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Do when and where matter? Initial labor market conditions and immigrant earnings

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Do when and where matter? Initial labor market conditions and immigrant earnings*

by

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Abstract

This paper investigates the long-term effects on immigrant earnings and employment from labor market conditions encountered upon arrival. We find substantial effects both of the state of the national labor market and of local unemployment rates. Comparing refugees entering Sweden in a severe and unexpected recession to refugees arriving in a preceding economic boom, we attempt to handle the issue of selective migration. The analysis of effects at the local level exploits a governmental refugee settlement policy to get exogenous variation in local labor market conditions.

Keywords: Immigration, earnings, labor market conditions
JEL classification: F22, J15, J61, R23

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1 Introduction

Immigrants on average perform worse than natives in the labor markets of most Western countries (OECD 2001). This has caused researchers to search for explanations to the gap between immigrants and the native population, and to differences across immigrant groups. On the latter topic, some studies have considered the possibility that labor market conditions at the time of immigration may have a long-term impact on earnings and employment.

There are two basic aspects of this issue. First, do cohorts arriving in a time of good prospects in the national labor market fare better compared to immigrants arriving in a time of bad prospects? Second, does the long-term success differ between people who arrive at the same time, but encounter different local labor market conditions?

If there is a negative effect on people immigrating during economic downturns, governments may opt to adjust immigration and/or integration policies over the business cycle. Many European countries use some type of policy to control the initial location of certain immigrant groups (Dutch Refugee Council 1999). If there proves to be a long-term effect of which type of local labor market one initially stays in, governments would probably like to focus their settlement policies on regions with favorable labor market conditions. It is also well-documented that immigrants at least in part base their location decisions on factors other than the properties of the labor market, such as the presence of other immigrants.[1] The earnings and employment consequences of this behavior becomes a policy concern if society values labor market success higher than the individual, or if location decisions are based on incomplete information about the impact of local conditions.

Previous empirical investigations have focused on the first issue: does the state of the national labor market matter? A limitation in these studies is that they have been unable to control for the fact that individual decisions on when and where to migrate may be influenced by labor market conditions in the host country. If the composition of immigrants arriving to a certain country in recessions differs from the composition of the inflow in better times, this may create a misleading picture of how initial labor market conditions affect individuals.

[1] This is confirmed by several studies, including Bartel (1989) and Zavodny (1999) for the US, and Åslund (2001) for Sweden.

We use Swedish data on refugees immigrating between 1987 and 1991 to investigate these issues. Studying this group of immigrants has several advantages. Refugees are likely to be less responsive to the state of the labor market, since push rather than pull factors are behind their migration. Furthermore, the cohorts we study made the decisions to migrate under similar labor market expectations, so any selection on host country characteristics should go in the same direction in all the included cohorts.[2] A dramatic increase in unemployment in the early 1990s created a situation where the included refugee cohorts met radically different labor markets. Those immigrating in 1987 spent up to four years in a good labor market, whereas the 1991 cohort met unprecedented levels of unemployment during their first years in Sweden.

In the analysis of the second question, we exploit a refugee settlement policy pursued by the Swedish government during the late 1980s and early 1990s. The policy meant that people were not free to choose their initial location in the country, and provides a source of exogenous variation to study the impact of initial local labor market conditions. Since we have cross-sectional variation under very different national labor markets, we also get a picture of whether initial locations matter more in a boom or in a recession.

Why could initial labor market conditions matter in the long run? One theory is that poor prospects increase the probability of unemployment, which in turn leads to long-term "scars". Scarring can be a problem if, for example, employers use past unemployment as a signal on low productivity, or if time out of employment leads to skill losses. Several recent studies find evidence for this type of state dependence; see e.g. Arulampalam et al. (2000), Arulampalam (2001), Gregg (2001), and Gregory & Jukes (2001) for the UK, and Hansen & Lofstrom (2001) for immigrants in Sweden.[3]

Beaudry & DiNardo (1991) show that conditions at the time of entry on the labor market can matter for a long time if there is imperfect job mobility among the workers. In their model, the existence of a relocation cost gives employers the opportunity to hold down wages for those who entered at a low wage level. Costs of geographic mobility can also affect the impact of local conditions. If

[2] Agreeably, refugees can choose which country to go to. If conditions in Sweden changed relative to other alternatives, the selection process could differ across the studied cohorts. We will return to this issue in section 4 with a presentation of some facts indicating that this is not likely to be a major problem.

[3] Ellwood (1982) is an early study of the scarring phenomenon. Steiner (2001) rejects the hypothesis of unemployment persistence in an investigation using data for West Germany.

starting out in a persistently bad location means a greater probability to be living there also after some period of time, this could be a mechanism through which initial exposure to poor conditions is associated with less success in the labor market.

Previous research that has focused on the conditions at entry does not give a clear message on the existence of long-term effects. Some studies find a negative impact that lasts for at least some time; see e.g. Chiswick & Miller (2002) on earnings among US immigrants. Other results for the US actually suggest that entering in a time of high unemployment could be associated with higher probabilities of individual employment (Chiswick et al. 1997). Similarly, McDonald & Worswick (1999) find that people immigrating to Australia during high unemployment experience faster earnings assimilation.[4] None of these studies deal with the issue of selective migration.[5]

Our results suggest that both national and local conditions matter for several years. Entering in a labor market recession decreases the chances of employment and lowers earnings during the observation period (5–7 years after immigration). Meeting poor local conditions has a clear impact on earnings and employment for at least ten years. There are indications that initial local conditions matter not only through an increased probability of remaining in a persistently bad location: initial unemployment rates appear to affect outcomes also when controlling for contemporary local unemployment.

The rest of the paper is outlined as follows. Section 2 gives a brief overview of refugee migration to Sweden, the government placement policy for refugees, and the rise in unemployment during the 1990s. Section 3 describes the data. Section 4 contains the empirical analysis. We first use methods along the lines of previous studies to relate Sweden to other countries. Then we go on to our main results, beginning at the national level and then proceeding to the local level. Section 5 concludes.

[4] MacDonald & Worswick (1998) report a negative but insignificant relation between the unemployment rate at entry and earnings for immigrant men in Canada. Nakamura & Nakamura (1992) report lower current wages for immigrants who entered the US or Canada under high national unemployment. Stewart & Hyclak (1984) find higher earnings among people who immigrated to the US during high annual GNP growth.

[5] Note that the inconclusive results in these studies do not necessarily indicate an absence of scarring effects among immigrants, since they do not directly analyze the issue of scarring.

2 Background

Our empirical approach builds on two sources of variation that we argue can be treated as exogenous: the governmental refugee settlement policy and the Swedish economic crisis of the early 1990s. This section gives some background on these issues.

Figure 1 shows the development of refugee immigration and annual unemployment rates. In the later part of the 1980s the Swedish economy was experiencing a boom with low and falling levels of unemployment. In 1990 unemployment began to rise from below two percent, reaching the unprecedented level of ten percent in 1993. Employment in both the private and the public sector decreased (Lundborg 2000). As a matter of fact, industrial employment began to fall already in the later part of 1989, and so did the vacancy rate.

The figure also shows that refugee immigration increased rapidly during the 1980s, with a peak of around 25,000 residence permits in 1989. After a small downturn in the first years of the 1990s, there was a new, and much higher, peak caused primarily by the war in Bosnia-Herzegovina.

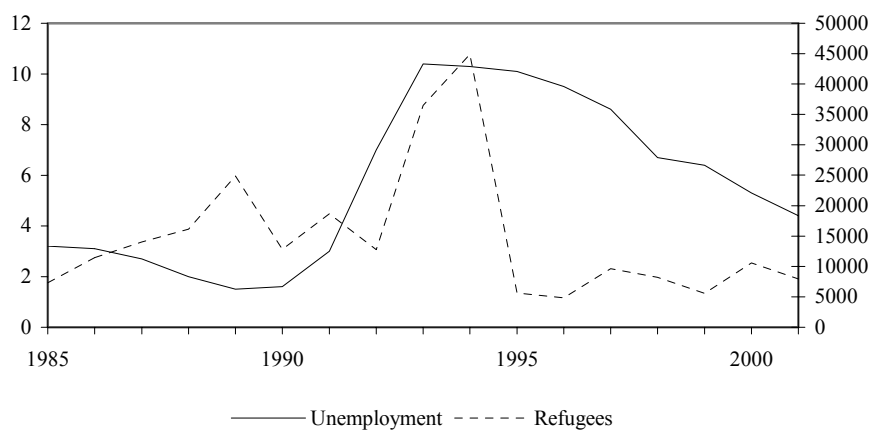

Figure 1 National unemployment (percentage of labor force) and the number of residence permits granted to refugees, 1985–2001.

