	.026	.030	46.4
Welfare	.101	.063	.038	60.4

Notes: "Total" taken from Table 6 and "Distribution" from the first row of Table 7. "Common" is the difference between "Total" and "Distribution".

[25] We estimated $\gamma^j = \alpha + \beta_0 \log n^j_{t+8} + \beta_1 \log im^j_{t+8} + \beta_2 \log et^j_{t+8} + \beta_3 u^j_{t+8} + \varepsilon^j$, where γ^j is the estimated municipal effect (from the log earnings equation), *n* is the number of natives, *im* is the number of other immigrants, *et* is the number of people from the own country, and *u* is local unemployment. The regression includes 168 observations; the explanatory variables are municipal means (for program participants). This regression yields an R-square of about 15 percent, so the local characteristics included explain some but far from all of the difference in municipal effects.

Since the estimated total and distribution effects vary between comparison groups, the decomposition is somewhat sensitive to how we make the comparison. Some experimentation with alternative comparison groups reveal that the fraction attributed to the respective components is quite stable for earnings, but varies more for idleness and welfare. If we instead use natives as comparison, the percentage attributed to the common component is larger for idleness (72%), and smaller for welfare (38%).[26]

Our conclusion from this section is 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 as opposed to labor market policy, which had detrimental effects on people arriving under the new policy.

4.7 Is it possible to evaluate the reform?

There are two issues that could make evaluation of the reform difficult. They both relate to the unprecedented size of the inflow of refugee immigrants during the 87/89 period. While the placement policy was in place, the annual number of residence permits issued to refugees increased from levels of 4,000–6,000 before the reform, to a peak at almost 25,000 in 1989.

The first question is whether the municipal distribution of the 81/83 cohort is an appropriate counterfactual distribution. It is possible that the increased inflow of immigrants would have changed the distribution also in absence of the reform. We try to capture this by using the difference-in-difference between the distributions. The distribution of the 81/83 cohort plus the change within the group of 1975/80 immigrants is probably the best approximation we can get of what the distribution would have been if the program had not been implemented.

The second question is whether the size of the inflow itself affects the 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. We have investigated this issue further by dropping

[26] With the OECD comparison group, we in fact get negative estimates on the distribution component, which implies that the common effect is larger than the total effect; however, the small number observations is problematic for this group in this part of the analysis.

individuals arriving in 1989, when the total inflow was 77 (54) percent larger than in 1987 (1988). The difference-in-difference estimates of the total effects are slightly smaller with this restriction.[27]

We have also looked at whether the performance of refugee immigrants arriving during the late 1970s and early 1980s varies with the number of immigrants, but found no correlation between outcomes eight years after arrival and the size of the inflow at an individual's time of arrival. Admittedly, the value of this evidence is limited since there are no years with refugee immigration comparable to the late 1980s, but at least the results are not at odds with our evaluation approach.

5. 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 25 percent lower because of the new policy; idleness increased by around six percentage points; and welfare receipt rose by almost 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.

[27] With 75/80 immigrants as comparison, the estimates are: log earnings: –0.246 (0.036); idleness: 0.041 (0.009); welfare receipt: 0.073 (0.010).

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 20 percentage points higher because of the placement policy. Since people moved out of the initial locations, this loss was largely undone, however. We find some indications that the effects of the distribution component of the policy can be traced to placement in regions with high unemployment and absence of ethnic networks.

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 indirect 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. This might be instructive for future policies in Sweden and elsewhere.

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Appendix

Table A1a: Sample sizes.

Group	Total sample 81/83	Total sample 87/89	Earnings sample 81/83	Earnings sample 87/89
Immigrants	2,679	9,883	2,311	6,418
Natives	363,083	378,016	337,925	342,077
OECD migrants	1,973	3,744	1,659	2,799
Immigrants 75/80	20,474	21,412	17,826	16,032
Young and less educated natives	68,211	48,099	66,177	42,808

Notes: Sample sizes by group and period. Earnings sample contains only those with positive earnings in 8 years after arrival. "Immigrants" are taken from the LINDA immigrant and population samples. They are identified as refugees according to criteria described in section 2. "Natives" come from the LINDA population sample. "OECD migrants" consists of individuals from the LINDA immigrant and population samples who immigrated during the same period as the refugee immigrants. "Immigrants 75/80" consists of individuals in the LINDA immigrant sample who arrived from non-OECD countries between 1975 and 1980. The same age restrictions apply in all these groups: 26–63 when outcomes are measured (18–55 at arrival for "Immigrants" and "OECD migrants"). "Young and less educated natives" are natives aged 20–29 with no more than short high school education (11 years of schooling).

Table A1b: Sample composition, percent.

Year	Total sample	Earnings sample
81	5.45	6.77
82	9.08	11.42
83	6.79	8.28
87	20.47	20.28
88	27.92	26.23
89	30.28	27.01

Notes: Entries give the percentage of immigrants in the respective period (pre-reform, reform) immigrating in a particular year. The comparison groups are weighted to conform to this composition; i.e. the weight given to observations made in different years are the same in all comparison groups as in the immigrant sample. Earnings sample contains only those with positive earnings eight years after arrival.

