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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. A unique administrative dataset with assigned settlement municipalities is used to identify the causal effect of initial location characteristics. Being placed in a labor market where other non-OECD immigrants do well increases own annual labor earnings up to 6 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. 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

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1 Introduction

This paper examines how initial labor market conditions at the time of immigration affect later labor market outcomes of refugee immigrants. Refugee immigrants typically face barriers to labor market entry, such as language problems and low educational attainment. As a group, refugee immigrants are characterized by low earnings and employment rates, and high rates of social assistance use. In 2012, the employment rate of refugee immigrants in Norway was 50.1%, compared to 68.7% in the full population and 62.8% among all immigrants. These averages mask significant regional variation: Comparing labor market outcomes of refugees in Norway's 19 counties, there was a 17 percentage point difference between average employment rates in the top and bottom regions (57% vs 40%). Similar regional differences in employment rates were also present when comparing refugee immigrants from the same country of origin (Olsen, 2014).

The poor labor market attachment of refugee immigrants can lead to poverty and social exclusion. From a public finance perspective, the cost of accommodating refugee immigrants crucially depends on the extent to which they are able to find work and be economically self-sufficient. This is especially true in European welfare states where individuals have access to generous social insurance arrangements. If local labor market conditions at the time of immigration have an impact on later labor market outcomes, this may have consequences for the design of refugee settlement policies.

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 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.

In order to credibly identify the causal effects of interest, the present paper presents evidence from a settlement program providing quasi-experimental variation in initial labor market conditions. The population studied in this paper is so-called resettlement refugees, who are settled directly in a municipality upon immigration. The quasi-experimental nature of the settlement program allows for the identification of causal effects of local labor market conditions at the time of arrival.

The present paper utilizes a unique data source containing assigned settlement decisions for individual refugees. These assignment records can be linked to data on later individual location and employment outcomes as well as local conditions in the assigned labor market region. The basic models estimate total effects of these characteristics in the initial region the year of settlement

on later earnings. Extended models are then estimated in order to shed light on the possible mechanisms underlying this relationship.

Being settled in a labor market region with high immigrant employment rate increases later annual labor earnings. Extended models find no evidence that this is driven by differences in population size, peer effects, or local differences in social programs organized by the municipalities. Moreover, the relationship is not driven by individual scarring effects: when controlling for the contemporaneous employment rate in the assigned region, effects of initial conditions disappear. 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. This interpretation is further supported by models looking directly at later mobility outcomes: Conditioning on current labor market conditions, the initial local immigrant employment rate is found to have a delayed negative effect on the probability of leaving the assigned municipality up to 9 years after immigration.

The paper is closely related to Åslund and 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. 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 working in combination with scarring effects.

The present paper extends the analysis by Åslund and Rooth (2007), by estimating a series of additional model extensions in order to shed further light on the underlying mechanisms. This includes relating labor market conditions directly to models of moving decisions. Åslund (2005), using a similar placement scheme, finds that higher local employment rate in initial municipality reduces the likelihood of having left the assigned municipality within the first 4 years after immigration. On the surface, this might seem like a bit of a contradiction to the later findings of Åslund and Rooth, whose models indicate the presence of lock-in effects. In the present paper, this relationship is examined further, by estimating how the effects of initial labor market conditions on later moving decisions play out over time.

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 and 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 unemploy-

ment is associated with higher employment rates, possibly due to immigrants in a recession being positively selected. 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 and Raaum, 2002).

The rest of this paper is organized as follows. Institutions and data are presented in section 2. Estimates from the basic model is presented in section 3. In section 4, model extensions are presented to shed some light on possible mechanisms. Section 5 presents additional robustness tests as well as 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, along with some additional background on the institutional setting. 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 and related institutions

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.

Settlement of recently arrived refugee immigrants is generally thought to be costly for local communities, especially in the short term, as the municipalities are responsible for providing for the new arrivals until they are able to be economically self-sufficient. In an effort to distribute these responsibilities across municipalities, refugee immigrants arriving in Norway are subject to a settlement program. Through this program, local governments agree on accepting a set number of refugee immigrants. In other words, settlement is voluntary from the point of view of the local municipalities. For the first five years after settlement, municipalities receive transfers from the central government - this is intended to cover costs related to settlement and labor market integration. Amounts vary with the age and family situation of the settled immigrants. Currently, municipalities receive 232,000 NOK (around 26,000 EUR) for a single adult immigrant the first year of settlement, with gradually reduced transfers up to year 5.

Refugee immigrants who comply with the settlement program are provided with basic housing, until they are able to find more permanent living arrangements. Moreover, receiving financial assistance (welfare) is formally

conditional on complying with the settlement program. After 2004, all newly arrived refugee immigrants are offered a training program teaching basic skills for living and working in Norway. The program is full time, lasting 1-2 years, and participants are paid "wages" in the form of an introductory benefit. Participation in this program is conditional on living in the assigned municipality. Overall, there are significant financial incentives (free housing and cash benefits) for refugee immigrants to comply with the settlement program.

The Norwegian welfare state is characterized by a comprehensive array of benefits, with different rules regarding eligibility and payments. In general, refugee immigrants face the same rules regarding eligibility, replacement rates, maximum benefit duration etc as other residents[1].

Financial assistance is a means-tested cash benefit intended to provide enough money for subsistence for persons who have no other means of subsisting themselves. Eligibility does not depend on employment history, but there is a requirement that recipients be legally and permanently residing in Norway. Refugee immigrants fulfill this residence requirement from the time they are granted a legal residence permit, making them immediately eligible. This is different from certain other groups of immigrants, such as EEA citizens, who have to prove permanent residence in order to qualify.