Source: The Labor Market Board and the Swedish Migration Board.

2.1 The refugee reception system and the settlement policy[6]

In 1985 the Swedish government implemented a new refugee reception system containing two new major elements: municipal placement of refugees and an extensive introduction period. Under this policy the typical refugee arrived to Sweden, applied for asylum, and then stayed in a refugee center in anticipation of a residence permit.

The placement policy was a reaction to the geographic concentration of new immigrants, where some municipalities in metropolitan areas thought they took an unfair share of the burden of immigration. The policy meant that refugees were not free to choose where to reside initially, but were assigned to a municipality by the government after being given residence permits. There were, though, no restrictions against relocating if the refugees could find a place on their own. The placement system was in reality viable during the period 1985–91, and a vast majority of the refugees were placed by the government. In the years 1987–91 the figure was about 90 percent. Beginning in 1992, the system collapsed under heavy immigration from former Yugoslavia. Formally, it was in place to 1994, however.

At first the idea was to put people in municipalities that provided good opportunities for work or education. Over time, a focus on small locations and the advantages of their presumed closeness between natives and immigrants evolved. In practice, available housing became the deciding factor, as this was a scarce factor in many cities during the boom of the late 1980s. As immigration soared, almost all municipalities became involved in the reception: 277 out of 284 municipalities had an agreement with the Immigration Board in 1989, compared to the original idea of 60 reception locations.

Edin et al. (2002 & 2003) argue that the municipal placement can be regarded exogenous, conditional on observed characteristics of the individual. For example, people of some nationalities were more likely to end up in certain locations than others, but there was no interaction between municipal placement officers and refugees.[7] Similar to those studies, we use the placement pol-

[6] This section draws primarily on Edin et al. (2002 & 2003).

[7] This means that selection into different locations was based solely on observed characteristics. Level of education was a factor for selection, since municipal officers tried to cream-skim among refugees. Also, singles were less attractive because small apartments were scarce.

icy as a natural experiment generating an initial geographic distribution that was independent of characteristics unobserved to the researcher.

The other central feature of the new refugee reception system was an introduction period that lasted on average 18 months after the municipal placement (in many cases considerably longer). During this time the individual was not supposed to enter the labor market, but instead to participate in language training and other introductory activities. However, the individual had the legal right to enter the labor market.

The introduction system remained largely unaltered during the period of interest for this study (1987–91). One change is, however, worth mentioning. In 1991 the system for reimbursement from the central government to the municipalities changed from "running expenses" to a standard amount per refugee.[8] The idea was to increase the municipalities' incentives to provide a quick economic integration (see The Immigration Board 1997 for further details).

The unexpectedly large number of immigrants increased waiting times for receiving residence permits. On average, those receiving resident permits in 1987 spent about four months in Sweden pending the decision; for the 1991 cohort the corresponding figure was more than one year. There were considerable differences in waiting times depending on visa category, but waiting times were in general longer in later cohorts also within categories (see Rooth 1999 for a further description on waiting times and refugee categories). [9]

2.2 Implications for this study

The dramatic increase in unemployment in the early 1990s was unexpected. Given this and the long waiting times pending a decision on residence permits discussed above, it is likely that the immigrant cohorts of interest made their decision to go to Sweden under similar expectations about the conditions in the Swedish labor market. Any selection (of individuals) based on the state of the host country's labor market is therefore expected to act in the same direction for all cohorts. Furthermore, refugees are likely to be less sensitive than other migrants to labor market opportunities in their decisions, since push factors play a central role for this group.

[8] The standard amount was differentiated for some groups, for example for old people.

[9] The refugee categories included are: Convention, Conscientious objectors, De Facto, Quota, Humanitarian, and General Decision. Waiting times were especially short for quota refugees, who in practice received their residence permits at arrival.

As the length of the introduction period indicates, labor market entry did in most cases not occur immediately after reception of the residence permit. The later the cohort arrived, the lower the number of people who could enter before the recession started. It seems likely that almost everybody in the 1987 cohort entered under favorable conditions, while this was the case for very few immigrants in the 1991 cohort. We will use the economic crisis as an exogenous force that made otherwise similar refugee groups face very different national labor markets.

Our other source of exogenous variation is the refugee placement policy. Following previous investigations (Edin et al. 2002 & 2003), we exploit this policy to estimate the effect of entering in different local labor markets.

3 Data

Our empirical analysis is based on the FLYDATA dataset, which integrates records from the Swedish Immigration Board, the National Labor Market Board and Statistics Sweden. FLYDATA contains information on all refugees and tied movers who received residence permits during the period 1987–91; see Rooth (1999) for further details.

The subset of FLYDATA used here contains information about 46,967 refugee immigrants (not tied movers) of working age, who received a permanent visa during the period 1987–91. In this analysis we only consider refugee immigrants who were 19 to 55 years old when they were granted their permanent residence permits.

The refugees are followed in the different registers through 1998, which gives the opportunity to observe each cohort for at least seven years after the year of immigration. The maximum observation period is eleven years (the 1987 cohort). Since we are interested in persistent effects of initial labor market conditions, we (arbitrarily) begin estimating in year t+5: five years after immigration.

Table 1 Sample means for some variables. Standard deviations in parentheses.

Cohort	1987	1988	1989	1990	1991
Age at migration	29.7 (7.6)	30.2 (7.8)	30.5 (7.7)	30.7 (7.5)	30.6 (7.5)
Married	.59	.60	.65	.67	.63
Men	.63	.63	.62	.63	.64
Pre-immi. Edu.[10]					
<9 years	.17	.19	.22	.26	.17
9–10 years	.18	.20	.25	.19	.11
High school ≤2 years	.22	.17	.16	.12	.11
High school>2 years	.25	.23	.20	.18	.28
University <3 years	.11	.11	.10	.12.	.14
University ≥3 years	.08	.10	.08	.13	.19
Imputed years of sch.	11.0 (2.6)	11.1 (2.8)	10.7 (2.7)	11.0 (3.1)	12.0 (3.2)
Country of origin:					
Ethiopia	.10	.09	.08	.09	.10
Somalia	.00	.00	.02	.05	.12
Romania	.05	.07	.04	.01	.05
Yugoslavia	.00	.01	.02	.05	.05
Turkey	.02	.02	.03	.03	.03
Poland	.02	.01	.02	.01	.00
Iran	.48	.39	.18	.17	.16
Iraq	.04	.10	.07	.15	.15
Lebanon	.02	.03	.08	.06	.05
Vietnam	.02	.02	.02	.09	.01
Chile	.11	.10	.21	.02	.00
Unknown	.01	.03	.05	.06	.02
No citizenship	.02	.03	.03	.03	.03
Other countries	.11	.10	.15	.18	.23
Waiting time (years)	.30 (.89)	.42 (.52)	.62 (.84)	.54 (.48)	1.09 (.64)
No. of individuals	8,623	9,592	14,728	6,040	7,984

Notes: Data from FLYDATA, employment sample (including those with zero earnings). All variables measured two years after immigration. The variables are described in the appendix.

The dataset contains information on annual earnings, level of pre-immigration education, municipality of residence, country of origin, age at migration, sex, and marital status. It also includes visa status and time spent in

[10] The construction of FLYDATA used several means of tracing pre-immigration education, such as comparisons of data from the Immigration board, education registers, and telephone interviews. We have excluded from the sample individuals whose level of education could not be determined. Including these individuals does not change the results presented in the paper.

Sweden waiting for a residence permit. Table 1 shows that there were only minor differences across the 1987–91 cohorts in terms of age at migration and gender distribution. We find slightly more variation in the fraction of married individuals and substantially larger differences in pre-immigration education. Most strikingly, the 1991 cohort (and to some extent the 1990 cohort) contained a substantially higher fraction of people with university education, in particular longer university training. The average length of schooling differs by about a year between those immigrating in 1991 and those who entered Sweden 1987–90.