Table A1c: Summary statistics, individual characteristics, means (std).

Variable	Refugee immigrants 81/83	Refugee immigrants 87/89	Natives 81/83	Natives 87/89	OECD migrants 81/83	OECD migrants 87/89	Immigrants 75/80 81/83	Immigrants 75/80 87/89	Young and less educated natives 81/83	Young and less educated natives 87/89
Female	.479	.445	.491	.489	.533	.448	.486	.487	.458	.442
	(.500)	(.497)	(.500)	(.500)	(.499)	(.497)	(.500)	(.500)	(.498)	(.497)
Age	37.549	38.124	43.231	43.543	37.393	38.372	39.542	42.872	24.470	25.345
	(8.456)	(8.289)	(10.536)	(10.601)	(8.674)	(8.914)	(7.961)	(8.398)	(2.805)	(2.622)
Married	.605	.592	.596	.532	.521	.463	.665	.609	.155	.123
	(.489)	(.492)	(.491)	(.499)	(.500)	(.499)	(.472)	(.488)	(.361)	(.329)
Kid	.500	.514	.350	.330	.434	.402	.575	.491	.221	.235
	(.500)	(.500)	(.477)	(.470)	(.496)	(.490)	(.494)	(.500)	(.415)	(.424)
Married* female	.299	.280	.304	.272	.299	.228	.338	.302	.096	.073
	(.458)	(.449)	(.460)	(.445)	(.458)	(.420)	(.473)	(.459)	(.295)	(.260)
Kid*female	.287	.291	.196	.198	.292	.244	.320	.263	.176	.194
	(.452)	(.454)	(.397)	(.398)	(.455)	(.429)	(.467)	(.440)	(.381)	(.395)
Education										
<9 years or missing	.258	.214	.220	.126	.251	.186	.273	.201	.033	.014
	(.437)	(.410)	(.415)	(.331)	(.434)	(.389)	(.445)	(.401)	(.179)	(.119)
9–10 years	.108	.185	.126	.122	.123	.187	.123	.132	.224	.264
	(.311)	(.388)	(.331)	(.327)	(.328)	(.390)	(.329)	(.339)	(.417)	(.441)
High school ≤2 yrs	.288	.168	.307	.358	.239	.217	.285	.317	.743	.721
	(.453)	(.374)	(.461)	(.479)	(.426)	(.412)	(.452)	(.465)	(.437)	(.448)
High school >2 yrs	.135	.185	.111	.121	.126	.144	.111	.121		
	(.342)	(.388)	(.314)	(.326)	(.332)	(.351)	(.314)	(.326)		
University <3 yrs	.102	.134	.116	.145	.115	.124	.095	.112		
	(.304)	(.341)	(.320)	(.352)	(.319)	(.330)	(.293)	(.316)		
University ≥3 yrs	.109	.114	.120	.129	.146	.141	.112	.116		
	(.312)	(.317)	(.325)	(.335)	(.353)	(.348)	(.316)	(.320)		
Ethnicity										
Nordic					.622	.644				
					(.485)	(.479)				
Western Europe					.312	.262				
					(.464)	(.440)				
Eastern Europe	.373	.180					.351	.338		
	(.484)	(.384)					(.477)	(.473)		
Africa	.092	.116					.091	.087		
	(.288)	(.320)					(.288)	(.282)		
Middle East	.233	.457					.277	.287		
	(.423)	(.498)					(.447)	(.452)		
Asia	.142	.083			.008	.007	.120	.126		
	(.349)	(.277)			(.090)	(.083)	(.325)	(.331)		
North America					.051	.070				
					(.220)	(.255)				
South America	.160	.164					.161	.163		
	(.366)	(.370)					(.367)	(.369)		
Oceania					.007	.017				
					(.082)	(.129)				

Notes: Sample sizes and composition (by year) in Tables A1a–b. "Kid" = 1 if there were children aged 15 or less in the household, 0 otherwise.

Table A2: Earnings, idleness, and welfare receipt by group and cohort, mean (std), outcomes in *t*+8.

Group		81/83		87/89		Difference (87/89–81/83)
(Refugee) Immigrants	Log (earnings)	11.48	(1.09)	10.96	(1.51)	–0.52
	Idleness (%)	12.72	(33.32)	29.24	(45.49)	16.52
	Welfare receipt (%)	21.46	(41.06)	32.09	(46.68)	10.63
Natives	Log (earnings)	11.94	(0.81)	11.85	(1.02)	–0.09
	Idleness (%)	6.75	(25.09)	8.85	(28.40)	2.10
	Welfare receipt (%)	3.69	(19.84)	4.10	(19.84)	0.41
OECD	Log (earnings)	11.63	(1.08)	11.52	(1.28)	–0.11
	Idleness (%)	15.44	(36.14)	23.06	(42.12)	7.62
	Welfare receipt (%)	11.20	(31.55)	11.45	(31.84)	0.25
Im 75/80	Log (earnings)	11.59	(1.02)	11.40	(1.31)	–0.19
	Idleness (%)	11.97	(36.14)	23.36	(42.31)	12.39
	Welfare receipt (%)	14.27	(34.98)	11.64	(32.07)	–2.63
Young and less edu natives	Log (earnings)	11.67	(0.79)	11.36	(1.20)	–0.31
	Idleness (%)	2.56	(15.78)	8.04	(27.18)	5.48
	Welfare receipt (%)	9.34	(29.10)	13.59	(34.27)	4.25

