Local Labor Markets and Earnings of Refugee Immigrants

Anna Godøy*

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Abstract

This paper estimates how local conditions at the time of immigration influence later outcomes for refugee immigrants to Norway, exploiting the quasi-experimental nature of the Norwegian system for settlement for "quota" or resettlement refugees. Resettlement refugees are selected and processed for immigration before arrival in Norway, and settled directly in a municipality, making initial location as good as random conditional on observable characteristics.

Being placed in a labor market where other non-OECD immigrants do well increases own annual labor earnings up to 10 years after immigration. Extended models suggest that this effect is not driven by individual scarring effects: when controlling for the contemporaneous employment rate in the assigned region, effects of initial conditions disappear or turn negative. Rather, the effects appear to be due to persistence in local labor market conditions combined with limited geographical mobility in response to adverse labor market conditions.

Keywords: immigration, settlement policies, location choice, labor market outcomes

JEL Classification Numbers: J15, J18, J61, R23

1 Introduction

This paper examines how initial assignment of settlement area affects the later labor market outcomes of refugee immigrants. We can think of two distinct ways in which local labor market conditions can affect later outcomes. First, there could be effects through persistence on the individual level, i.e. effects on early experience or individual scarring effects of unemployment. In this case, people who are placed in a bad labor market will gain less early experience, accumulate

*Institute for Social Research. a.a.godoy@socialresearch.no. This research is part of the project "Social Insurance and Labor Market Inclusion in Norway" financed by the Norwegian Research Council (grant 202513).

less country specific human capital, which in turn will make them do worse in the labor market in the future, regardless of the later state of the labor market. Second, there could be effects through a persistence on the local level combined with limited geographical mobility. In that case, people who are placed in a bad labor market will be more likely to experience difficult conditions later, even if there are no effects on them as individuals. For both mechanisms, identifying the causal effects of labor market conditions is problematic for persons who freely choose when and where to migrate. For example, simply comparing average earnings for persons immigrating to high and low unemployment region would exaggerate the actual effects of settling in a region with poor job prospects if persons with higher earnings potential systematically select themselves into more favorable locations.

The population studied in this paper is so-called resettlement refugees, who are cleared for immigration before arrival to Norway and settled directly in a municipality upon immigration. As the settlement decision is made while the individual is still abroad, there is little possibility to self-select based on unobservable characteristics. The identifying assumption throughout this paper is that settlement decision is as good as random, at least conditional on observable characteristics such as nationality and family size. The quasi-experimental nature of this scheme allows for the identification of causal effects of local labor market conditions at the time of arrival.

Using Norwegian administrative register data, employment outcomes are observed for the first ten years after immigration. These records can be linked to data on local conditions in the assigned labor market region. Using microdata, local employment rates can be computed for different demographic groups.

The basic models estimate total effects of these characteristics in the initial region the year of settlement on later earnings. The model predicts that being settled in a labor market region with high immigrant employment rate increases later earnings up to 10 years after immigration. Extended models suggest that this effect is not driven by individual scarring effects: when controlling for the contemporaneous employment rate in the assigned region, effects of initial conditions disappear or turn negative. Rather, the effects appear to be due to persistence in local labor market conditions combined with limited geographical mobility in response to adverse labor market conditions.

The paper is closely related to Åslund & Rooth (2007) who use a similar settlement program in Sweden to assess persistent effects of local unemployment on earnings and employment. Higher initial unemployment leads to reduced earnings and employment (measured by positive earnings) up to 11 years after immigration. The authors suggest one potential explanation could be scarring effects – poor initial conditions leading to early unemployment which is perceived by later employers as a bad signal. However, when estimating an IV model where lagged experience is instrumented by the initial unemployment rate, IV estimates are larger than OLS estimates, suggesting an independent effect of initial unemployment rate. When controlling for the current local unemployment rates, estimates drop and in some cases become insignificant, consistent with the presence of geographical lock-in effects. The program con-

sidered by Åslund & Rooth (2007) is of somewhat wider scope than the one used in this paper, as it covers all refugees, including persons who spend some time in a reception center while their immigration application was being processed. In this sense, the data used in the current paper provide a cleaner experiment, as it is more likely that local conditions are actually the first conditions experienced by new immigrants, and the scope for self-selection is reduced.

The paper is related to a large literature on persistent effects of labor market conditions at the time of labor market entry on earnings. Long term effects of initial unemployment could occur for instance if there are scarring effects of unemployment (Ruhm 1991). Papers studying the effects of college students graduating in a recession find effects up to ten (Oreopoulos et al. 2012) or even twenty years (Kahn 2010). Similar effects are found in Norwegian data (Raaum & Røed 2006) Evidence on the impact on immigrants is less clear cut: Chiswick et al. (1997) using repeated cross sections of US microdata find no evidence of negative effects of immigrating during periods of high unemployment. If anything, arriving during periods of high unemployment is associated with higher employment rates, possibly due to immigrants in a recession being positively selected. The paper by Åslund and Rooth discussed above examining both the effect of arriving during a recession and arriving in areas with high local unemployment controlling for calendar time effects, finds significant effects of initial unemployment on immigrant earnings and employment. Evidence on Norwegian data also finds earnings of immigrants from non-OECD countries to be more sensitive to unemployment rate with estimated elasticities of earnings with respect to local unemployment three times as large for this group compared to natives (Longva & Raaum 2002).

The rest of this paper is organized as follows. Institutions and data are presented in section 2. The basic model is presented in section 3. Results are discussed in section 4 together with model extensions to shed some light on possible mechanisms. To get an impression of the quantitative implications of the estimated effects, section 5 presents some simple policy simulations. Section 6 concludes the paper.

2 Institutions and data

This paper uses a settlement policy for resettlement refugees to identify effects of local labor market conditions on later outcomes. Here, the settlement program is described in some detail. Next, the sample selection criteria are presented together with a descriptive overview of the data used in the main analysis.

2.1 The settlement program

Each year, the Norwegian parliament sets a quota of resettlement refugees, currently at 1200 persons a year. Selection of refugees is done abroad by the UN refugee agency and Norwegian immigration authorities. Crucially, resettlement

refugees are settled in a municipality directly after arrival to Norway, the settlement decision being made before the arrival.

In the present paper, the settlement policy for resettlement refugees is used to identify causal effects of local labor markets on later individual earnings. The identifying assumption required for this interpretation is that the initial location of each person in the sample is random, conditional on a set of observable characteristics that include year of immigration, country of origin, age, gender and family characteristics. This identifying assumption can in turn be formulated as two requirements. The first requirement is random assignment: assigned location should be random, conditional on the observable characteristics we have in the dataset. Second, we need compliance: the initial location should be the assigned location. As there is limited formal documentation on the details on settlement policies, information on the workings of the settlement program has been collected from correspondence with the Directorate of Integration and Diversity (IMDi), the agency responsible for implementing refugee settlement.