Compared to financial assistance, other benefits (health related benefits and unemployment insurance) are typically more generous, while imposing more restrictions on eligibility. For example, unemployment insurance has a 62.5% replacement rate, but eligibility is limited based on earnings history. As a rule, benefits are regulated at the national level, leaving limited local variation in benefits. The exception is financial assistance, which is administered at the municipality level.

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 is that the assigned location should be random, conditional on the observable characteristics we have in the dataset. 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. 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

[1] The only exception is the "introduction benefit" described above, which is only available to program participants (refugee immigrants including former asylum seekers and their families).

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. If (unobserved) earnings potential is correlated within such networks of friends and families, this could lead to an indirect selection bias, for instance if highly motivated new refugee immigrants are placed in municipalities where their similarly highly motivated friends are driving up the immigrant employment rate. In this case, any effects of initial conditions should be driven primarily by outcomes among local immigrants from a similar country background, who are more likely to be part of existing networks of friends and family. To address this point then, extended models are formulated that allow for a differential effect of local outcomes in the existing immigrant population from a similar country background.

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 and 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 and Pettersson-Lidbom, 2012). By focusing on resettlement refugees only, as well as using assignment data rather than observed locations, the following analysis will 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. For each person, the assigned municipality is identified directly from administrative records; data on registered municipalities of residence are then attached to the sample up to year 10 after immigration. Outcomes are observed up to 2010. The resulting sample is an unbalanced panel: while the 1993 to

2000-cohorts are retained for 10 years, the latest arriving cohort (2007) are observed only up to year 3 after immigration. Next, 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 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. The final sample contains 7,394 persons [2].

Person-years when individuals cannot be found in population residence data are 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[3] from the computation of immigrant-specific rates. The labor market situation of OECD immigrants is more similar to the situation of natives; they are also more likely to arrive on work related visas, compared to non-OECD immigrants.

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. The language barrier

[2] 7 individuals are registered with a country of origin that was a member of OECD before 1990 (excluding Turkey). Countries of origin for the excluded individuals are France, The Netherlands, Great Britain, Switzerland and Germany. These observations likely reflect an error in recorded country of origin or refugee status, and are excluded from the sample.

[3] 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.

is likely to be a bigger problem for refugee immigrants compared to other immigrant groups (e.g. OECD immigrants on work visas), as many have limited knowledge of English as well as Norwegian. As a result, communication with potential employers becomes difficult, even in low skill occupations. In other words, 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. The resulting immigrant employment rate is intended to reflect overall employment prospects for newly arrived immigrants. This could reflect several local factors: the quality of local welfare/ training programs, attitudes toward immigrants, local business cycles as well as differences in the industrial makeup of the local labor market.

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 macro regions[4], with local employment rates calculated separately by region of origin.

Table 1 provides some descriptive statistics. Column 1 contains average values for the entire sample, column 2 contains averages for persons whose first registered location corresponds to the assigned location ("compliers") and col-

[4] The 12 regions in the sample are 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.

umn 3 represents individuals who were registered as living outside the assigned labor market region the year after immigration ("immediate movers")[5].

On average, around 41% are settled in the labor market regions around the four largest cities - Oslo, Bergen, Trondheim and Stavanger. A large majority of settlement refugees comply with the settlement program. Only 8.4% are classified as "immediate movers". These early movers are on average slightly more likely to have a university degree, there are also differences in country of origin. Immediate movers are on average settled in locations with small populations, and are much less likely to be settled in major cities. This could indicate that individuals who are placed in locations deemed more desirable - e.g. more urban - become more willing to accept the settlement decision. Among compliers, the majority of people never move to another labor market region - only 32 % are ever observed living outside the originally assigned region. For those settled in a major city, the rate is even lower, around 14%.

An important assumption made throughout the paper is that there is no sorting on unobservables - the initial location should be uncorrelated with unobserved earnings potential. While this assumption cannot be verified empirically, it is possible to evaluate the degree of sorting on observables, that is, how individuals' observed characteristics at the time of immigration correlate with assignment. More specifically, the following regression is estimated:

$$e_{i0} = \theta_i^s + x_i\beta + \varepsilon_i \tag{1}$$

where e_{i0} is the employment rate among the existing immigrant population in the assigned labor market region the year of immigration. θ_i are dummies for year of immigration and x_i is a vector of observable characteristics that may impact earnings potential: age, gender, family situation, country of origin and education. Selected estimates are shown in table 2. The estimated year-of-immigration effects θ (not shown) are statistically significant, possibly reflecting changing macroeconomic conditions. Demographics and education are not significant in explaining variation in the initial immigrant employment rate. The same is true when regression 1 is estimated with the local unemployment rate in the full population as the dependent variable. Moreover, F-tests of joint significance of these individual observable characteristics these tests fail to reject the hypothesis that all the coefficients in table 2 are equal to zero (at conventional significance levels)[6].

[5] "Immediate movers" are retained in the sample, with local characteristics defined using the assigned municipality. The decision of whether or not to comply with assignment program could be motivated in part by characteristics of assigned municipality, in a way which varies across individuals according to unobserved characteristics. For instance, highly motivated workers could be more likely to move in response to being assigned to a depressed region. Excluding these individuals from estimations would lead to a selected sample and biased estimates.