There was also a shift in source country composition over time. Iranians made up almost half of the 1987 cohort, Chilean refugees came primarily in 1989, whereas people from Somalia were more common among the 1991 immigrants. These differences indicate the importance of controlling for education and country of origin in the analysis.

Table 2 below gives some descriptive statistics on the outcome variables: log annual earnings and employment. The employment variable is one if the individual had positive earnings in the particular year, zero otherwise. This is indeed a very generous definition of employment, and one could argue that a higher threshold should be used. Our main arguments for using this earnings limit are the following: (i) a large fraction of the sample has zero earnings, and there is probably a difference between having no connection to the labor market whatsoever and working for at least some time—any other threshold is at least as arbitrary and it is less clear what it measures; (ii) annual earnings will always to some extent capture employment variation regardless of the earnings limit. We have also performed robustness checks using alternative thresholds; they are discussed in section 4.

The average employment rates show that those who arrived to a good labor market worked to a much larger extent soon after immigration than did those who met a bad labor market. The latter immigrants, however, increased their employment rates steadily, whereas especially the 1987 and 1988 cohorts were impeded by the economic crisis of the 1990s. Average earnings among workers (those with positive earnings) are also higher for refugees in the earlier cohorts compared to the people in our sample who immigrated in the 1990s.

The table shows that a fraction of the immigrants worked already in the year they received their residence permit, and in the following year more than half of the 1987–89 cohorts have positive earnings. This suggests that some people entered the labor market directly instead of only participating in the introduc-

tion program and/or that the introduction program in some cases included short-term subsidized employment. However, annual earnings were in many cases very low in the first two years; e.g. more than 60 percent of those in the 1990 cohort working one year after immigration had annual earnings lower than the average quarterly earnings of a worker in the Swedish manufacturing industry (Nordström Skans 2002).

Table 2 Earnings and employment among refugees, *t*–*t*+11.

	Log earnings					Employment				
ysm	87	88	89	90	91	87	88	89	90	91
0	9.21	9.54	9.86	9.61	9.11	.23	.25	.35	.19	.07
1	9.94	10.10	10.35	9.80	9.32	.56	.54	.55	.31	.21
2	10.38	10.47	10.51	9.91	9.63	.75	.71	.60	.33	.28
3	10.58	10.50	10.54	9.95	9.91	.80	.69	.54	.33	.37
4	10.57	10.54	10.50	10.13	10.24	.75	.59	.49	.41	.44
5	10.65	10.53	10.52	10.40	10.37	.66	.55	.51	.45	.47
6	10.62	10.56	10.65	10.53	10.49	.61	.55	.54	.45	.49
7	10.68	10.68	10.74	10.63	10.67	.60	.56	.53	.46	.53
8	10.78	10.78	10.83	10.70		.61	.55	.53	.49	
9	10.87	10.84	10.94			.60	.56	.56		
10	10.93	10.99				.60	.57			
11	11.07					.62				

Notes: log earnings in 2001 prices and employment (= earnings>0) by cohort and years since migration (ysm). The variables are described in the appendix.

Our primary geographic unit is local labor market regions. The 1993 definition of these regions divides Sweden's municipalities into 109 regions, based on commuting behavior. Compared to municipalities, this geographic grouping better captures the local labor market prospects that actually faced the individuals. Especially within metropolitan areas, the possibility of commuting across municipalities may make municipalities a less appropriate geographic unit for measuring local labor market opportunities. [11] However, we use municipalities in a sensitivity analysis.

[11] The category *Stockholm*, which is the largest local labor market area by this definition, is made up of twenty-nine municipalities while others are made up of just one municipality with a small population.

The analysis of the impact of the state of the national labor market requires a comparison group to eliminate time effects. Data for these groups have been taken from the LINDA database; see Edin & Fredriksson (2000) for details. This database is also the source of individual data for the estimations that relate Sweden to results from other countries (section 4.1).

The LINDA database consists of annual information from tax records and population registers. There is one three-percent sample of the overall Swedish population, and another sample containing 20 percent of the foreign-born population. We use a number of different samples from the database, some representative for the overall Swedish population and some samples of immigrants arriving during certain periods. In section 4 we return to how we construct and use the samples.

For measuring local labor market properties we primarily use annual employment statistics from Statistics Sweden and unemployment statistics from the Labor Market Board (Ams). We also utilize data on vacancies (Ams) and job creation and job destruction rates (IFAU) and unemployment data from the Swedish Labor Force Surveys (AKU). All variables are described in the appendix.

4 Empirical analysis

We begin this section by briefly comparing patterns in Swedish data with what has been found for other countries in previous studies. Then we turn to analyzing the impact of labor market conditions at the national level, where we compare the outcomes of the 1987–91 refugee cohorts. The investigation of the impact of the initial local labor market concludes the section.

4.1 Relating Sweden to results for other countries

Some previous studies (Chiswick et al. 1997; McDonald & Worswick 1999) indicate that meeting a bad labor market at the time of immigration may be correlated with higher earnings and employment rates in subsequent years.[12] This

[12] Chiswick et al. (1997) estimate the relation between the unemployment rate at the time of entry and individual employment for foreign-born individuals, natives, and a pooled sample of these two groups. The results are of the same magnitude in all samples, although only statistically significant in the sample of natives and in the pooled sample.

finding is indeed peculiar from a theoretical point of view (if not taking selective migration into account), but as a starting point it is interesting to see which impression we would get from a similar analysis on Swedish data.

Table 3 presents estimates from regressions using ten cross-sections for the years 1990 through 1999 from the LINDA database. The dependent variable is either employment or log earnings, and the explanatory variable of interest is the national unemployment level in the year of immigration.

The first two columns suggest quite small effects of initial labor market conditions. At face value, an increase in initial unemployment of one percentage point[13] is associated with about 1.7 percent lower earnings and half a percentage point lower employment probability. When we allow the effect to vary by immigration period (before and after 1990) we get larger effects. The estimates for those who arrived prior to 1990 suggest that a one percentage point rise in unemployment is connected with an employment probability *increase* of 2.3 percentage points and about 6.6 percent *higher* earnings. For the immigration in the 1990s, the corresponding unemployment variation is coupled with 1.0 percentage points *lower* employment rates and 3.2 percent *lower* earnings.

These results are quite mixed—just as findings from other countries. The estimations show that also for Sweden one could conduct an analysis where a bad initial labor market is associated with better outcomes, at least for some groups. In the next step, we can see whether this conclusion is altered when we attempt to control for the selection problems that are likely to influence the results of Table 3.

[13] A standard deviation in the variable amounts to 1.8 percentage points.

Table 3 Labor market outcomes and the unemployment rate at entry, cross-sections 1990–99.

	Log earnings	Employment	Log earnings	Employment
Unemployment	–.016**	–.005**		
	(.003)	(.001)		
Unemp., im. Year <1990			.066**	.023**
			(.009)	(.003)
Unemp., im. Year ≥ 1990			–.032**	–.010**
			(.004)	(.001)
Observations	475,959	701,164	475,959	701,164
R-squared	.17	.17	.17	.17

Notes: Regressions of employment and log earnings (conditional on employment) on the national unemployment level (percentage of the working age population) in the year of immigration (taken from the Labor force surveys (AKU). Employment equals 1 if the individual had positive earnings, 0 otherwise. The regressions include controls for observation year, years since migration (dummy for each year), age and age squared, gender, level of education, and immigration decade. Full estimation results are available upon request. The sample is restricted to those immigrating between 1976 and 1999, aged 19–64, and at least 18 years old at immigration. * (**) denotes significance at the 5(1)-percent level.

4.2 The national level

This subsection compares the outcomes of the 1987–91 refugee cohorts to get a picture of how the state of the national labor market at the time of immigration influences employment and earnings. We follow individuals during three years, starting five years after immigration.