In this context, random assignment requires that the assigned municipality is uncorrelated with unobserved characteristics that affect earnings capacity. That is, conditional on age, gender, country of origin and family structure, the allocation of refugees to municipalities should be random. For a large majority of refugees, this appears to be the case. There is no communication between refugee and caseworker before settlement. This greatly reduces the opportunity of the individual refugee to influence the assignment. The settlement decision is final with no opportunity to appeal. However, three exceptions to this rule may be problematic.

First, placement can take into account the individual's educational background or work experience. According to IMDi however, this is rarely relevant for resettlement refugees, who are mostly low skilled and are required to go through qualification and training programs before being qualified for work or regular education. Second, while health information as a general rule is not transmitted to caseworkers, there is an exception for persons with medical conditions that require treatment. For people with complex conditions where treatment is not widely available, this would have an effect on assignment. Again, it is hard to obtain statistics on how many people are affected by this. Third, there is an attempt to settle those who happen to have family or friends already residing in Norway in the same municipality. This would complicate studying effects of ethnic concentration; however it is not clear how this would affect estimation of models of local labor market conditions.

The second element, compliance, means that the observed initial location should correspond to the assigned settlement location. Like other persons holding a valid residence permit, resettlement refugees are free to move wherever they want, i.e. there are no legal barriers to settling anywhere in the country, without government assistance. However, in order to settle in a municipality of their choice, it is a requirement that they should be able to financially support themselves and their family. This will rarely be the case for newly arrived resettlement refugees, who as a general rule are settled upon arrival, with no

intervening stay in reception centers.

The immigration procedure for resettlement refugees differs significantly from the process of asylum seekers, who typically spend a significant amount of time in a reception center while having their application for a residence permit processed. Then, once a residence permit has been issued, there is typically an additional waiting time before being settled in a municipality, on average 4.5 months. During this time, there is arguably room for the more resourceful immigrants to find employment and settle independently, without assistance from the authorities. As a consequence, initial location is less likely to be random for this group. For this reason, asylum seekers are not included in the sample in the current paper.

This distinction is also relevant in relating the present paper to the existing literature based on similar settlement policies, such as Åslund & Rooth (2007). This paper and others are based on Swedish settlement policies which apply to the full refugee immigrant population, including asylum seekers. Concerns have been raised concerning both the randomness of initial assignment – that the requests of the individual refugees were given weight in the decision – and compliance (Nekby & Pettersson-Lidbom 2012). By focusing on resettlement refugees only, the following analysis will hopefully be based on a cleaner policy experiment, though at the cost of a smaller sample size.

2.2 Data

The sample consists of resettlement refugees arriving in Norway between 1993 and 2007. To identify first municipality of residence, I use the municipality of residence the year after migration, as location is missing for most persons the year of migration. This is problematic if people move away from their assigned location before they are observed in the data for the first year. Those few people (59 individuals out of the initial sample of 15,986 resettlement refugees, before age and other sample restrictions) of who cannot be linked to a region of residence the first year after immigration are excluded from the sample.

The sample is merged with individual demographics - country of origin, age, gender, marital status and number of children. Data on education is included for those individuals where it is available in the year of arrival (73% of the sample) in the form of indicator variables for having a completed secondary school degree or a college degree at the time of immigration. Persons younger than 18 or older than 55 the year of immigration are excluded from the sample. 7 individuals are registered with a country of origin that was a member of OECD before 1990 (excluding Turkey)[1]. These observations likely reflect an error in recorded country of origin or refugee status, and are excluded from the sample. The final sample contains 7901 persons.

Individuals are included in the sample for the first 10 years after immigration. Person-years when individuals cannot be found in population residence data are

[1]Countries of origin for the excluded individuals are France, The Netherlands, Great Britain, Switzerland and Germany

removed from the sample as they may have left the country; however no further attempt has been made to identify migration out of Norway to a third country or back to the country of origin, temporary or permanent. For each year, data is added on individual labor earnings, including both wage income and income from self-employment. Average labor earnings are low and a significant fraction of the sample (40% of all person-years) is registered with zero labor earnings in a given year. By using linear earnings, these observations are kept in the sample. As linear earnings is sensitive to the presence of outliers, the 2% highest earnings each year since arrival are censored at the 98^{th} percentile.

In this paper, the geographical units used are labor market regions, an aggregation based on commuting patterns between municipalities, subject to the constraint that regions should be sufficiently large for empirical analysis (Bhuller 2009). There are a total of 46 regions. Having established region of placement, data is linked to a dataset containing local characteristics.

The primary variable of interest is the local immigrant labor market. Throughout this paper, I exclude "OECD immigrants" - immigrants with background from countries that were members of the OECD before 1990[2] from the computation of immigrant-specific rates. The labor market situation of OECD immigrants is more similar to the situation of natives. As Table 1 shows, OECD immigrants are more likely to arrive on work related visas, compared to non-OECD immigrants, taking into account that the around 51% of OECD-immigrants who immigrated for "other" reasons includes a large number of Nordic citizens who do not require a work visa to live and work in Norway.

Table 1: Immigrant Background of OECD, Non-OECD Immigrants

	OECD mean	Non-OECD mean
Refugee	0.00	0.27
Work	0.22	0.22
Family	0.19	0.38
Education	0.07	0.10
Other	0.51	0.02
N	162757	311148

A key question is which measure best captures the local employment prospects of the people in my sample. One possibility is to use the local unemployment rate in the full population. This is problematic if immigrants operate in segments of the labor market that deviate significantly from those of natives.

The newly arrived refugee immigrants in the sample have limited language skills and may also have other difficulties qualifying for available jobs in the Norwegian labor market. For example they may have limited education or health issues that make them unable to apply for many jobs. In other words,

[2]Australia, Belgium, Canada, Denmark, Finland, France Greece, Ireland, Iceland, Italy, Japan, Luxembourg, The Netherlands, New Zealand, Portugal, Spain, The UK, Switzerland, Sweden, Turkey, Germany, USA, Austria.

I worry that a mismatch between the needs of the local labor markets and the qualifications of refugee immigrants may make local unemployment rate a "bad" measure of employment prospects.

One way to investigate this is to use figures on registered unemployment also for different categories of immigrants. A problem with this strategy, however, is that many jobless immigrants have weak incentives to register as a jobseeker. While this measure is likely to be a good reflection of unemployment among people who qualify for unemployment benefits, it is likely to under-report unemployment among persons with low labor market attachment who do not qualify for benefits. A consequence of this is that among demographic groups with low average labor force attachment, such as non-OECD immigrants, low local registered unemployment rate may reflect a bad labor market where few people qualify for benefits, rather than a good labor market where many people are employed.

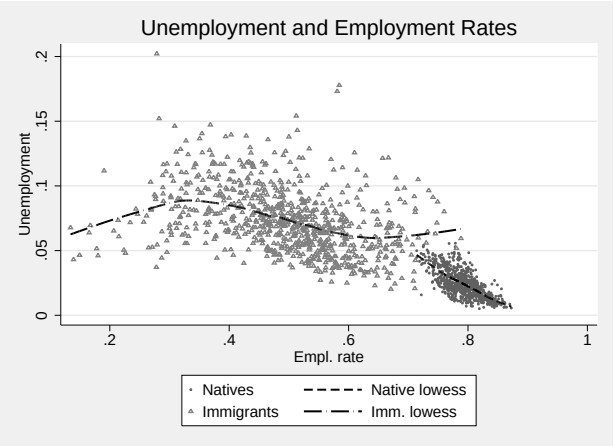
To bypass this problem I include local gross employment rates. I construct local employment rates as the share of residents aged 25-55 registered with earnings at least 2 times the base amount, equivalent to around 25,920 USD in 2009. This threshold implies that many part time workers will be counted as employed, however persons with very low working hours will not be counted. Figures obtained from using this income threshold have been shown to correspond well to employment figures from other sources, such as the Labor Force Survey.