[6] As an additional robustness test, I have run a series of t-tests for differences in means between persons placed in regions with low employment (at or below sample median, calculated by year of arrival) and those placed in regions with high immigrant employment rates. Results are shown in the appendix. This approach finds somewhat more evidence of selection on observables, primarily by country background, but also to a lesser extent demographics

Table 1: Summary statistics

	(1) All mean	(2) Compliers mean	(3) Movers mean
Age	31.7	31.8	30.7
Female	0.44	0.44	0.43
Married	0.58	0.58	0.54
Any children	0.59	0.59	0.53
Family size	3.43	3.45	3.20
East Africa	0.10	0.096	0.15
Southeast Asia	0.20	0.22	0.036
South Asia	0.30	0.29	0.43
West Asia	0.18	0.18	0.26
Other region	0.22	0.23	0.12
Secondary school	0.17	0.17	0.16
College	0.19	0.18	0.24
Settled in major city	0.41	0.43	0.19
Immediate move	0.084	0	
Moved	0.38	0.32	
Moved if settled in major city	0.17	0.14	
Employment rate, non-OECD immigrants	0.46	0.46	0.42
Employment rate, natives	0.80	0.80	0.79
Unemployment rate, all	0.024	0.024	0.026
Local population, in 1000s	225.4	239.0	76.5
Observations	7394	6775	619

*Note:*Table shows descriptives of refugee immigrants in the sample. Demographics are observed the first year after immigration to Norway. Column 1 represents the full sample, column 2 contains represents persons whose first registered location corresponds to the assigned location ("compliers") and column 3 represents individuals who were registered as living outside the assigned labor market region the year after immigration ("movers").

In other words there does not appear to be much sorting on observables. This is comforting, as these observable characteristics are expected to be correlated with earnings potential. This is particularly true for education and age (as it reflects experience). This lack of sorting on observables then makes sorting on unobservables seem less likely, especially given the lack of contact between refugees and caseworkers before assignment.

To get a first impression of the data, I do a simple comparison of average labor earnings among people assigned to high and low immigrant employment regions. Panel (a) of Figure 1 plots average labor income for high and low employment rate subsamples, while panel (b) plots the share of people living outside the assigned labor market region, by years since migration. Average labor earnings increase in an almost linear fashion for the first 6 years after immigration, then the growth appears to slow down or stop. Refugee immigrants placed in regions with above-median employment rates appear to have higher average earnings up to 10 years after immigration. Secondary mobility

(marital status and gender). For the variables arguably most closely related to unobserved components of the earnings potential, indicators of high school and college degrees, as well as age, there appears to be no differences between the two groups.

Table 2: Initial placement

	(1) Empl, non-OECD	(2) Unempl, all
Age	0.0000409	0.000000127
	(0.50)	(0.02)
Female	0.00000161	-0.0000437
	(0.00)	(-0.45)
Married	-0.00484	-0.000156
	(-1.37)	(-0.50)
Any children	0.00262	-0.0000355
	(0.79)	(-0.14)
East Africa	-0.00867	0.000374
	(-0.96)	(0.47)
Southeast Asia	0.0174	-0.00144
	(1.39)	(-1.39)
South Asia	0.0101	-0.00107*
	(1.18)	(-1.77)
West Asia	0.00367	-0.000108
	(0.48)	(-0.17)
Secondary school	0.00219	-0.000252
	(0.94)	(-0.96)
College	-0.00333	-0.000107
	(-1.08)	(-0.48)
Observations	7393	7394
F observables	1.565	1.761

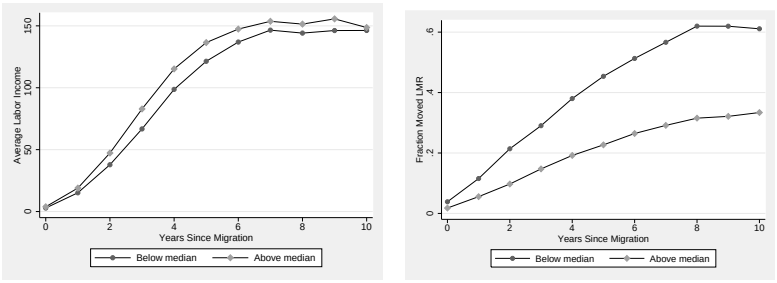
t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variable in column (1) is the immigrant employment rate in assigned region on at time of immigration (mean 0.46). In column (2) the dependent variable is the unemployment rate in the full population (mean 0.024). Model includes controls for year of immigration (not shown). Standard errors clustered at labor market region

also appears to increase substantially with time spent in the country. Persons placed in regions with above-median employment rates are less likely to move away from the assigned location; this difference increases with length of stay.

As discussed above, for this group of refugee immigrants region of placement is as good as random. The differences reported in Figure 1 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.

Fig. 1: Earnings & Mobility, by Initial Immigrant Employment Rate



(a) Average Labor Income

(b) Fraction Move Labor Market Region

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.

3 Empirical model

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 $(k = 1, ..., 10)$.

The basic model specification is:

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

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 characteristics 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 assigned labor market region 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. All estimates reported in this paper are ITT (intention-to-treat) measures, and as such capture the total effect of being assigned to an area, irrespective of whether the individual still lives there.