Consider estimating the following model

$$Y_{it}^{k} = \alpha_c + \alpha_r r_i + \beta_c' X_{it}^{k} + \beta_r' X_{it}^{k} r_i + \varphi' YSM_{it} + \delta_t' T_t + \gamma^{k\prime} C_i^{k} + \varepsilon_{it} \qquad (1)$$

where Y_{it}^{k} is either employment or log earnings for individual *i* in cohort *k* at time *t*, r_i is a dummy indicating if the person is a refugee or not, X_{it}^{k} is a vector of individual characteristics, YSM_{it} is a set of dummy variables indicating the number of years the person has spent in the host country, T_{it} are observation year dummy variables, and C_i^{k} are dummy variables for belonging to refugee cohort *k* (set to zero for the comparison group). The parameter vector δ_t captures general time effects, and γ^{k} is a vector of cohort effects stemming from

the labor market conditions the refugees met at arrival. Of course, γ^k contains the parameters of primary interest.

Since we compare the outcomes in different calendar years, it is important to separate the general time effect δ_t from γ^k, i.e., to eliminate macro effects that would have caused different outcomes even in the absence of an effect of the initial labor market conditions. We add a comparison group in order to identify the general time effects, since φ, δ_t, and γ^k cannot be identified together using only the refugee sample . The approach of eliminating contemporary macro effects by assuming that they are equal among, e.g., immigrants and natives is common in these types of studies; see e.g. Borjas (1994).

The problem is then to find a comparison group that captures the relevant macro effects. Edin et al. (2002) analyze a similar problem and find that the best comparison group is immigrants from non-OECD countries who arrived before the refugees in our sample.[14] Based on this, we use non-OECD immigrants who immigrated between 1975 and 1980 in our baseline analysis.[15] We also estimate our model with a representative sample of the native population as the comparison group. We will return to this in the presentation of the results.

The model specified in (1) estimates the average difference in outcomes between cohort *k* and the reference cohort (1987) in the three years of observation, conditional on observed individual characteristics, time spent in Sweden, and the general time effect δ_t. The approach builds on a number of important assumptions. First, the time effect should be the same among refugees and the comparison group. Second, for γ^k to capture the effect of entering in different states of the labor market, this must be the only thing that separates the reference cohort and refugee cohort *k* (conditional on observed characteristics). The individual characteristics include controls for gender, age, marital status, level of education, and country of origin. Note also that we allow the effects of individual characteristics to vary between refugees and the comparison group.

[14] Edin et al. (2002) find that OECD immigrants arriving at the same time as the refugees is the second-best group. This alternative is, however, not appropriate in this study, since an effect of the initial labor market is likely to affect everybody immigrating in a particular year.

[15] For each observation year, immigrants who arrived in any year 1975–80 are included. Note, though, that we control for years since migration also for this group.

What are the expectations on differences across cohorts? As discussed in section 2, most individuals of the 1987 cohort had finished their introduction activities in time to enter the labor market under favorable conditions. In the 1991 cohort on the other hand, few people entered before unemployment had become high or was at least rising rapidly.

If initial conditions matter in a longer run we should therefore expect the 1991 cohort to perform worse than those immigrating in 1987. In the intermediate cohorts, a falling fraction likely entered the labor market in good times. Therefore, we would expect a pattern of declining performance the later the cohort arrived. Note, though, that the hypothesis says nothing about which period of time that actually defines "initial labor market conditions". It could be that just a few months of "treatment" is enough to grant a stronger position in the labor market; it could also be that the individual needs a couple of years to become established.[16] Therefore, our priors concerning the results are not that clear for the 1988–1990 cohorts.

The specifications use observations on refugees 5–7 years after immigration. If there is a persistent effect of initial conditions, it should be present in our data.[17] One interesting point to note is that the historical experience of refugee immigrants to Sweden suggests that most of the economic assimilation occurs within the first four years after arrival (Edin et al. 2000). If this is true also for the people in our sample, differences during our period of observation may well persist in a longer run.

Table 4 presents our baseline estimates for the national level, using log earnings and employment (earnings >0) as outcomes. The estimates presented in the first four columns indicate that those who entered in a poor labor market (1991) were less likely to work five to seven years later compared to those who met a good labor market (1987). Also, those who did work had lower earnings than their predecessors. The magnitude of the estimated effects is substantial: 7–9 percentage points lower probability to work, and 11–15 percent lower earnings among workers. The pattern for the intermediate cohorts is also roughly consistent with the underlying hypothesis. There appears to be declin-

[16] Chiswick et al. (1997) approach this problem by including averages of unemployment rates during the individual's first three years in the US.

[17] We would of course like to include also a much longer perspective. Since our data only include observation up to and including 1998 this is impossible at present.

ing performance across cohorts, and the 1990 refugees have as weak a position as those immigrating 1991.[18]

Why does the 1991 cohort perform at least as well as the 1990 cohort? There are at least two possible explanations to this result. First, most of the 1990 cohort may not have entered the labor market before the economic crisis began, given an average introduction period of one and a half years. Second, as already mentioned, the reimbursement system from the central government to the municipalities changed in 1991 in a way that probably increased the incentives to put people into work.

It can be argued that neither the comparison group of 1975–80 immigrants nor the one containing natives capture the appropriate time effects. These groups have a stronger position on the labor market than the refugees, and may therefore not be as sensitive to macroeconomic fluctuations. As a robustness check, we limited the comparison groups to the quartile with the lowest predicted outcomes.[19] The second panel of Table 1 shows that the results of these estimations are quite similar to those discussed above.

We now proceed to discuss some additional variations and robustness checks. The first two columns of Table A 1 show estimates from a model without general time effects (including only refugees). It seems that excluding the control for observation year yields a too negative picture of the performance in the 1988 and 1989 cohorts. Since this implies that correctly estimated time effects are important for the interpretation of the results, it is reassuring that Table 4 suggests the same basic pattern regardless of which group we use to generate these time effects.

[18] A few notes on the statistical models are in order. The reason for not applying a selection-correction model in the earnings specification is that we do not have access to an identifying instrument for the selection equation into employment. However, the results for the employment and earnings specifications go in the same direction. Had this not been the case, we would have been more worried about the interpretation of the earnings results. Since we have to control for general time effects, and do this in a difference-in-differences manner, the employment models are estimated using the linear probability model. Another issue is that there are repeated observations of individuals in the estimation samples, since we pool the data for three years of observation. To address the issue of correlated error terms, we apply the "cluster" command provided by the Stata software. This produces standard errors that only require independence across units—individuals in this case—and not between multiple observations of one unit. See Statacorp (2001) for a further description.

[19] The predicted outcomes were obtained through regressions of the outcome variable on the individual variables mentioned in the text. The regressions were made separately for each year.

Table 4 The effect of initial conditions in the national labor market.

	Overall			
Comparison	Immigrants 75/80		Natives	
	Log(y)	Empl.	Log(y)	Empl.
Cohort 1988	–.040	–.035**	–.051*	–.049**
	(.023)	(.007)	(.021)	(.006)
Cohort 1989	–.061*	–.065**	–.092**	–.082**
	(.026)	(.008)	(.020)	(.006)
Cohort 1990	–.131**	–.091**	–.162**	–.110**
	(.037)	(.011)	(.027)	(.007)
Cohort 1991	–.121**	–.068**	–.162**	–.084**
	(.041)	(.013)	(.025)	(.007)
Observations	110,507	189,923	243,528	325,690
R-squared	.12	.11	.26	.25

	Poor predicted			
Comparison	Immigrants 75/80		Natives	
	Log(y)	Empl.	Log(y)	Empl.
Cohort 1988	–.042	–.040**	–.031	–.048**
	(.027)	(.006)	(.021)	(.006)
Cohort 1989	–.077	–.078**	–.060**	–.085**
	(.039)	(.006)	(.021)	(.006)
Cohort 1990	–.155**	–.112**	–.120**	–.118**
	(.057)	(.007)	(.029)	(.007)
Cohort 1991	–.166*	–.098**	–.115**	–.098**
	(.071)	(.007)	(.027)	(.007)
Observations	82,727	146,415	115,832	179,520
R-squared	.06	.13	.12	.24

Notes: Estimates of average differences between refugee cohorts (1987 reference), outcomes 5–7 years after immigration. Regression models also include controls for calendar year, years-since-migration (dummies), a group fixed effect (refugee-comparison), and individual characteristics (age, age squared, gender, marital status, gender*marital status, and country of origin). Full estimation results are available upon request. The impact of individual characteristics is allowed to differ between refugees and the comparison group. The standard errors are robust and clustered by individual. * (**) denotes significance at the 5(1)-percent level.