Figure 1 shows the relationship between rates of registered unemployment and computed employment rates for natives and non-OECD immigrants. Local variation in employment rates and rates of registered unemployment is much larger for non-OECD immigrants. For natives, the relationship appears to be fairly linear: low local employment rates are typically associated with higher rates of registered unemployment. For non-OECD immigrants, the correspondence is less tight and appears to be non-monotonic, as some labor market regions have both very low employment rates and low unemployment rates. This could indicate that the labor market attachment of many non-OECD immigrants in these regions is too low to qualify for unemployment benefits. Though computed immigrant employment rate will be the primary variable of interest, model extensions will include the local rate of registered unemployment as an additional control.

In this paper, I am also interested in employment outcomes for immigrants with a similar country background. Looking only at average outcomes in the existing immigrant population from the same country of origin may be difficult as smaller labor market regions may have no or very few existing immigrants from each country. Instead, each country of origin in the sample is placed in one of 12 geographical supranational regions: North Africa, Middle Africa, East Africa, Western Africa, South-East Asia, South Asia, Central Asia, West Asia, East Asia, The Balkans, Europe (Other) and Latin America. These regions are roughly based on the UN Statistics Division's M 49 standard for area codes. Local employment rates are then calculated separately for each macro regions.

Table 2 provides some descriptive statistics. Around 46% are settled in the

Figure 1: Scatter plots of computed employment rates, registered unemployment



Note: Figure shows scatter plots of native/non-oecd-immigrant specific unemployment rates/ employment rates.

labor market regions around the four largest cities - Oslo, Bergen, Trondheim and Stavanger. The majority of people never move to another labor market region - only 34 % of the sample are ever observed living outside the originally assigned region. For those settled in a major city, the rate is even lower, around 16%. The fraction with a higher education degree is 13%.

An important assumption made throughout the paper is that the initial location is uncorrelated with unobserved earnings potential. This assumption cannot be verified empirically using the available data, however we can look at the distribution of observable individual characteristics of people settled in high and low employment regions. Persons arriving in regions where the local employment rate of natives is above the median rate that year are classified in the high employment subsample while the rest are placed in the low employment group.

Table 3 contains average demographic characteristics calculated separately for the two groups, together with estimation results from a joint regression of these characteristics on a dummy for high employment region. The groups appear to be fairly similar in terms of age, gender and family status. There are small, statistically significant differences in country of origin: persons placed in labor market regions with above median immigrant employment rate are slightly more likely to be from Southeast Asia and less likely to be from East Africa. The comparison finds little evidence of sorting on education. People placed in high employment regions are around one percentage point more likely to have a secondary school degree, but the difference is not statistically significant. Note that there are no differences between the two categories when it comes to the fraction with a college degree.

To get a first impression of the data, I do a simple comparison of average

Table 2: Summary statistics

	mean	sd
Age	31.6	8.82
Female	0.44	0.50
Married	0.61	0.49
Any children	0.50	0.50
Number of children (if any)	2.62	1.60
Middle Africa	0.064	0.24
East Africa	0.10	0.31
Southeast Asia	0.19	0.39
South Asia	0.31	0.46
West Asia	0.18	0.38
Other region	0.15	0.36
Secondary school	0.087	0.28
College	0.13	0.34
Years in sample	6.08	2.73
Settled in major city	0.42	0.49
Moved	0.34	0.47
Moved if settled in major city	0.15	0.36
Employment rate, non-OECD immigrants	0.49	0.080
Employment rate, natives	0.80	0.025
Unemployment rate, all	0.026	0.0086
Local population, in 1000s	355.4	459.5
Observations	7901	

Note: Table shows descriptives of refugee immigrants in the main sample. Demographics are observed the first year after immigration to Norway.

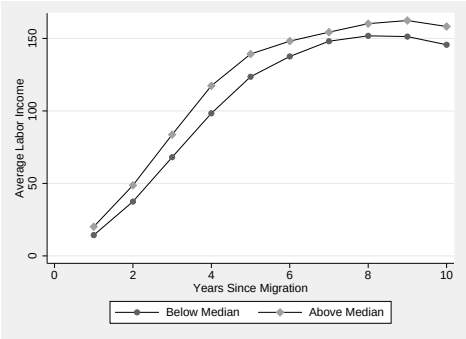
Table 3: Descriptives, by initial location

	Low imm empl	High imm empl	Reg	
	Mean	Mean	Coeff	S.E.
	(1)	(2)	(3)	(4)
Age	31.6	31.6	-0.00384	(0.199)
Female	0.45	0.43	-0.0206	(0.0112)
Married	0.62	0.60	-0.0148	(0.0110)
Children	1.35	1.28	-0.0643	(0.0390)
East Africa	0.13	0.086	-0.0404***	(0.00689)
Southeast Asia	0.17	0.21	0.0396***	(0.00885)
South Asia	0.30	0.31	0.00722	(0.0104)
West Asia	0.17	0.18	0.00906	(0.00866)
Secondary school	0.081	0.092	0.0113	(0.00636)
College	0.13	0.13	0.000983	(0.00766)
Observations	3660	4241	7901	
p			5.05e-09	
Chi square			59.24	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows average demographic characteristics of persons placed in regions with employment rates (at or) above and below the cohort-specific median. Demographics are observed the first year after immigration to Norway. Columns (3) and (4) show results from (jointly) regressing these variables on a dummy for high employment, and chi-square and p-value from a test of significance in all 10 equations.

Figure 2: Average Labor Income, by Initial Immigrant Employment Rate



Note: Figure shows average labor income up to year 10 after immigration for refugee immigrants placed in regions with employment rates (at or) above and below the cohort-specific median.

labor earnings among people placed in high and low immigrant employment regions. Figure 2 plots average labor income for high and low employment rate subsamples. Refugee immigrants placed in regions with above-median employment rates appear to have higher average earnings up to 10 years after immigration. As discussed above, for this group of refugee immigrants region of placement is as good as random, at least conditional on observable characteristics. The differences reported in Figure 2 are therefore potentially interesting as they may reflect long term effects of initial placement. In order to assess this further, the next section presents a simple model to assess effects of initial placement on later earnings.

3 Model

In this section, I set up a simple model relating individual labor earnings to local characteristics in the assigned region. In the basic model, let s denote the year of migration, and let Y_{it} be total labor earnings for individual i in year $t = s + k$, k years after immigration ($k = 2, ..., 10$).