Table 3: Main regression estimates

	(1) Earnings	(2) Earnings	(3) Earnings	(4) Moved
e_0, YSM=1	27.63	-21.92	-158.0***	-0.497***
	(1.37)	(-0.89)	(-3.19)	(-3.51)
e_0, YSM=2	76.86***	-7.078	-80.11*	-0.416**
	(3.45)	(-0.25)	(-1.98)	(-2.65)
e_0, YSM=3	116.8***	46.94*	-7.823	-0.275*
	(4.74)	(1.71)	(-0.28)	(-1.81)
e_0, YSM=4	109.1***	88.03***	2.123	-0.334*
	(4.22)	(3.36)	(0.08)	(-1.76)
e_0, YSM=5	88.81***	129.8***	-16.56	-0.366**
	(2.73)	(2.95)	(-0.50)	(-2.48)
e_0, YSM=6	68.08**	101.5**	-52.60	-0.330**
	(2.10)	(2.14)	(-1.44)	(-2.20)
e_0, YSM=7	55.31	62.47	-70.67	-0.337**
	(1.56)	(1.21)	(-1.57)	(-2.24)
e_0, YSM=8	55.59	1.584	-65.17	-0.394***
	(1.37)	(0.02)	(-1.44)	(-2.89)
e_0, YSM=9	54.56	-26.09	-65.27	-0.325**
	(1.21)	(-0.33)	(-1.31)	(-2.18)
e_0, YSM=10	52.76	-62.09	-82.98	-0.280
	(1.08)	(-0.74)	(-1.59)	(-1.66)
e_t, same year			209.3***	-0.0774
			(4.80)	(-1.09)
Observations	57545	57545	57524	42902
Include additional local char.	No	Yes	No	No
Inlude same-year empl. rate	No	No	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 the assigned labor market region 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. e_0 and e_t denote the employment rate of non-OECD immigrants in the assigned region, 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.

Column (1) of Table 3 contains estimated effects of local immigrant employment rates on later labor earnings (full estimation results can be found in the appendix). These numbers correspond to the estimated $\hat{\gamma}^k$ of equation (2). Again, these are estimated ITT (intention-to-treat)-effects, capturing the effects of conditions in assigned area, not taking into account how long an individual remained in that location (or if they even settled there in the first place). 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.

The estimates are positive for all years up to ten years after immigration. From year 2 to 6, the point estimates are statistically significant at the five percent level; before and after this the estimates are smaller and less significant.

Equation (2) is estimated on an unbalanced panel, as cohorts arriving after year 2000 are observed for less than 10 years after immigration. As a result, there are fewer observation underlying estimated effects are estimated with less precision at higher values of years since migration (YSM).

The fact that estimates appear to diminish over time could also reflect mobility patterns over time. The proportion of the sample who are still living in the assigned labor market region is decreasing with time spent in the country (see next point). By year 6, 38% of the sample are living in another labor market region than the one they were assigned to. On average then, the link between labor market conditions in the assigned LMR and the local employment rate actually experienced will grow weaker with time.

Keeping this in mind, there are other reasons to expect results to diminish with time spent in Norway. Local labor market conditions could be more important during the period of adjusting to the Norwegian labor market, as more individuals at the margins of the labor market. That is, at higher YSMs, tho have become more attached to the labor market and thus less vulnerable to local labor market conditions compared to more recently arrived immigrants.

4 Model extensions

In the main model, the local immigrant employment rate is used as the only measure of local labor market conditions. The first 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). Column 2 of Table 3 show estimated effects of the local employment rate when the model is expanded to include effects of these other local characteristics on later earnings. Now, the initial immigrant employment rate has no significant effect on earnings before year 3 or after year 6. For years 3 - 6 after immigration, the estimated effects remain positive. 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 2 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 1 to 3 after immigration; after that there are no significant effects. One possible explanation for this is if local labor markets are 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.

As discussed in the introduction, the settlement program may attempt to settle newly arrived immigrants close to friends or family already residing in Norway - potentially leading to problems with identification. In this case, we would expect the employment rate among immigrants from a similar country background to be particularly important in explaining later outcomes. This would also be the case if the findings of the previous section were driven by networks or peer effects more generally.

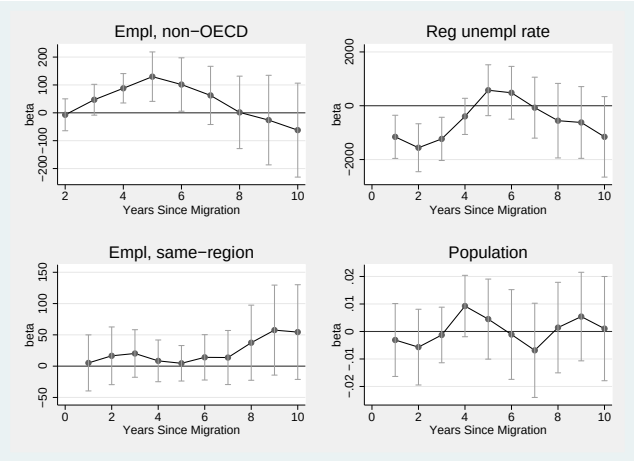
Looking at the third panel, there is little evidence for such effects. The initial local employment rate among immigrants from a similar country background has no significant effects on later outcomes, with the exception of a positive and significant estimated effect year 10 after immigration. Overall, the results do not appear to be driven by peer effects.

The fourth panel of Figure 2 shows estimated effects of the population size of the assigned labor market region. Surveys by Statistics Norway regarding attitudes toward immigration consistently find that people in more densely populated areas are more positive toward immigrants compared to individuals living in more rural areas (Blom, 2011). From this, we might expect that refugee immigrants who are settled in more populous regions do better in terms of labor earnings. However, this does not seem to be the case: conditional on other 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 1-3 years after immigration. Overall, estimated effects of local immigrant employment rate remain positive when controlling for a wider set of local conditions.