Studying rather than entering the labor market may have been the primary target of some individuals included the sample. To test if this affects the findings we restricted the refugee sample to those aged at least 26 at immigration. Table A 1 (headings (2) and (3)) shows that the magnitude of the estimated effects is somewhat larger with this restriction. This is what one would expect to find if each cohort contains a similar fraction of young people that will study regardless of work opportunities.

We also experimented with increasing the number of individual characteristics. Specifically, we included controls for type of refugee status[20] and time spent in Sweden before receiving a residence permit. The latter variable does not exhibit much correlation with the outcome variables, but including the dummies for different refugee categories made the 1989 cohort look somewhat more similar to the 1990–91 cohorts than in the baseline estimates. However, the overall pattern of the findings remained.

In section 4.3 we argue that which local labor market the individual was placed in may have affected outcomes. If later cohorts more often started out in bad locations, this could give a false impression of an impact of the state of the national labor market. We therefore included a set of dummies to control for the initial distribution over local labor markets (all variables were set to zero for the control group). This had virtually no effect on the estimates.

If we alter the threshold for being classified as employed—and thereby included in the earnings regression—from 0 to SEK 36,900 or SEK 100,000 respectively[21], we get the same qualitative pattern across cohorts as in Table 4, but the point estimates are in general smaller the higher the threshold.[22]

Performing the analysis by gender reveals that the cohort differences are very similar for males and females in the refugee sample (i.e. with no control for the macro-effect). The results are also qualitatively the same when natives are used as comparison group. However, the earnings specification using

[20] The reason for not including this variable in the baseline specification is because it is unclear whether individuals change their reason for applying for asylum based on changing governmental practices (to increase their probability of being granted a residence permit) or if it captures actual differences in the characteristics of applicants.

[21] Approximately 4,000 and 9,000 euros. The threshold of 36,900 is the level of the so-called "base amount", which determines e.g. eligibility for social assistance.

[22] Using the higher threshold of 100,000 SEK (with the unrestricted "Immigrants 75/80" comparison group) the estimates for the 1990 and 1991 cohorts are –.053 (.013) and –.040 (.015) in the earnings specification. The corresponding employment estimates are –.041 (.010) and –.029 (.013).

1975/80 immigrants to control for general time effects suggests that the development in the late 1990s differed across gender in this group. This particular set of estimations shows no significant cohort differences for females, but strong effects for males. We have no good explanation as to why the general time effects should be so different in these two subgroups. The effects on employment remain significant and are of the same order of magnitude as for males.

There are also more fundamental questions about the empirical strategy. One issue concerns the potential effect of cohort size on economic performance. However, our results suggest that cohort performance does not seem to follow cohort size. The 1991 cohort is larger than the 1990 cohort, and presumably met worse conditions upon arrival; still, labor market performance is similar in the two cohorts. A similar objection is that the labor market may become saturated once the stock of immigrants becomes large enough, and that this, rather than the economic downturn, is the factor behind our findings. Even though it is impossible to rule out this possibility, we are inclined to believe that a "saturation effect" is not responsible for a major part of the estimated effects.[23]

Another topic is our implicit assumption of no, or similar, selection of immigrants. One fact that signals that this may not be a big problem is the time pattern of refugee applications in Sweden compared to the rest of Western Europe and the World. In the later part of the 1980s the relative number of applications submitted in Sweden fell at the same time as the economy was booming. In 1992 it rose drastically despite the economic downturn in Sweden. If changes in relative labor market opportunities between countries were an important determinant of refugee destinations, this pattern would be unlikely to occur.

Some researchers find that structural change explains the decline over time in immigrant labor market outcomes (Rosholm et al. 2001, Bevelander 2000). For such changes to explain our results, there has to be a very rapid structural change that only affects refugees and not the comparison groups. It seems unlikely that this is the explanation to the patterns in the data.

[23] If one is willing to draw inference from the local to the national level, results presented in section 4.3 support this view. We there find an effect of initial local conditions, controlling for average differences across cohorts (which eliminates saturation effects).

The results of this section show that the state of the national labor market at immigration matters both for employment and earnings. Cohorts that were able to enter and spend some time in a good national labor market perform better in a medium-term perspective than do cohorts who were initially exposed to a declining economy. Our next step is to see if there in addition to this national effect is an impact of initial local conditions. This is the issue of section 4.3.

4.3 The local level

This section analyzes the second question posed in the introduction: the effect of local labor market conditions. By exploiting the governmental refugee placement policy as a natural experiment giving exogenous variation in initial locations, we investigate the effect of entering in different local labor markets.

After a short description of the empirical models, the section presents results from a model including only measures of local conditions at the time of entry into the labor market. [24] This model estimates the total effect of starting out in a bad labor market, and is probably the most interesting one from a policy perspective. We then present results from a model that also includes contemporary local conditions. The latter specification is an attempt to identify whether the baseline model captures scarring and/or geographic immobility.

4.3.1 The empirical models

This subsection provides a short description and discussion of our empirical strategy, the assumptions it rests on, and ways of testing its potential problems. Let us first briefly discuss the choice of local labor market variables. The study operationalizes local labor market conditions by the local unemployment rate. We have elaborated with different sets of variables, but found the unemployment rate or alternatively the non-employment rate to be the most important factor.[25] Other variables (vacancies, job creation and destruction rates) did not provide any further information when the unemployment rate was included. Given this and that most previous studies have used unemployment rates, we proxy local conditions by the unemployment rate.

[24] Year of entry is either the year the person received a residence permit and was allowed to start a job, or two years later (when most people had finished their introduction programs). We will return to this issue below.

[25] In fact, unemployment and non-employment rates gave very similar results (when being standardized).

As already mentioned, the baseline specification includes the local unemployment rate at the time of labor market entry. In a second type of specification we also include measures of contemporary unemployment, i.e., at the time of observation. Consider estimation of the following model

$$Y_{ij(t+k)} = \beta' X_{i(t+k)} + \gamma' C_i + \delta U_{j(t+k)} + \eta_i + \varepsilon_{ij} \quad (2)$$

where $Y_{ij(t+k)}$ is the outcome variable for person *i* living in region *j*, *k* years after immigrating (receiving a permanent residence permit) at time *t*. $X_{i(t+k)}$ is a vector of observed individual characteristics, C_i is a vector of cohort (and in this setup calendar time) effects, and $U_{j(t+k)}$ is the unemployment rate in region *j* at time *t*+*k*, while β, γ, and δ contain the parameters to be estimated. η_i is unobserved individual heterogeneity, and ε_{ij} is an idiosyncratic error term.

It is clear that $U_{j(t+k)}$ may be endogenous, i.e., correlated with η_i. Our approach is to instrument this variable with the current unemployment rate in the region of placement: $U_{j^*(t+k)}$. Under which conditions is this instrument valid? By assumption (see discussion in section 2), placement was independent of η_i. Thus, $\mathrm{cov}(U_{j^*(t-1)}, \eta_i) = 0$, i.e., there is no correlation between unobserved individual factors and the pre-entry local unemployment rate. This means that we do not have to worry about sorting into locations based on different labor market aptitudes among individuals.

Further, the contemporary unemployment rate in the placement region can be decomposed into

$$U_{j^*(t+k)} = U_{j^*(t-1)} + \Delta U_{j^* k+1}, \quad (3)$$

i.e., the pre-entry unemployment rate plus the change in local unemployment over time. This says that the instrument is valid unless $\mathrm{cov}(\Delta U_{j^* k+1}, \eta_i) \neq 0$; put another way, if there is an association between unobserved factors that are not captured by the variables included in $X_{i(t+k)}$ and the change in local unemployment. We believe that this is only likely to be the case if, somehow, the inflow of refugees to a region affects the level of regional

unemployment. Note, though, that this effect must come from unobserved characteristics of the refugees.

The potential effect of the refugee inflow is probably dependent on the size of the inflow relative to the size of the region's working-age population. In 1989, the year with the highest number of residence permits, the average relative inflow was only 0.3 percent. However, the largest inflow amounted to 1.5 percent of the region's working-age population, which potentially could have had a significant impact on local unemployment.