Notes: The comparison groups are weighted to conform to immigrants' composition by year; i.e. the weights given to observations made in different years are the same in all comparison groups as in the immigrant sample. "Idleness" is defined as having zero earnings and study allowances. Outcomes measured in *t*+8, i.e. 89/91 and 95/97 for all groups. Earnings deflated by the CPI.

Table A3: Difference-in-difference estimates with alternative comparison groups, outcomes in measured in *t*+8.

	Program effect	Time effect	# individuals	R-squared
Log earnings				
Natives	–.387	–.108	688,731	.12
	(.029)	(.002)		
Young and less educated natives	–.081	–.414	117,714	.12
	(.030)	(.007)		
Low-skilled natives	–.356	–.139	177,414	.04
	(.030)	(.005)		
OECD	–.331	–.164	13,187	.11
	(.047)	(.036)		
Immigrants 75/80	–.263	–.233	42,587	.08
	(.032)	(.013)		
Idleness				
Natives	.123	.025	753,661	.10
	(.008)	(.001)		
Young and less educated natives	.087	.061	128,872	.12
	(.008)	(.001)		
Low-skilled natives	.105	.043	197,043	.07
	(.008)	(.002)		
OECD	.066	.082	18,279	.15
	(.013)	(.010)		
Immigrants 75/80	.056	.092	54,448	.12
	(.009)	(.004)		
Welfare				
Natives	.077	.004	753,661	.06
	(.009)	(.000)		
Young and less educated natives	.029	.051	128,872	.09
	(.009)	(.002)		
Low-skilled natives	.073	.007	196,989	.05
	(.009)	(.001)		
OECD	.085	–.005	18,279	.12
	(.013)	(.009)		
Immigrants 75/80	.101	–.021	54,448	.10
	(.010)	(.003)		

Notes: OLS parameter estimates, standard errors in parentheses. The regressions also include immigrant status, gender, age, age squared, level of education, region 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 immigrant status and remaining individual characteristics. For details on the different samples, see Tables A1a–c. The "Young and less educated natives" comparison group consists of natives aged 20–29 with no more than short high school education. "Low-skilled natives" consists of the quartile of the "Natives group" predicted to have the poorest outcomes by a regression of the outcome variable on individual characteristics. The native comparison groups have not been reweighted to conform to immigrants' geographic distribution.

Table A4: Coefficients on individual characteristics in estimations for Table 7.

	(1) log (earnings)	(2) Pr(idle)	(3) Pr(welfare)
Female	.0279	–.0013	.0688
	(.0904)	(.0227)	(.0168)
Age	.0558	–.0166	–.0161
	(.0273)	(.0063)	(.0047)
Age squared ($*10^{-2}$)	–.0618	.0310	.0257
	(.0337)	(.0075)	(.0056)
Married	.2429	–.0748	–.0381
	(.0867)	(.0223)	(.0165)
Kid	–.0413	–.0439	.1253
	(.0851)	(.0225)	(.0167)
Married*female	–.1243	.0311	–.0932
	(.1135)	(.0293)	(.0217)
Kid*female	–.3044	–.0312	–.0119
	(.1145)	(.0298)	(.0221)
Education (missing and <9 years, reference)			
9–10 years	.0586	–.1147	–.0334
	(.0796)	(.0195)	(.0144)
High school ≤ 2 years	.2161	–.1899	–.1136
	(.0798)	(.0203)	(.0150)
High school > 2 years	.2103	–.1656	–.1104
	(.0802)	(.0198)	(.0147)
University < 3 years	.1411	–.2592	–.1255
	(.0855)	(.0215)	(.0159)
University ≥ 3 years	.4995	–.2399	–.1992
	(.0885)	(.0228)	(.0169)
Region of origin (Eastern Europe, reference)			
Africa	–.2064	.0458	.2137
	(.0936)	(.0249)	(.0185)
Middle East	–.4681	.1154	.2074
	(.0715)	(.0190)	(.0141)
Asia	–.0393	.0245	.0260
	(.0984)	(.0266)	(.0197)
South America	–.0101	–.0564	.0560
	(.0824)	(.0228)	(.0169)
Immigration year (1987, reference)			
1988	–.0483	.0408	.0173
	(.0593)	(.0158)	(.0117)
1989	–.0247	.0713	.0946
	(.0596)	(.0156)	(.0116)
# individuals	6,418	9,883	9,883
# municipalities	168	168	168

Notes: Parameter estimates, standard errors in parentheses. See Table A1c for descriptive statistics.