The next model extension examines the possibility that estimated effects are driven by differences in the construction of social programs, e.g. social assistance and labor market programs aimed at recently arrived immigrants. So far, all local explanatory variables have been measured at the labor market level, even though the settlement policy makes assignments to the municipality

Fig. 2: Estimates, model (2)



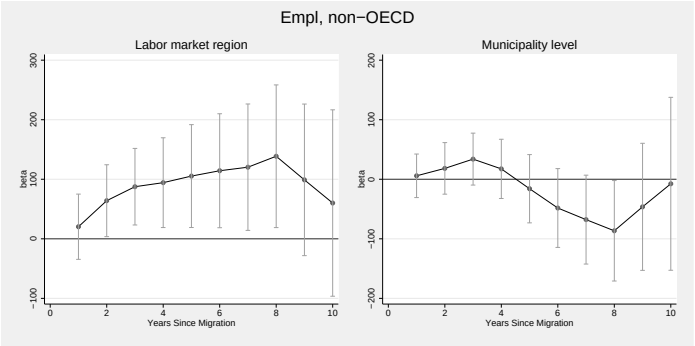
Note: Figure plots selected estimates with 95% confidence intervals. The dependent variable is total annual labor earnings. The model includes controls for gender, age, marital status, number of children, country of origin, year of immigration, and years since migration.

level. As discussed above, labor market regions typically consist of several municipalities; the definition of the regions is based on commuting patterns between municipalities, so if the estimated effects are truly due to variations in labor market conditions, labor market region should be the correct level to measure these variables at. Social programs, on the other hand, are designed at the municipality level, and thus tend to vary within labor market region.

To empirically assess this, an alternative specification of equation (2) is estimated where the immigrant employment rates computed at both the labor market region level and the municipal level are included as explanatory variables. Figure 3 shows estimated effects of the immigrant employment rate in each geographical level. The estimated effects appear to be entirely driven by variation across labor market regions. The estimated effects of the regional immigrant employment rate are positive and significant for most years. The estimated effects of the immigrant employment rate calculated at the municipality level are largely not statistically significant. The estimated effects shown in figure 3 suggests that the estimated effects are in fact driven by differences in labor market conditions and not variation in the design of social programs.

The estimates presented so far 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.

Fig. 3: Estimates, municipality and labor market levels



Note: Figure plots estimated effects of the initial immigrant employment rate on annual total labor earnings, with 95% confidence intervals. The model includes controls for gender, age, marital status, number of children, country of origin, year of immigration, and years since migration.

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 (equation (3)) is proposed to distinguish between these channels.

$$\begin{aligned} Y_{it} = &\sum_{k=1}^{10} ysm_{it}^{k}\delta^{k} + \sum_{k=1}^{10}(e_i \times ysm_{it}^{k})\gamma^{k} + e_{it}\eta \\ &+ \sum_{c}\theta_i^{c} + \sum_{s}\theta_i^{s} + x_i\beta + \varepsilon_{it} \end{aligned} \tag{3}$$

Here, e_{it} is the immigrant employment rate in the assigned labor market region 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 3. 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 region 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 (3) The dependent variable is replaced with an indicator variable equal to 1 if the person is currently living outside the initial labor market region. 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 of Table 3. When looking at moves out of the initial labor market region , the effect of the current year's employment rate is negative but not statistically significant. Meanwhile, the estimated effects of the initial immigrant employment rate remain 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.

Åslund (2005), using a Swedish settlement scheme, found a negative relationship between local immigrant employment rates and the propensity to move out of the municipality. The present model setup allows to investigate the dynamics of this relationship in more detail. While I also find significant effects of immigrant employment rates on mobility, this response appears to be delayed.

From figure 1, descriptive evidence indicates that refugee immigrants placed in low employment region are more likely to move compared to those placed in high employment regions. Meanwhile, this gap appears to widen over time, suggesting that the decision to move away from bad labor markets tends to be delayed rather than instant. The estimates reported in column 4 further support this interpretation.

5 Robustness and policy simulations

The main explanatory variable, the immigrant employment rate, is calculated using the fraction of the relevant population with reported labor earnings above a certain threshold. This measure may be problematic if it reflects local differences in wages, possibly correlated with variations in cost of living, rather than employment propensities. In other words, the positive estimates found so far could reflect that some labor market regions have higher nominal wages, rather than causal effects of labor market conditions. To address this, alternative specifications are estimated with the main explanatory variable re-defined as the fraction of the existing local immigrant population who are registered with positive labor earnings. That is, the earnings threshold when

computing the local employment rate is set to zero, to avoid picking up effects from differences in local nominal wage levels. These estimated models (not shown) yield estimated effects that are similar to those found in the previous models, in fact, with effects more statistically significant. Replacing the dependent variable, labor earnings, with a dummy equal to one if annual labor earnings are positive, estimates remain largely qualitatively consistent with effects reported in table 3.

In a related alternative specification, the model is estimated using the regional registered unemployment rate rather than the immigrant employment rate as the main explanatory variables. In these models, effects are qualitatively consistent with the findings in the preferred model, though the estimated effects are less persistent: higher local unemployment at the time of immigration significantly decreases own labor earnings up to four years after immigration.

The models above were estimated on an unbalanced panel. As a robustness test, models were estimated on a balanced panel where only persons who are observed for a full ten years are retained in the sample, i.e. only persons immigrating between 1993 and 2000 with no missing data for any years. The estimated effects are qualitatively similar (though less statistically significant). The dependent variable, labor earnings, is censored at zero. Alternative specifications using Tobit regression models were estimated to account for this, and estimates were largely similar to those of the preferred specifications reported above.