We have employed two strategies to investigate how the potential impact of the refugee inflow on unemployment could bias our results. The first method was to use unemployment at t–1 instead of t; the idea being that the inflow of refugees could not have affected time t–1 unemployment. The second method was to drop ten percent of the sample who were placed in regions with the highest relative inflow during the period 1987–91, where we expect the endogeneity problems to be biggest. Both these tests yielded results very similar to the ones presented below. We take this as evidence that the inflow of refugees did not affect unemployment rates in a way that compromises our empirical models. Hence, the local rate of unemployment at labor market entry (time t) is regarded as exogenous and therefore specifications using unemployment measures for time t are estimated by OLS. To use local unemployment rates at t rather than at t–1 is preferred since this is the local conditions one actually meets if entering the labor market directly after receipt of a residence permit.[26]

Further, it seems natural to consider labor market conditions at time t, the year of immigration, as a benchmark case. However, if labor market entry was in fact delayed by the integration plans, one would instead like to use the local unemployment rate at a later point in time. When the actual initial labor market entry occurs is not known and can therefore only be arbitrarily chosen. As mentioned in section 2, the integration plans on average lasted about one and a half years.[27] Therefore a lead of two years is used in alternative specifications; for example, for those who received a residence permit in 1987 we use local un-

[26] Also, if using unemployment rates at t–1 we would need to make an assumption on the persistence of local unemployment rates over time since local unemployment rates in t and t–1 are not perfectly correlated.

[27] Rooth (2001) shows that the average time from receiving a residence permit until finding a first job is approximately 1.5 years for the cohort arriving in 1987 and 2 years for the cohort arriving in 1991. We have experimented with leads of one and three years, and received results similar to those presented.

employment in 1989. For the same reasons as for contemporary local unemployment, the local unemployment rate at t+2 might be endogenous. This problem is handled by the above-mentioned instrumental variable procedure using the unemployment rate at t+2 in the region of placement as an instrument. Therefore, all models including local unemployment rates measured in later periods than t are estimated using 2SLS. Due to the need for IV, the employment specifications are estimated using the linear probability model.[28]

4.3.2 The total effect of initial local conditions

We now go on to present the results from the baseline models. Through the longitudinal nature of the data, it is possible to see how the effects of initial local conditions develop over time, i.e. what is the medium and long-term impact of the local labor market one is initially placed in. It is important to discuss what these estimates actually capture. Since we start measuring earnings and employment five years after immigration, the estimates do probably not measure direct effects of initial local conditions. They are more likely to be sums of a number of indirect effects. One of these effects stems from a higher risk of having experienced unemployment if the person was initially living in a poor local labor market (scarring). Another comes from an increased probability of still living in a bad region (if bad regions tend to persist over time). The effect of initially being placed in a region with poor conditions that we estimate in this section is the sum of these indirect effects.

Table 5 presents results from estimations of the following model

$$Y_{ij(t+k)} = \beta' X_{i(t+k)} + \gamma' C_i + \delta U_{j(t+l)} + \varepsilon_{ij} . \qquad (4)$$

The variables are the same as in (2), l is either 0 or 2, and k goes from 5 through 10 across the columns of the table. We pool the five refugee cohorts together; cohort/time fixed effects are included to control for cohort differences.[29] The effect of local conditions at entry is therefore identified both from variation in unemployment across labor markets as well as from variation in the unemployment rate within labor markets over time acting beyond the average

[28] Doing IV regressions in a non-linear context relies on the use of estimators, whose properties are not yet well-established (see for instance Mullahy, 1997, for an early example of such models).

[29] In a panel dataset like ours it is not possible to separately identify cohort fixed effects from time fixed effects. Hence, by using cohort fixed effects also time effects are controlled for.

time effect.[30] We first report the results from the OLS estimations, i.e. using the local unemployment rate in the year of immigration, and then the results from the instrumental variable estimations, i.e., using a lead of two years for the local unemployment rate. [31] Note that the sample sizes decrease substantially after *t*+7, since fewer and fewer cohorts are included in the estimations.[32] Because of the change in sample composition, one should be cautious in interpreting the development of the estimated effects over time.

The results for earnings suggest a very stable relationship over time with the IV estimates being somewhat larger in magnitude than the OLS estimates. The reason for this difference in magnitude is probably, as argued above, that the local unemployment rate during the first year is less important than the one in the third year (*t*+2) due to that few refugee immigrants actually enter the labor market within the first year.[33]

Assuming that the local labor market two years after entry is the correct measure of initial local labor market prospects, only the IV estimates are discussed. The results imply that a refugee that is placed in a region with one percentage point higher unemployment than another region suffers a 13–17 percent earnings penalty, ceteris paribus. This earnings penalty also seems to be present in every observation period within a ten-year period from receiving a permanent residence permit. It should be stressed that the earnings disparities do not solely reflect (wage) differences across full-year workers, but also employment variation among people with at least some earnings.

Similar results are obtained for employment outcomes. Again the IV estimates are larger in magnitude than the OLS estimates and imply that a refugee starting out in a region with one percentage point higher unemployment gives

[30] We have experimented with a specification including both regional and cohort/time fixed effects. However, the differences in within-region variation in unemployment over time appear to be too small to identify any effects of initial conditions.

[31] The standard errors are corrected using the above-mentioned "cluster" command in Stata (see Moulton, 1990, for a discussion of problems with using aggregate explanatory variables on micro data).

[32] Since the cohorts only are observed until and including 1998 the estimates in *t*+8 to *t*+10 are not based on data including all cohorts. *t*+8 does not include the cohort receiving a residence permit in 1991, *t*+9 does not include the cohorts receiving a residence permit in 1990 or 1991, while *t*+10 only include the cohorts receiving a residence permit in 1987 and 1988.

[33] This interpretation is supported by the fact that OLS estimates using the local unemployment rate two year after entry are similar in magnitude to the IV estimates in Table 4. This fact also suggests that non-random relocations within the first three years is not an important issue affecting the results.

him/her a 3–5 percentage point employment penalty, ceteris paribus, which again is persistent over time. In the period 1992–98, the difference in the local unemployment rate between the median person and the person at the 10th (or 90th) percentile was roughly one percentage point.

Table 5 The effect of initial local unemployment on log earnings and on employment.

	t+5	*t* +6	*t* +7	*t* +8	*t* +9	*t* +10
Log earnings:						
OLS	–9.2**	–8.4**	–8.0**	–5.4**	–6.8**	–4.8**
	(2.8)	(2.5)	(2.6)	(1.8)	(1.9)	(1.6)
IV	–17.0**	–15.3**	–13.3**	–13.5**	–13.6**	–13.2**
	(4.3)	(3.7)	(3.0)	(2.9)	(3.4)	(4.9)
# observations	24,636	24,417	24,416	20,278	17,633	9,995
Employment:						
OLS	–1.3	–1.7*	–1.6	–1.7*	–1.6*	–2.3**
	(0.7)	(0.9)	(0.9)	(0.8)	(0.8)	(0.8)
IV	–3.4**	–3.4**	–3.9**	–3.7*	–4.6*	–7.2*
	(1.2)	(1.2)	(1.3)	(1.9)	(1.9)	(2.9)
# observations	46,215	45,722	45,220	37,060	30,987	17,078
Share with positive earnings	0.53	0.53	0.54	0.55	0.57	0.59

Notes: Table entries show estimates of the effect of local unemployment (percent) on log earnings and the estimates (from a linear probability model) of local unemployment (percent) on the probability to be employed. Log earnings are calculated conditional on having positive earnings. Employment is defined as one if having positive earnings and zero otherwise. Regression models also include controls for cohort/time effects and individual characteristics (age, age squared, gender, marital status, gender*marital status, and country of origin). Full estimation results are available upon request. The number of observations decreases mainly because cohorts can be followed for different periods; e.g. the 1991 cohort is included through T+7. * (**) denotes significance at the 5(1)-percent level.

As mentioned above, analyzing separate cohorts can give a picture of whether local labor market conditions at labor market entry matter more in a boom or in a recession of the economy. The 1987 and 1988 cohorts entered the labor market in a boom while the others entered it in a recession (if the assumption of a two-year lag before entering the labor market is correct). The results

from separate estimations by cohort do not support the idea of different effects. There is no clear order between cohorts in the magnitude of the estimates (see Tables A2 and A3 in the appendix). According to these results, it does not seem to make a difference (in the long run) whether one is being placed in a "bad" region in a boom or in a recession.