The basic model specification is:

$$Y_{it} = \sum_{k=2}^{10} ysm_{it}^{k}\delta^{k} + \sum_{k=2}^{10}(e_i \times ysm_{it}^{k})\gamma^{k} + \sum_{c}\theta_i^{c} + \sum_{s}\theta_i^{s} + x_i\beta + \varepsilon_{it} \qquad (1)$$

Here x_i is controls: gender, age, marital status, education and dummies for number of kids. As education, marriage and fertility may be endogenous variables, I use observed values at the time of migration; these are also the observed characersitics that caseworkers could potentially use in determining where people are settled.

θ^c and θ^s are indicators for country of origin and year of immigration. ysm^k is an indicator for length of stay, equal to one if $t = s + k$. This specification is flexible in that it does not impose any particular functional form on the relationship between length of stay and economic outcomes, however, it also means is not possible in this model to distinguish between calendar time effects and cohort effects.

In the basic model, e_i is the local employment rate in the immigrant population in the first region of residence in the year of arrival. In extended models, e_i will also contain variables describing local demographic characteristics. These characteristics are calculated using only the existing immigrant population already residing in the region, meaning they do not contain the employment outcome of individual i.

4 Results

Column (1) of Table 4 contains estimated effects of local immigrant employment rates on later labor earnings (full estimation results can be found in the appendix). The estimates are positive for all years up to ten years after immigration, with 8 of the 9 point estimates statistically significant at the five percent level. The dependent variable is total labor earnings measured in 1000 NOK, so an estimated $\hat{\gamma}_k = 100$ implies that being placed in a labor market region with a 1 percentage point higher employment rate in the existing immigrant population translates to 1,000 NOK higher expected earnings in year k after immigration to Norway.

In the main model, the local immigrant employment rate is used as the only measure of local labor market conditions. An extended model includes effects of the local rate of registered unemployment, the employment rate of immigrants with a similar country background (using the 12 geographical regions defined earlier) and local population size, all interacted with years since migration (YSM). 62 individuals (7.8% of the sample) are placed in regions where there were no people aged 25-55 already living there from their own region of origin, and are for this reason not included in the estimation. Column 2 of Table 4 show estimated effects of the local employment rate when the model is expanded to include effects of these other local characteristics on later immigrant earnings. All point estimates remain positive, however estimated effects are reduced at years 2,3 and 10 after immigration. For years 4-9 after immigration, including these additional characteristics increases estimated effects somewhat. Overall, the positive effect of the initial local immigrant employment rate on later earnings appears to be robust to the inclusion of other local characteristics.

Figure 3 illustrates estimated effects of additional local variables. For comparison, the first panel graphically plots the estimates from column 2 of the effects of the local immigrant employment rate. The second panel shows effects of the local registered unemployment rate. The local unemployment rate in the full population reduces earnings years 2 and 3 after immigration; after that there are no significant effects. One possible explanation for this is if local labor

Table 4: Main regression estimates

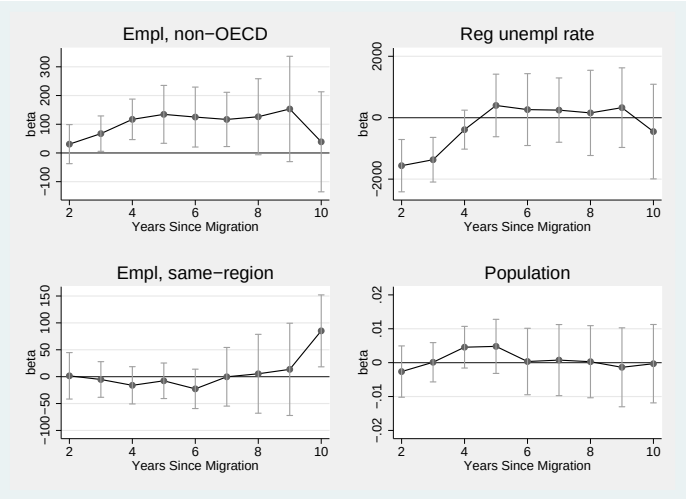
	(1) Earnings	(2) Earnings	(3) Earnings	(4) Moved	(5) Moved Up
e_0, YSM=2	108.5***	30.69	-72.56*	-0.166	-0.188*
	(4.20)	(0.91)	(-1.70)	(-1.22)	(-1.95)
e_0, YSM=3	130.0***	67.33**	-12.10	-0.217	-0.279**
	(4.48)	(2.20)	(-0.33)	(-1.36)	(-2.20)
e_0, YSM=4	114.4***	117.0***	-1.849	-0.177	-0.355***
	(3.80)	(3.34)	(-0.05)	(-1.06)	(-3.57)
e_0, YSM=5	90.94**	134.4**	-12.49	-0.179	-0.348**
	(2.52)	(2.69)	(-0.32)	(-0.76)	(-2.55)
e_0, YSM=6	69.84*	125.0**	-46.24	-0.264	-0.403***
	(1.92)	(2.41)	(-1.12)	(-1.41)	(-3.57)
e_0, YSM=7	85.41**	116.8**	-52.15	-0.177	-0.217
	(2.49)	(2.49)	(-1.09)	(-0.90)	(-1.57)
e_0, YSM=8	104.9***	126.1*	-45.56	-0.282	-0.338***
	(2.80)	(1.92)	(-0.93)	(-1.47)	(-2.84)
e_0, YSM=9	122.3**	153.2*	-47.19	-0.251	-0.226*
	(2.37)	(1.68)	(-0.74)	(-1.61)	(-1.87)
e_0, YSM=10	139.1***	38.83	-51.30	-0.0553	-0.130
	(2.83)	(0.45)	(-0.85)	(-0.25)	(-0.98)
e_t, same year			227.9***	-0.178**	-0.419***
			(5.29)	(-2.29)	(-5.32)
Observations	48066	47777	48059	37572	37572
Include additional local char.	No	Yes	No	No	No
Inlude same-year empl. rate	No	No	Yes	Yes	Yes

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows effects of local immigrant employment rates in first region of residence year 0 on later labor market outcomes. In models 1-3 the dependent variable is total labor earnings. In model 4 the dependent variable is moving to out of initial labor market region, in model 5 moving to a labor market region with higher immigrant employment rate. e_0 and e_t denote the employment rate of non-OECD immigrants in the initial region of residence, in the year of arrival and current year respectively. All models include controls for gender, age, marital status, number of children, country of origin, year of immigration and years since migration. Standard errors clustered at labor market region.

Figure 3: Estimates, model (2)



Note: Figure plots selected estimates with 95% confidence intervals. The dependent variable is total annual labor earnings. The model includes controls for gender, age, marital status, number of children, country of origin, year of immigration, and years since migration.

markets are most important around the transition from training/qualification to full time work, and the timing of labor market entry is correlated with employability. The people who enter the labor market early (within the first three years after arrival in Norway) are qualified for a wider range of jobs, thus the rate of registered unemployment may be a better measure of local employment prospects.