The estimated models all contain a large vector of control variables (112 variables in addition to local labor market characteristics), including dummies for country of origin. The panel data model of equation (2) essentially restricts the effects of these control variables to be similar across YSM. By imposing this restriction, I am able to increase the precision of the estimates. To assess the robustness of the estimates to this restriction, models have been estimated separately by years since migration. Given the significantly smaller estimation samples, these estimates will be less precise. Overall, estimated effects of initial immigrant employment rates are qualitatively similar to the estimates from panel models, but less significant.

To summarize, the employment prospects in the assigned labor market region of refugee immigrants seems to influence their later earnings. Extended models find that this does not seem to be driven by differences in population size, peer effects through friends and family, or local differences in social policy design. Moreover, I find no evidence of scarring effects. Estimated models suggest that local employment prospects are persistent, combined with limited and delayed geographical mobility: People do not immediately respond to adverse labor market conditions by moving to labor market regions where the chances of finding a job are higher.

Comparing my findings with those of Åslund and Rooth (2007), results are broadly consistent, but there are also significant differences. Both papers find that initial labor market conditions have lasting effects on later earnings and employment of refugee immigrants. Specifically, Åslund and Rooth find

that the initial local unemployment rate is found to have negative effects on later earnings and employment up to 11 years after immigration. However, the mechanisms driving this persistence are somewhat different. In the Swedish case, extended models attribute these effects to a combination of scarring effects and geographical immobility. In the current paper, using a similar model framework, I find little indication of scarring effects.

There are several potential mechanisms behind this difference. One potential explanation concerns identification as it relates to settlement data. Throughout these analyses, the identifying assumption remains that initial location should be random, conditional on observable characteristics. If this assumption is violated, e.g. if high-ability individuals are able to choose better labor markets, estimates would suffer from omitted variable bias, which would in turn show up as scarring effects. The present paper's use of assignment data, as well its focus on resettlement refugees should give a cleaner experiment, making it more likely that the identifying assumption holds, in turn yielding less scope for such bias. Another explanation could be related to differences in the macroeconomic context: average unemployment rates were higher in Sweden compared to Norway during large parts of the respective analysis periods (6.2% vs 3.7%[7]). In particular, the economic downturn of the 1990s resulted in unemployment rates up to 10% in Sweden. It seems plausible that there would be more scarring effects of early unemployment in that case, as it becomes more important to have some early labor market experience when unemployment is high and there is more competition for vacant positions.

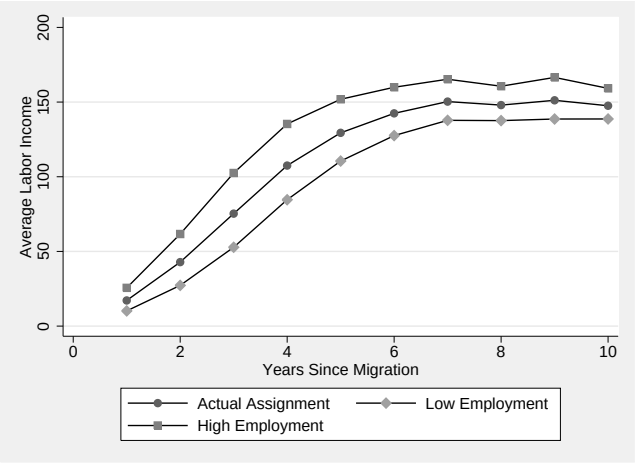
The estimated models find that immigrants assigned to regions with good employment prospects tend to have higher later labor earnings. In order to get an impression of the quantitative importance of these effects, the basic model estimated above is used to predict average earnings under alternative settlement policies. First, the model is 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 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

[7] Source: OECD harmonized unemployment rates.

Fig. 4: Policy simulations



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

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. Estimates indicate that local labor market conditions are important for the labor market outcomes of refugee immigrants. Assigning refugee immigrants to regions with good immigrant labor markets increases later total labor earnings.

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. In addition, the extended models indicate that effects are not driven by local differences in welfare policies, designed at the municipality level.

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. 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.

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 22% 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 18% 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.

The findings of this paper may have implications related to the consequences of settlement programs. Integrating newly arrived refugee immigrants is generally considered costly for local communities, as municipalities are required to provide housing, health services etc as well as welfare benefits until the settled individuals are able to support themselves by other means. The settlement program studied in this paper distributes these costs of integrating newly arrived refugee immigrants across municipalities. The limited mobility of new refugee immigrants is in some ways a wanted consequence of the settlement program, as welfare payments and training programs are largely conditional on remaining in the assigned municipality. Meanwhile, my findings indicate that this limited mobility may in some cases be a barrier to successful labor market integration. Refugee immigrants who are placed in bad labor markets are found to have lower earnings for years following immigration.

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 and 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

Sorting on observables: T-tests

As an additional check for sorting on observables, I have run a series of t-tests for differences in means between those placed in regions with low employment (at or below sample median, calculated by year of arrival) and those placed in regions with high immigrant employment rates (above the sample median). Results from these t-tests are shown in table 4. This exercise reveals some differences. The most pronounced differences are in terms of geographical background, i.e. region of origin. Refugee immigrants from East Africa are over-represented in low employment regions, while persons from Southeast Asia are over-represented in high employment regions. The tests also find differences in terms of demographics: being female and married is associated with a lower probability of being placed in high employment regions. For the variables arguably most closely related to unobserved components of the earnings potential, indicators of high school and college degrees, as well as age, there appears to be no differences between the two groups.