The presented estimations use the unemployment rate to capture local labor market conditions. Since data for several years are included, this implicitly assumes that one percentage point higher unemployment rate has the same effect in one year as in another. We have experimented with other ways of defining the unemployment variable: (i) relative to the average regional unemployment rate ($\frac{u_j}{\bar{u}}$) for each year and (ii) expressed in standard deviations of regional unemployment for the particular year. These variations gave results that were qualitatively the same as those presented in the paper.

Finally, let us briefly discuss some other robustness checks. First, two alternative thresholds (arbitrarily chosen) for being classified as employed were tested: SEK 36,900 and SEK 100,000. The effect on (log) earnings is found to be less negative the higher the threshold, while the opposite is true for the effect on employment.[34] Hence, the effect is to a large extent generated by the problems of finding stable employment in regions with high rates of unemployment.

In another specification municipalities were used instead of labor market regions as geographic units. Those results show very similar effects of initial conditions as compared to the ones in Table 4. Following the same line of reasoning as in section 4.2, those less than 26 years of age at immigration were excluded from the sample to see whether voluntary participation in education is likely to affect our results. Again the results are qualitatively similar to the baseline case. Separate regressions for men and women also reveal similar estimates across gender.

4.3.3 The underlying mechanisms

In this subsection we attempt to separate the effect of contemporary local unemployment from the effect of the local unemployment rate at entry. If initial unemployment has no effect when contemporary unemployment is included in

[34] This is somewhat in contrast to the findings of section 4.2, where estimates on both earnings and employment tended to decrease with higher thresholds.

the model, it suggests that the effect found in the previous subsection stems from an increased probability of remaining in a persistently bad region. Otherwise, some type of scarring is likely to account for at least part of the effect.

The model specification of this section adds a contemporary local unemployment variable to equation 4. The contemporary local rate of unemployment is likely to be endogenous due to nonrandom relocations and therefore needs to be instrumented. This is made possible by using the local unemployment rate at time ($t+k$) in the region one was initially placed in as an instrument for the local unemployment rate at time ($t+k$) in the contemporary region.[35]

The results in Table 6 are found using a specification instrumenting both the initial local unemployment rate at $t+2$ as well as contemporary local unemployment. The results show that initial local unemployment rates exhibit a more stable correlation with earnings than do contemporary unemployment, while the employment specifications exhibit more mixed results. Interestingly, the estimated effect on earnings is in most cases larger for the initial local unemployment rate (U at entry) rather than for contemporary unemployment. However, even though the simple correlation coefficient between the two local unemployment variables is not extremely high (somewhat less than 0.5), one could worry that the models are unable to separate the effects of the unemployment rates in different periods.[36] Indeed, it seems odd that contemporary unemployment rates would not affect earnings and employment, but that local labor market conditions experienced several years ago would. Still, it is interesting to find that the inclusion of contemporary unemployment does not eliminate the correlation between outcomes and past unemployment rates.

Since the initial and the contemporary local unemployment rate variables have different distributions this could potentially affect our results. Therefore, as a robustness check, we used an alternative specification were both initial and contemporary local unemployment rates were standardized and mean-centered; the results were qualitatively the same.

[35] Without further assumptions, the estimate of the contemporary local unemployment captures a local average treatment effect (LATE). Of course, the same is true for initial unemployment when we use the IV estimator in $t+2$. See e.g. Angrist & Krueger (1999) for a discussion of the LATE estimator.

[36] The data include 102 cross-section observations (local labor markets) observed three to nine years apart.

Table 6 The effect of initial and contemporary local unemployment on log earnings and on employment.

	t+5	*t* +6	*t* +7	*t* +8	*t* +9	*t* +10
Log earnings:						
U at entry	–14.2**	–11.9**	–6.7*	–10.8**	–11.8**	–11.3
	(5.0)	(3.7)	(3.0)	(4.2)	(5.0)	(6.4)
Contemporary U	–3.9	–5.3*	–12.0**	–3.9	–2.9	–3.3
	(3.3)	(2.6)	(3.5)	(3.8)	(5.6)	(6.9)
# observations	24,636	24,417	24,416	20,278	17,633	9,995
Employment:						
U at entry	–2.3*	–1.7	–3.2*	–1.9	–2.5	–7.3*
	(1.1)	(1.1)	(1.2)	(1.6)	(1.9)	(3.2)
Contemporary U	–1.8*	–3.0**	–1.5	–3.2**	–3.7**	0.1
	(0.9)	(1.0)	(1.1)	(1.2)	(1.4)	(2.1)
# observations	46,215	45,722	45,220	37,060	30,987	17,078

Notes: Table entries show estimates of the effect of initial local unemployment at *t*+2 (percent) and contemporary local unemployment (percent) on log earnings and the estimates (from a linear probability model) of the effect of initial local unemployment at *t*+2 (percent) and contemporary local unemployment (percent) on the probability to be employed. We use the above-mentioned instrumental variable approach to deal with endogeneity in initial as well as contemporary local unemployment. Log earnings are calculated conditional on having positive earnings. Employment is defined as one if having positive earnings and zero otherwise. * (**) denotes significance at the 5(1)-percent level.

To conclude, local labor market conditions at arrival appear to matter for the success on the labor market for refugee immigrants. We find a stable long-term impact of which local labor market one initially enters in. There are effects both on the probability to work and on earnings among workers. Robustness checks imply that much of the effect comes from different chances to get stable employment rather than wage differences among full-year workers.

Attempts to identify the underlying mechanisms suggest that the effect does not solely work through an increased likelihood of remaining in regions where unemployment is persistently high. Even though there might be issues of separability, estimations indicate an effect of initial labor market conditions also when we include controls for contemporary conditions. Some sort of scarring effect is therefore a possible explanation at least to some part of the effect of initial local labor market conditions.

5 Concluding remarks

This paper investigates the effects of labor market conditions at the time of immigration on immigrant earnings and employment. We consider effects at two levels: the state of the national labor market and local conditions where one resides initially.

Previous studies have been unable to handle potential problems arising from selective migration; i.e., individuals arriving in good times may be different from those arriving in bad times. Presumably, this is the factor behind findings implying that bad labor markets at the time of arrival could be associated with better long-term outcomes.

Our analysis at the national level studies five cohorts of refugee immigrants who, due to an unexpected severe recession, met very different labor markets during their first years in Sweden. The results suggest that initial conditions have a substantial impact on medium-term earnings and employment. Cohorts where most individuals had entered before the recession are 7–9 percentage points more likely to be employed, and have about 12–18 percent higher earnings than individuals who entered in a deteriorating labor market.

The refugee cohorts in our sample were subjected to a placement policy in which the government assigned people to an initial location. We use the policy as an exogenous source of variation that enables us to estimate effects of initial local conditions. The findings go in the same direction as for the national level. Meeting a bad labor market at arrival leaves traces on earnings and employment for at least ten years.

The total effect of starting out in a region with poor prospects can depend on two basic mechanisms: a geographic "lock-in" effect and some type of scarring. According to our estimations, initial unemployment rates have an effect on outcomes also when contemporary regional unemployment is included among the regressors. This hints that the total effect is at least not fully a result of people remaining in persistently bad regions.

To sum up, our results demonstrate that refugees arriving in recessions can be expected to have disadvantages in terms of lower earnings and employment for several years compared to refugees arriving in better times, who on average also have a weak position in the labor market. Governments often adopt policies to control the geographic distribution of newly arrived immigrants. Focusing on regions with favorable labor markets could improve the odds for suc-

cessful labor market integration and affect earnings and employment for at least a decade.