Looking at the third panel, there is little evidence for differential effect of higher employment rate among immigrants from the same group of countries. The initial local employment rate among immigrants from a similar country background has no significant effects on later outcomes, with the exception of a positive and significant estimated effect year 10 after immigration. Conditional on these labor market characteristics, the population size of the initial labor market region has no statistically significant effects on later earnings.

Estimating this extended model shows that local employment prospects measured by the immigrant employment rate is not the only relevant variable in explaining later labor market outcomes. The local rate of registered unemployment may be important in predicting average earnings the first 2-3 years after immigration. Overall, estimated effects of local immigrant employment rate remain positive when controlling for a wider set of local conditions.

The estimates in columns (1) and (2) should be interpreted as total effects on later earnings. These effects could operate through two separate channels: First, they may reflect a combination of persistence in local labor market conditions and geographical immobility. If this is the case, being placed in an area with

poor employment prospects conditions at the time of migration would increase the chances of experiencing similar bad conditions in the future, which in turn would reduce labor earnings.

The second channel would be through distinct and lasting impacts of initial conditions, e.g. in the form of scarring effects. In this case, being placed in an area with poor employment prospect would increase the probability of unemployment the first years after immigration. If this early unemployment experience could then be interpreted as a negative signal by later prospective employers, the negative impacts may persist for a long time.

An extended model (2) is proposed to distinguish between these channels.

$$\begin{aligned} Y_{it} = &\sum_{k=2}^{10} ysm_{it}^{k}\delta^{k} + \sum_{k=2}^{10}(e_i \times ysm_{it}^{k})\gamma^{k} + e_{it}\eta \\ &+ \sum_{c}\theta_i^{c} + \sum_{s}\theta_i^{s} + x_i\beta + \varepsilon_{it} \end{aligned} \tag{2}$$

Here, e_{it} is the immigrant employment rate in the first region of residence in year t, constructed leaving out the contribution of individual i. If there are in fact individual scarring effects, estimated γ^k should remain positive even when the contemporaneous immigrant employment rate is included as a control in the model. Estimates are shown in column (3) of Table 4. When controlling for the contemporaneous employment rate, there are no longer any positive effects of the initial employment rate. In fact, the effects turn negative, though with two exception estimates are small and not statistically significant. These estimates suggest that the positive estimates of γ^ks in the basic model are not due to individual scarring effects. Rather, the explanation appears to lie in persistence in local labor market conditions over time, in combination with immobility.

The contemporaneous employment rate in the assigned municipality has, perhaps not surprisingly, a large positive effect on own earnings. However, this employment rate will not be the actual employment rate experienced by movers. The size and significance of this coefficient then would suggest limited mobility. Either that most people tend to remain in the initial labor market region, or perhaps move to adjacent regions which tend to experience similar types of shocks.

To shed some light on this, moving to another labor market region is modeled as a separate outcome, using the extended model in equation (2). First, the dependent variable is replaced with an indicator variable equal to 1 if the person is currently living outside the initial labor market region. There may be many motivations for moving to another part of the country, not necessarily related to the state of the local labor market. As an alternative outcome, I construct a second indicator equal to one only if the person is living in a labor market region where the immigrant employment rate (still excluding individual i) in year k is higher than in the initial location that same year. The model is estimated on a subsample where persons are excluded after the first move to another labor market region.

Results are shown in column 4 and 5 of Table 4. When looking at moves out of the initial labor market region , the current year's employment rate has a negative and significant effect on the probability of moving. Effects of initial conditions however, are all negative but not statistically significant. This changes when looking at movements to "better" labor markets only. The effect of the contemporaneous unemployment rate is still there, with a larger and more significant point estimate; the exact interpretation of this is complicated however, due to regression to the mean. Specifically, the way the dependent variable is defined induces a mechanical negative correlation between the dependent variable and the contemporaneous employment rate in the initial municipality, making it hard to give the estimated $\hat{\eta}$ a behavioral interpretation. More interesting then perhaps are the estimated effects of initial labor market conditions. In column 5, estimated effects of the initial immigrant employment rate are significant even when controlling for the same year employment rate. One interpretation of this is that resettlement refugees who are placed in "bad" labor markets may be unable to move to a better region right away. The first few years, it is more likely that liquidity constraints are an issue. Moreover, participation in paid introduction and training programs may be conditional on staying in the assigned municipality.

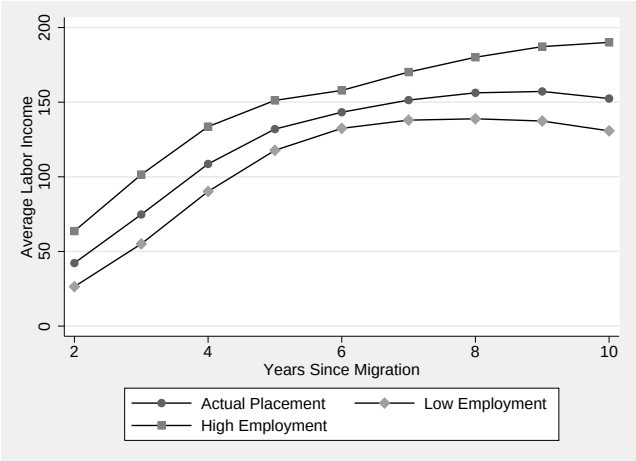
To summarize, the employment prospects in the first region of settlement of refugee immigrants seems to influence their later earnings. However, I find no evidence of scarring effects. Estimated models suggest that local employment prospects are persistent, combined with limited geographical mobility: People do not fully respond to adverse labor market conditions by moving to labor market regions where the chances of finding a job are higher.

5 Policy simulations

The previous section found that the employment prospects in the settlement location affect later earnings. In this section, I make an attempt to quantify these effects by predicting average earnings under alternative settlement policies. First, estimated models of the previous section are used to predict individual earnings given the actual initial settlement decision. For each year, the labor market region with the highest and lowest immigrant employment rates are identified and used to formulate two alternative placement policies. In the "high-employment" policy, all resettlement refugees arriving in a given year are settled in the labor market region with the highest immigrant employment rate that year. Symmetrically, in a "low employment" policy, all resettlement refugees are settled in the region with the lowest immigrant employment rate. For each of these two location policies, the estimated models are used to predict individual earnings.

Figure 4 plots model predicted earnings under the actual settlement policy and for each of the counterfactual settlement policies described above. On average, the high employment settlement policy predicts 26% higher earnings, while the low employment policy predicts 19% lower earnings compared to the

Figure 4: Policy simulations



Note: Figure plots average predicted earnings of the sample population using actual and counterfactual settlement policies

predictions under the actual settlement policies.

In both counterfactual policies, all refugees are settled in a single labor market region. In practice, this is not a credible policy alternative. However, this exercise may still be useful as it defines limits to effects of alternative settlement policies for instance targeting municipalities with better (or worse) labor markets to make them accept a greater share of new resettlement refugees.

6 Conclusions

Identifying effects of local labor market conditions on the individual labor market outcomes is difficult if immigrants are free to select where to live. The present paper models labor market outcomes for a subset of refugee immigrants subject to a quasi-experimental settlement policy which provides variation in observed initial location. Estimates indicate that local labor market conditions are important for the labor market outcomes of refugee immigrants. Placing immigrants in regions with good immigrant labor markets increases total labor earnings up to ten years later.