Table 4: T-tests

	Low empl	High empl	Diff
Age	31.75	31.67	0.0787
			(0.204)
Female	0.451	0.429	0.0214*
			(0.0116)
Married	0.593	0.567	0.0262**
			(0.0115)
Any children	0.596	0.580	0.0159
			(0.0115)
East Africa	0.123	0.0741	0.0493***
			(0.00697)
Southeast Asia	0.172	0.232	-0.0596***
			(0.00930)
South Asia	0.292	0.307	-0.0152
			(0.0107)
West Asia	0.182	0.187	-0.00544
			(0.00903)
Secondary school	0.164	0.169	-0.00432
			(0.00868)
College	0.189	0.181	0.00798
			(0.00906)
Observations	3,898	3,496	

Full estimation results

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

Table 5: Full estimates

	Earnings	Earnings	Earnings	Moved
	(1)	(2)	(3)	(4)
Age at imm.	-1.766***	-1.771***	-1.759***	-0.000971***
	(-8.50)	(-8.53)	(-8.43)	(-3.41)
Married	16.65***	16.87***	17.39***	0.00141
	(3.53)	(3.58)	(3.64)	(0.29)
Female	-45.88***	-45.86***	-45.80***	0.0000894
	(-12.09)	(-12.09)	(-12.08)	(0.06)
High school	20.49***	20.45***	20.14***	-0.00271
	(5.22)	(5.18)	(5.19)	(-0.51)
College	42.42***	42.31***	42.50***	0.0121**
	(11.60)	(11.57)	(11.53)	(2.14)
Family size 1	68.05***	68.01***	69.99***	0.000334
	(5.96)	(6.08)	(6.14)	(0.02)
Family size 2	50.91***	50.72***	52.73***	-0.00373
	(5.22)	(5.32)	(5.32)	(-0.23)
Family size 3	52.61***	52.43***	53.85***	-0.0182
	(6.24)	(6.35)	(6.40)	(-1.17)
Family size 4	51.85***	51.40***	53.11***	-0.0233
	(7.94)	(8.15)	(8.20)	(-1.54)
Family size 5	41.34***	40.80***	42.68***	-0.0137
	(6.85)	(6.86)	(6.77)	(-0.92)
Family size 6	38.63***	38.51***	39.15***	-0.0131
	(5.50)	(5.69)	(5.62)	(-0.77)
Family size 7	20.49***	20.02***	21.39***	-0.0202
	(3.20)	(3.12)	(3.24)	(-1.28)
Family size 8	20.90**	20.27**	21.65**	-0.00393
	(2.54)	(2.39)	(2.66)	(-0.24)
Family size 9	3.482	3.205	4.214	-0.0217
	(0.50)	(0.47)	(0.59)	(-0.78)
Imm year 1994	-14.99**	-17.11***	-15.04**	-0.00619
	(-2.61)	(-2.94)	(-2.64)	(-0.74)
Imm year 1995	-37.67***	-41.39***	-37.88***	-0.00749

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(-5.22)	(-5.26)	(-5.30)	(-0.66)
Imm year 1996	-41.34***	-45.18***	-37.30***	0.0106
	(-5.23)	(-4.79)	(-4.44)	(0.71)
Imm year 1997	-23.52***	-30.24***	-15.67*	0.0326
	(-3.10)	(-3.09)	(-1.91)	(1.66)
Imm year 1998	-21.43**	-30.52**	-9.290	0.0497**
	(-2.22)	(-2.28)	(-0.84)	(2.15)
Imm year 1999	2.859	-6.161	9.993	0.0708***
	(0.31)	(-0.54)	(1.09)	(3.28)
Imm year 2000	-6.607	-14.91	-2.617	0.0748***
	(-0.71)	(-1.20)	(-0.29)	(3.90)
Imm year 2001	-6.690	-14.81	-0.0412	0.0685**
	(-0.83)	(-1.35)	(-0.01)	(2.48)
Imm year 2002	-21.45**	-28.33**	-16.53*	0.0708***
	(-2.54)	(-2.68)	(-1.97)	(2.82)
Imm year 2003	-3.077	-7.346	-5.180	0.0329
	(-0.35)	(-0.66)	(-0.58)	(1.27)
Imm year 2004	5.694	1.212	-1.300	0.0224
	(0.60)	(0.11)	(-0.14)	(0.85)
Imm year 2005	-12.68	-18.28	-22.67**	0.0156
	(-1.22)	(-1.43)	(-2.29)	(0.58)
Imm year 2006	-9.892	-21.52	-18.56*	0.0349
	(-0.94)	(-1.54)	(-1.83)	(1.13)
Imm year 2007	-15.19	-32.24**	-19.13*	0.0410
	(-1.40)	(-2.15)	(-1.74)	(1.21)
Any children	10.17	10.78	10.05	0.00507
	(1.47)	(1.57)	(1.43)	(0.67)
Age youngest child	0.0177	0.0360	-0.00575	-0.000381
	(0.07)	(0.15)	(-0.02)	(-1.38)
Belarus	409.6***	407.3***	402.7***	-0.0808***
	(62.58)	(54.91)	(54.55)	(-3.44)
Croatia	52.47***	51.70***	53.74***	-0.0485**
	(4.45)	(4.44)	(4.48)	(-2.65)
Poland	-118.4***	-120.3***	-121.8***	0.853***
	(-19.17)	(-15.56)	(-18.68)	(39.12)
Russia	15.63	12.70	11.64	-0.0328
	(0.69)	(0.53)	(0.51)	(-1.44)
Turkey	26.44	30.16	24.84	0.0226