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Appendix

Description of variables

Individual variable	Definition	Source
Earnings	Sum of annual income from wage work or business. Deflated by the CPI to 2001 prices.	FLYDATA (ref.), LINDA (comp.)
Employment	Equals 1 if earnings>0, 0 otherwise.	
Female	Equals 1 if female, 0 if male	FLYDATA (ref.), LINDA (comp.)
Age	Age at December 31 in the observation year.	FLYDATA (ref.), LINDA (comp.)
Married	Equals 1 if married, 0 otherwise	FLYDATA (ref.), LINDA (comp.)
Education	Highest level of completed education, before immigration for refugees, at time of observation for comparison groups. Categories: <9 years, 9–10 years, High school ≤2 years, High school >2 years, University <3 years, University ≥3 years. Estimations include dummies for each level.	FLYDATA (ref.), LINDA (comp.)
Country of origin	Birthplace. Estimations in 4.3 include dummies for each country. In 4.2, countries with less than 20 observations (in all cohorts) are defined as one category.	FLYDATA (ref.), LINDA (comp.)
Years since migration	Observation year – immigration year. Estimations include dummies for each year, set to zero for natives.	FLYDATA (ref.), LINDA (comp.)
Observation year	Estimations include dummies for each observation year.	FLYDATA (ref.), LINDA (comp.)
Refugee cohort	Dummies for refugees receiving residence permit in year x.	FLYDATA
Time waiting	Years in Sweden before receiving residence permit.	FLYDATA
Visa status	Refugee categories include: Convention, Conscientious objectors, De Facto, Quota, Humanitarian, and General Decision.	FLYDATA

Local variable	Definition	Source
Local unemployment	# unemployed / pop aged 18–64	The National Labour Market Board (Ams) (unempl), Statistics Sweden (SCB) (pop.)
Local non-employment	1– (# employed / pop aged 18–64)	Statistics Sweden (SCB) (empl.), IFAU (pop.)
Local vacancy rate	# Vacancies reported to the employment office / pop aged 18–64	Ams (vacancies), SCB (Pop)
Job creation (destruction) rate	The sum of positive (negative) employment changes to one year from the previous year at the plant level within each region, divided by employment in the region.	IFAU
National unemployment level	# unemployed / working-age pop.	Labor Force Surveys (AKU)

Table A 1 The effect of initial conditions in the national labor market, variations.

Group	(1)		(2)		(3)	
	Log(y)	Empl.	Log(y)	Empl.	Log(y)	Empl.
Cohort 1988	–.071**	–.057**	–.046	–.037**	–.055*	–.051**
	(.021)	(.006)	(.027)	(.008)	(.025)	(.007)
Cohort 1989	–.095**	–.094**	–.077*	–.062**	–.105**	–.081**
	(.020)	(.006)	(.030)	(.009)	(.024)	(.007)
Cohort 1990	–.137**	–.123**	–.147**	–.093**	–.181**	–.116**
	(.027)	(.007)	(.042)	(.012)	(.032)	(.008)
Cohort 1991	–.099**	–.096**	–.146**	–.072**	–.198**	–.093**
	(.025)	(.007)	(.046)	(.014)	(.031)	(.008)
Observations	73469	137157	82174	140203	187351	247723
R-squared	.06	.09	.11	.12	.25	.25

Notes: Robustness checks to results presented in Table 3. (1) Refugees only; (2) Age at migration>=26 Immigrants 75/80; (3) Age at migration>=26, natives. Estimates of average differences between refugee cohorts (1987 reference), outcomes 5–7 years after immigration. Regression models also include controls for calendar year, years-since-migration (dummies), a group fixed effect (refugee-comparison), and individual characteristics (age, age squared, gender, marital status, gender*marital status, and country of origin). The impact of individual characteristics is allowed to differ between refugees and the comparison group. The standard errors are robust and clustered by individual.

Table A 2 The effect of the local unemployment rate at entry on log earnings.

	t+5	*t*+6	*t*+7	*t*+8	*t*+9	*t*+10
OLS:						
Cohort 1987	–3.7	–7.7*	–2.6	–2.8	–5.5	–5.5**
	(3.4)	(3.4)	(4.5)	(3.0)	(3.1)	(1.7)
Cohort 1988	–15.4**	–4.7	–6.3	–1.0	–7.5**	–4.3
	(3.4)	(3.0)	(3.6)	(2.4)	(2.4)	(2.8)
Cohort 1989	–8.9	–11.5**	–13.6**	–11.4**	–8.2**	–
	(5.4)	(3.5)	(3.5)	(3.0)	(2.6)	
Cohort 1990	–15.1*	–7.6*	–11.0*	–4.9	–	–
	(4.5)	(3.6)	(4.5)	(3.6)		
Cohort 1991	–7.2	–9.8*	–8.9**	–	–	–
	(4.1)	(3.8)	(3.2)			
IV:						
Cohort 1987	–5.1	–25.3*	4.2	–2.3	–16.6	–22.1**
	(11.1)	(10.3)	(19.0)	(11.1)	(10.7)	(8.1)
Cohort 1988	–40.8**	–14.8**	–16.2**	–3.9	–16.5*	–9.3
	(12.1)	(5.4)	(5.4)	(4.8)	(6.4)	(5.2)
Cohort 1989	–19.1**	–17.8**	–18.0**	–19.6**	–11.9**	–
	(7.1)	(5.8)	(5.9)	(4.7)	(3.7)	
Cohort 1990	–19.3*	–15.5*	–16.2*	–15.4*	–	–
	(7.5)	(7.0)	(7.7)	(6.8)		
Cohort 1991	–5.8	–11.3**	–11.3**	–	–	–
	(4.3)	(4.0)	(3.8)			
# obs. 1987	5,690	5,183	5,003	5,072	4,963	4,859
# obs. 1988	5,159	5,137	5,214	5,054	5,053	5,136
# obs. 1989	7,367	7,598	7,424	7,334	7,617	–
# obs. 1990	2,673	2,648	2,655	2,818	–	–
# obs. 1991	3,747	3,851	4,120	–	–	–
Share with positive earnings						
Cohort 1987	0.66	0.61	0.60	0.61	0.60	0.60
Cohort 1988	0.55	0.55	0.56	0.55	0.56	0.57
Cohort 1989	0.51	0.54	0.53	0.53	0.56	–
Cohort 1990	0.45	0.45	0.46	0.49	–	–
Cohort 1991	0.47	0.49	0.53	–	–	–

Notes: Table entries show estimates of the effect of local unemployment (percent) on log earnings. Log earnings are calculated conditional on having positive earnings. * (**) denotes significance at the 5(1)-percent level.

Table A 3 The effect of the local unemployment rate at entry on employment.

	t+5	*t*+6	*t*+7	*t*+8	*t*+9	*t*+10
OLS:						
Cohort 1987	–0.8	–1.4	–2.1*	–2.4**	–2.2**	–2.5**
	(0.6)	(1.0)	(0.8)	(0.7)	(0.7)	(0.8)
Cohort 1988	–0.1	–1.6	–2.3*	–2.6*	–2.1	–2.2*
	(1.2)	(1.0)	(1.1)	(1.1)	(1.2)	(1.0)
Cohort 1989	–1.5	–1.0	–0.3	–0.6	–0.8	–
	(0.8)	(0.9)	(1.1)	(0.8)	(0.9)	
Cohort 1990	–0.7	–1.7	–1.1	–0.8	–	–
	(1.0)	(1.2)	(1.2)	(1.2)		
Cohort 1991	–3.7**	–3.2**	–2.0*	–	–	–
	(0.9)	(1.0)	(1.0)			
IV:						
Cohort 1987	–3.0	–2.9	–7.3*	–7.7	–6.2	–9.0*
	(1.9)	(2.8)	(3.4)	(4.0)	(4.2)	(4.2)
Cohort 1988	–0.8	–4.4*	–6.5*	–7.3*	–7.1*	–6.6*
	(3.3)	(2.2)	(3.4)	(3.3)	(3.1)	(3.0)
Cohort 1989	–3.5*	–4.0**	–3.2	–2.9	–2.9	–
	(1.6)	(1.5)	(1.7)	(1.6)	(1.6)	
Cohort 1990	–1.8	–2.2	–3.0	–0.1	–	–
	(1.9)	(2.0)	(2.1)	(2.1)		
Cohort 1991	–4.7**	–3.1**	–2.9*	–	–	–
	(1.0)	(1.1)	(1.1)			
# obs. 1987	8,557	8,473	8,384	8,306	8,234	8,142
# obs. 1988	9,424	9,318	9,231	9,145	9,056	8,936
# obs. 1989	14,383	14,199	14,047	13,863	13,697	–
# obs. 1990	5,951	5,885	5,813	5,746	–	–
# obs. 1991	7,900	7,847	7,745	–	–	–

Notes: Table entries show estimates (from a linear probability model) of local unemployment (percent) on the probability to be employed. Employment is defined as one if having positive earnings and zero otherwise. * (**) denotes significance at the 5(1)-percent level.

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