In extended models, other local labor market characteristics were included as controls to shed some light on the possible mechanisms. Estimates were robust to the inclusion of these additional variables. This indicates that the effects cannot fully be explained by local business cycle conditions that affect the full population equally, or by peer effects. Controlling for the contemporaneous immigrant employment rate in the assigned region completely removed the positive effect of the employment rate in the year of arrival. In other words, there do not seem to be any individual scarring effects of early unemployment

experience.

These extended models suggest that some labor market regions have policies that systematically increase the participation rates of non-OECD immigrants, and that refugee immigrants who are placed in these regions do better in the long term. Estimated models find no evidence of individual persistence. Rather, the effects seem to stem from a combination of persistence of local labor market conditions and a tendency that resettlement refugees do not move away to parts of the country with higher immigrant employment.

Finally, to evaluate the quantitative implications of the estimated effects, the paper includes some simple policy simulations. Based on the estimates in the basic model, if all resettlement refugees were placed in the labor market region with the highest immigrant employment rate, predicted average earnings would be 26% higher. Symmetrically, a "worst case" policy where all the slots were moved to the labor market region with the lowest immigrant employment rate, the model predicts average earnings to be 19% lower than what is observed in the data. Though these counterfactual policies are extreme, they do provide some indication that estimated effects are not only statistically significant but may be quantitatively relevant.

An open question is how these effects generalize to the wider population of refugee immigrants and immigrants in general. Resettlement refugees are a small group, characterized by low labor market attachment and little geographical mobility compared to asylum seekers (Kavli & Svensen 2001). From the analysis in this paper, this could mean that the effects of initial conditions are smaller for other refugee immigrants, if they are more likely to move to where the employment prospects are better.

Appendix: Full estimation results

Table 5 contains estimates from the models of section 3, showing estimated coefficients not included in Table 4.

Table 5: Full estimates

	Earnings	Earnings	Earnings	Moved	Moved Up
	(1)	(2)	(3)	(4)	(5)
Age at imm.	-1.950***	-1.953***	-1.951***	-0.00114***	-0.000721***
	(-11.42)	(-11.42)	(-11.59)	(-4.50)	(-3.16)
Married	10.12***	10.20***	10.39***	-0.00360	-0.00318
	(2.99)	(2.95)	(3.07)	(-0.94)	(-0.88)
Female	-56.55***	-56.60***	-56.58***	0.000184	-0.0000729
	(-12.52)	(-12.43)	(-12.61)	(0.10)	(-0.04)
High school	22.03***	21.64***	21.20***	-0.00500	-0.00162
	(5.07)	(5.20)	(5.04)	(-0.82)	(-0.34)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
College	46.46***	46.71***	46.38***	0.0133**	0.0115**
	(9.34)	(9.31)	(9.29)	(2.17)	(2.11)
Kids, 1	1.989	2.019	2.365	0.00675	0.00214
	(0.46)	(0.46)	(0.55)	(1.35)	(0.63)
Kids, 2	3.213	3.077	2.898	-0.00180	-0.00362
	(0.63)	(0.60)	(0.58)	(-0.31)	(-0.73)
Kids, 3	-6.081	-6.335	-5.487	0.00888	0.00182
	(-0.96)	(-0.99)	(-0.85)	(1.23)	(0.34)
Kids, 4	-10.45*	-10.89*	-11.06*	0.00211	0.00183
	(-1.78)	(-1.85)	(-1.90)	(0.28)	(0.28)
Kids, 5	-40.68***	-41.64***	-40.68***	-0.00142	-0.0109
	(-8.26)	(-8.27)	(-8.27)	(-0.12)	(-1.24)
Kids, 6	-39.83***	-39.78***	-40.55***	0.0164	0.00235
	(-3.50)	(-3.53)	(-3.62)	(0.99)	(0.21)
Kids, 7	-54.50***	-54.59***	-54.66***	0.0107	-0.00895
	(-8.49)	(-8.45)	(-8.56)	(0.47)	(-0.48)
Kids, 8	-58.85***	-58.20***	-55.05***	0.0475	0.0545
	(-4.77)	(-4.92)	(-4.30)	(0.98)	(1.18)
Kids, 9	-72.65***	-72.35***	-69.05***	0.143**	0.0917
	(-8.00)	(-7.35)	(-7.47)	(2.25)	(1.02)
Kids, 10	-81.64***	-80.48***	-80.50***	-0.0802***	-0.0822***
	(-10.57)	(-10.54)	(-10.19)	(-4.69)	(-5.92)
Imm year 1994	-8.445	-9.616	-11.00*	-0.000925	0.00463
	(-1.34)	(-1.44)	(-1.81)	(-0.08)	(0.49)
Imm year 1995	-35.82***	-37.66***	-38.92***	-0.00507	0.00574
	(-3.83)	(-3.63)	(-4.42)	(-0.49)	(0.68)
Imm year 1996	-32.76***	-35.04***	-30.98***	0.0171	0.0278**
	(-3.48)	(-3.30)	(-3.09)	(1.04)	(2.18)
Imm year 1997	-23.44***	-27.64***	-15.78**	0.0297	0.0452***
	(-3.43)	(-3.11)	(-2.03)	(1.58)	(4.06)
Imm year 1998	-25.28**	-30.94**	-9.867	0.0431*	0.0671***
	(-2.65)	(-2.46)	(-0.86)	(1.86)	(4.93)
Imm year 1999	-2.836	-8.818	12.02	0.0699***	0.0892***
	(-0.26)	(-0.78)	(1.13)	(2.72)	(5.67)
Imm year 2000	-5.586	-10.80	3.282	0.0709***	0.0792***
	(-0.54)	(-0.90)	(0.32)	(3.44)	(5.72)
Imm year 2001	-8.682	-14.07	0.110	0.0643**	0.0806***
	(-1.27)	(-1.56)	(0.01)	(2.33)	(5.26)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
Imm year 2002	-24.43***	-28.61***	-16.30*	0.0688**	0.0895***
	(-2.96)	(-3.07)	(-1.93)	(2.56)	(6.06)
Imm year 2003	0.113	-0.726	-2.262	0.0376	0.0595***
	(0.01)	(-0.06)	(-0.22)	(1.44)	(4.05)
Imm year 2004	12.38	10.45	1.355	0.0237	0.0490***
	(1.17)	(0.88)	(0.14)	(0.93)	(3.07)
Imm year 2005	-9.535	-11.40	-24.33**	0.0180	0.0544***
	(-0.75)	(-0.80)	(-2.08)	(0.64)	(3.97)
Imm year 2006	-8.640	-20.07	-20.71*	0.0384	0.0688***
	(-0.78)	(-1.49)	(-1.98)	(1.29)	(4.15)
Imm year 2007	-22.72*	-37.68**	-22.80*	0.0322	0.0743***
	(-1.91)	(-2.52)	(-1.89)	(0.92)	(3.78)
Belarus	438.1***	439.5***	430.3***	-0.0765**	-0.0282*
	(70.41)	(62.12)	(62.58)	(-2.53)	(-1.82)
Croatia	71.95***	72.87***	73.40***	-0.0577**	-0.0335**
	(6.68)	(6.96)	(6.65)	(-2.37)	(-2.08)
Poland	129.6***	129.5***	135.1***	0.0722	-0.0158
	(3.44)	(3.25)	(3.64)	(0.72)	(-1.54)
Russia	26.29	27.38	20.39	-0.0228	-0.0106
	(0.94)	(0.95)	(0.72)	(-0.80)	(-0.73)
Turkey	19.88	20.63	19.12	0.0391	0.0159
	(0.63)	(0.66)	(0.59)	(0.59)	(0.32)
Slovenia	245.1***	243.4***	246.4***	-0.0821**	-0.0360**
	(27.45)	(30.61)	(27.71)	(-2.33)	(-2.14)
Bosnia Hercegovina	58.99***	59.64***	61.32***	-0.0293	-0.0362*
	(5.63)	(5.75)	(5.61)	(-1.02)	(-1.85)
Macedonia	145.8***	146.0***	148.3***	-0.0841***	-0.0555***
	(3.01)	(3.07)	(3.09)	(-2.79)	(-2.72)
Serbia	89.96***	91.06***	92.82***	-0.0188	-0.0132
	(3.62)	(3.67)	(3.68)	(-0.53)	(-0.44)
Montenegro	-46.69**	-44.63**	-51.44***	-0.00269	-0.0687***
	(-2.46)	(-2.31)	(-2.86)	(-0.05)	(-3.04)
Kosovo	31.97*	33.29*	31.52*	-0.0459*	-0.0518**
	(1.87)	(1.98)	(1.81)	(-1.75)	(-2.59)
Algeria	18.61	20.09	15.56	0.0383	0.0258
	(0.56)	(0.60)	(0.45)	(1.14)	(0.81)
Angola	-79.79***	-64.74***	-68.82***	0.120	0.146
	(-6.24)	(-6.46)	(-5.82)	(0.79)	(1.03)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
Botswana	-1.536		5.602	-0.111***	-0.106***
	(-0.15)		(0.59)	(-4.56)	(-5.21)
Burundi	15.74	14.36	13.73	-0.0166	-0.00819
	(1.42)	(1.21)	(1.22)	(-0.85)	(-0.73)
Benin	-29.92***	-33.93***	-41.39***	-0.0222	0.0263**
	(-3.31)	(-3.51)	(-4.16)	(-0.87)	(2.02)
Ivory Coast	-18.86	-7.127	-23.28	0.0510	0.0982
	(-0.34)	(-0.12)	(-0.45)	(0.53)	(1.14)
Eritrea	35.97***	36.40***	31.41**	-0.0486*	-0.0134
	(2.70)	(2.80)	(2.25)	(-1.93)	(-1.04)
Ethiopia	29.80***	30.30***	26.15***	0.0216	0.0187
	(3.76)	(3.41)	(3.28)	(0.90)	(1.23)
Egypt	-4.944	-4.687	-6.594	0.0157	0.0408
	(-0.13)	(-0.12)	(-0.19)	(0.29)	(0.90)
Gambia	-64.97***	-68.85***	-60.99***	0.149***	-0.0344*
	(-6.27)	(-5.97)	(-5.95)	(4.30)	(-1.86)
Ghana	-128.1***	-105.2***	-140.8***	0.110***	0.0119
	(-10.58)	(-7.65)	(-10.09)	(3.55)	(0.81)
Cameroon	130.9***	140.2***	126.8***	0.0123	0.0109
	(10.93)	(13.15)	(12.06)	(0.33)	(0.55)
Kenya	44.06	44.71	40.01	-0.0551	-0.0194
	(0.98)	(1.00)	(0.89)	(-1.34)	(-0.60)
Kongo Rep.	-30.78***	-27.84**	-27.69**	-0.0416	-0.0157
	(-3.04)	(-2.57)	(-2.68)	(-1.21)	(-0.50)
Kongo DRC	12.43	11.78	12.53	0.0114	0.00203
	(1.30)	(1.16)	(1.37)	(0.55)	(0.15)
Liberia	-10.19	-6.678	-10.73	0.0000552	0.00790
	(-1.22)	(-0.79)	(-1.24)	(0.00)	(0.75)
Libya	-128.0***	-130.7***	-128.3***	-0.0288	0.0167
	(-18.10)	(-17.18)	(-20.99)	(-0.49)	(0.25)
Nigeria	0.912	2.141	-5.304	-0.0889***	-0.0560**
	(0.01)	(0.03)	(-0.07)	(-2.78)	(-2.07)
Zimbabwe	-24.68**	-26.18**	-34.58***	0.105	-0.00407
	(-2.40)	(-2.25)	(-3.03)	(0.70)	(-0.18)
Rwanda	51.76**	52.57**	49.04**	-0.0201	-0.0233
	(2.64)	(2.65)	(2.68)	(-0.94)	(-1.32)
Sierra Leone	36.42***	36.78***	32.15***	-0.0638***	-0.0404***
	(3.61)	(3.56)	(3.15)	(-2.92)	(-3.46)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
Somalia	-47.05***	-46.05***	-48.33***	-0.0232	-0.0289*
	(-4.61)	(-4.13)	(-4.85)	(-1.09)	(-1.82)
Sudan	-16.51**	-17.27**	-18.16**	-0.0259	-0.0180
	(-2.20)	(-2.28)	(-2.42)	(-1.25)	(-1.13)
Tanzania	-59.74***	-56.16***	-61.25***	0.0256	0.0115
	(-3.66)	(-3.14)	(-3.63)	(0.43)	(0.34)
Chad	-59.82***	-59.92***	-64.26***	-0.0740***	-0.0362***
	(-6.65)	(-6.76)	(-6.81)	(-3.66)	(-4.08)
Togo	-25.68***	-26.10**	-18.67**	0.0796**	0.112***
	(-3.44)	(-2.62)	(-2.61)	(2.20)	(5.03)
Tunisia	42.75	44.70	39.40	0.00837	0.0360
	(1.17)	(1.24)	(1.05)	(0.14)	(0.63)
Uganda	1.240	1.219	-0.220	0.165***	0.129***
	(0.09)	(0.09)	(-0.02)	(3.61)	(3.53)
Zambia	97.58*	96.62*	92.60*	-0.0538**	-0.00935
	(1.82)	(1.77)	(1.80)	(-2.10)	(-0.42)
Afghanistan	-10.33*	-9.923*	-12.41**	0.0123	0.00997
	(-1.99)	(-1.85)	(-2.35)	(0.81)	(0.84)
Azerbaijan	-9.432	-9.233	-13.74	-0.0213	-0.0239
	(-0.22)	(-0.21)	(-0.31)	(-0.77)	(-1.00)
Myanmar/Burma	9.162	8.807	9.178	-0.0486**	-0.0195*
	(0.95)	(0.97)	(1.00)	(-2.41)	(-1.75)
Sri Lanka	50.28	50.79	48.61	-0.0363	-0.0282
	(1.02)	(1.04)	(1.03)	(-0.69)	(-0.77)
The Philippines	78.03***	79.02***	81.25***	-0.0451	-0.0280*
	(3.01)	(2.99)	(3.16)	(-1.60)	(-1.70)
Georgia	43.08***	42.55***	44.86***	0.179***	0.211***
	(7.61)	(7.00)	(7.63)	(5.39)	(10.07)
India	18.84	21.04	18.15	0.00392	0.0211
	(1.16)	(1.27)	(1.06)	(0.08)	(0.53)
Indonesia	29.64***	30.03***	21.31***	-0.0709**	-0.0212*
	(4.77)	(5.19)	(3.18)	(-2.50)	(-1.76)
Iraq	-35.24***	-34.61***	-35.43***	-0.0276*	-0.0215*
	(-4.98)	(-4.83)	(-5.02)	(-1.95)	(-2.01)
Jordan	5.403	4.839	8.631	0.133*	0.168**
	(0.08)	(0.07)	(0.12)	(1.96)	(2.69)
Cambodia	-23.43***	-23.50***	-22.87***	-0.0665***	-0.0333***
	(-3.38)	(-3.54)	(-3.30)	(-3.39)	(-3.80)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
Kazakhstan	114.3***	114.0***	108.0***	0.0581	-0.0106
	(4.18)	(4.02)	(3.96)	(0.66)	(-0.70)
China	-39.81***	-39.67***	-44.06***	-0.00998	-0.00838
	(-5.66)	(-5.46)	(-5.65)	(-0.51)	(-0.68)
Kuwait	-41.45***	-42.14***	-33.00***	-0.00460	0.00254
	(-3.28)	(-3.38)	(-2.99)	(-0.08)	(0.05)
Kyrgyzstan	-83.84***	-85.88***	-92.67***	-0.0425*	0.00288
	(-2.85)	(-2.90)	(-2.95)	(-1.88)	(0.31)
Laos	-51.74***	-50.93***	-46.83***	-0.0679**	-0.0256*
	(-4.55)	(-4.78)	(-4.21)	(-2.53)	(-1.96)
Lebanon	6.181	6.194	4.968	-0.0499	-0.0340
	(0.24)	(0.24)	(0.19)	(-1.39)	(-1.61)
Malaysia	24.31***	24.53***	18.11***	-0.0734**	-0.0285**
	(7.55)	(6.46)	(4.77)	(-2.49)	(-2.02)
Palestinian territories	-82.55***	-83.26***	-85.86***	-0.0751***	-0.0514***
	(-4.83)	(-5.08)	(-5.42)	(-3.02)	(-2.72)
Nepal	38.65**	37.62***	40.92***	-0.0668***	-0.0280**
	(2.61)	(2.74)	(2.81)	(-3.20)	(-2.53)
Pakistan	5.156	3.526	1.185	-0.0582**	-0.0205
	(0.24)	(0.15)	(0.05)	(-2.30)	(-1.49)
Saud iArabia	2.031	1.666	2.296	-0.0342	0.000170
	(0.04)	(0.04)	(0.05)	(-1.54)	(0.01)
Singapore	-32.63	-30.84	-33.02	0.0918	0.123
	(-0.54)	(-0.50)	(-0.52)	(0.68)	(0.91)
Tadzjikistan	142.6***	143.1***	146.4***	-0.0512	-0.0103
	(2.85)	(2.78)	(2.96)	(-0.95)	(-0.20)
Turkmenistan	-12.83	-11.99	-13.92	0.0518	-0.00488
	(-0.76)	(-0.67)	(-0.87)	(0.63)	(-0.21)
Uzbekistan	1.623	-23.78	-1.767	-0.0382	-0.0281**
	(0.06)	(-1.03)	(-0.07)	(-1.36)	(-2.29)
Syria	-37.08***	-36.61***	-39.84***	-0.0163	-0.0177
	(-2.80)	(-2.70)	(-3.02)	(-0.41)	(-0.59)
Thailand	-16.20	-15.83	-16.00	-0.0571**	-0.0212
	(-1.04)	(-1.04)	(-1.02)	(-2.31)	(-1.58)
Vietnam	24.38***	24.18***	22.49***	-0.0416**	-0.0268**
	(3.63)	(3.54)	(3.28)	(-2.18)	(-2.24)
Yemen	-23.17**	-24.00***	-32.17***	-0.0674**	-0.0325*
	(-2.56)	(-2.72)	(-3.34)	(-2.12)	(-1.77)

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved	Moved Up
Cuba	30.50***	24.72**	23.93**	-0.0162	0.0208
	(2.88)	(2.39)	(2.22)	(-0.58)	(1.35)
Dominican Rep	-55.67***	-55.58***	-54.35***	-0.0712***	-0.0329***
	(-5.41)	(-5.79)	(-5.10)	(-3.26)	(-2.97)
Nicaragua	-85.39***	-90.18***	-91.57***	-0.142***	-0.0947***
	(-8.79)	(-8.16)	(-9.51)	(-4.32)	(-4.32)
Chile	38.00***	35.98***	37.45***	-0.0527*	-0.0242
	(4.66)	(4.60)	(4.83)	(-1.77)	(-1.63)
Colombia	42.52	42.59	42.65	-0.00917	-0.0119
	(1.10)	(1.11)	(1.06)	(-0.20)	(-0.30)
Peru	104.2***	102.6***	102.0***	-0.0528*	-0.0212
	(16.50)	(14.15)	(16.45)	(-1.95)	(-1.59)
YSM: 3	22.75**	12.34	1.696	0.0435	0.0604
	(2.36)	(0.93)	(0.15)	(0.87)	(1.31)
YSM: 4	63.69***	-1.604	26.51*	0.0323	0.101**
	(4.91)	(-0.05)	(1.87)	(0.63)	(2.54)
YSM: 5	98.77***	-11.38	51.77***	0.0275	0.0977
	(5.74)	(-0.23)	(2.85)	(0.33)	(1.46)
YSM: 6	122.5***	18.84	78.77***	0.0563	0.120**
	(6.74)	(0.35)	(4.42)	(0.86)	(2.10)
YSM: 7	126.8***	25.76	90.89***	0.00877	0.0299
	(6.73)	(0.57)	(5.28)	(0.14)	(0.58)
YSM: 8	122.1***	26.07	89.74***	0.0406	0.0750
	(6.06)	(0.47)	(4.69)	(0.60)	(1.31)
YSM: 9	119.4***	11.36	93.08***	0.0233	0.0220
	(4.55)	(0.19)	(3.60)	(0.44)	(0.56)
YSM: 10	113.2***	56.48	94.61***	-0.0608	-0.0160
	(4.79)	(0.87)	(3.74)	(-0.92)	(-0.38)
Constant	75.16***	158.6***	47.51***	0.242***	0.318***
	(7.43)	(5.66)	(4.16)	(4.38)	(6.05)
Observations	48066	47777	48059	37572	37572

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows full estimates from models of table 4. For country of origin, the reference category is Iran; for year of immigration, the reference is 1993. YSM stands for years since immigration to Norway.

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