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(0.97)	(1.10)	(0.90)	(0.38)
Slovenia	177.6***	173.6***	181.8***	-0.0568*
	(16.30)	(17.97)	(17.39)	(-2.00)
Bosnia Hercegovina	58.06***	57.22***	58.94***	-0.0329
	(5.14)	(5.09)	(5.21)	(-1.35)
Macedonia	126.4***	125.4***	129.7***	-0.0192
	(3.17)	(3.15)	(3.27)	(-0.30)
Serbia	85.43***	84.16***	85.42***	-0.0218
	(3.34)	(3.28)	(3.32)	(-0.66)
Montenegro	-42.26**	-42.72**	-44.48**	-0.00397
	(-2.18)	(-2.10)	(-2.60)	(-0.08)
Kosovo	23.00*	21.71	22.70*	-0.0447**
	(1.69)	(1.65)	(1.69)	(-2.09)
Algeria	16.16	18.20	14.54	0.0310
	(0.68)	(0.78)	(0.61)	(0.59)
Burundi	14.82	17.80	12.65	-0.0204
	(1.52)	(1.60)	(1.26)	(-1.15)
Benin	-25.49***	-23.76**	-32.90***	-0.0206
	(-2.86)	(-2.58)	(-3.28)	(-0.90)
Ivory Coast	-0.455	8.897	-4.459	0.00896
	(-0.01)	(0.18)	(-0.10)	(0.14)
Eritrea	33.41***	36.88***	29.12**	-0.0344*
	(2.74)	(3.06)	(2.26)	(-1.76)
Ethiopia	25.36***	31.03***	21.98***	0.0162
	(3.44)	(4.06)	(2.84)	(0.70)
Egypt	-14.51	-11.36	-14.39	-0.0174
	(-0.32)	(-0.25)	(-0.33)	(-0.42)
Gambia	-77.16***	-68.12***	-72.04***	0.148***
	(-8.01)	(-5.45)	(-7.49)	(3.95)
Ghana	-107.2***	-89.12***	-113.0***	0.244***
	(-9.54)	(-7.78)	(-9.22)	(7.91)
Cameroon	118.0***	121.8***	118.6***	-0.0345
	(11.01)	(11.84)	(13.89)	(-1.35)
Kenya	29.07	34.38	25.62	-0.0609*
	(0.68)	(0.82)	(0.60)	(-1.78)
Kongo Rep	-27.17**	-22.81*	-25.39**	-0.0346
	(-2.51)	(-1.88)	(-2.23)	(-1.42)
Kongo DRC	10.51	16.36	9.873	0.00520

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(1.14)	(1.58)	(1.06)	(0.28)
Liberia	-2.652	1.190	-4.235	-0.00132
	(-0.33)	(0.15)	(-0.51)	(-0.08)
Libya	-126.8***	-117.8***	-135.1***	0.839***
	(-13.57)	(-13.85)	(-14.47)	(36.37)
Nigeria	-14.71	-11.81	-19.17	-0.0577
	(-0.23)	(-0.18)	(-0.31)	(-1.55)
Zimbabwe	6.857	10.89	0.777	0.0955
	(0.46)	(0.82)	(0.05)	(1.03)
Rwanda	36.42***	40.33***	33.48***	-0.0311
	(3.56)	(4.20)	(3.35)	(-1.66)
Sierra Leone	34.63***	35.06***	32.15***	-0.0602***
	(3.32)	(3.40)	(3.07)	(-3.34)
Somalia	-42.82***	-38.09***	-44.01***	-0.0333*
	(-4.45)	(-3.98)	(-4.54)	(-1.68)
Sudan	-9.296	-8.360	-10.32	-0.0258
	(-1.28)	(-1.17)	(-1.40)	(-1.37)
Tanzania	-41.42*	-35.71	-41.19*	0.0124
	(-1.91)	(-1.55)	(-1.91)	(0.26)
Chad	-77.38***	-76.02***	-80.32***	-0.0718***
	(-8.28)	(-8.13)	(-8.17)	(-4.10)
Togo	-9.962	-2.368	-6.808	0.0752***
	(-1.44)	(-0.33)	(-0.98)	(3.19)
Tunisia	31.22	35.12	31.92	-0.00992
	(1.09)	(1.24)	(1.04)	(-0.18)
Uganda	0.392	5.051	-3.267	0.110***
	(0.03)	(0.34)	(-0.23)	(2.70)
Zambia	71.81*	74.66*	68.02*	-0.0602**
	(1.97)	(2.01)	(1.90)	(-2.66)
Afghanistan	-8.044	-7.891	-10.21**	0.0138
	(-1.62)	(-1.53)	(-2.03)	(1.02)
Azerbaijan	-28.96	-25.19	-31.36	0.00392
	(-0.75)	(-0.65)	(-0.80)	(0.12)
Myanmar (Burma)	9.209	9.222	8.998	-0.0517***
	(0.95)	(0.96)	(0.96)	(-2.70)
Sri Lanka	74.55	75.37	70.66	0.000811
	(1.58)	(1.62)	(1.56)	(0.01)
The Philippines	54.49***	55.97***	56.16***	-0.0583**

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(3.72)	(3.79)	(3.90)	(-2.40)
Georgia	58.27***	60.53***	53.49***	0.176***
	(9.59)	(10.20)	(8.20)	(6.80)
India	18.92	20.20	15.35	-0.0106
	(1.02)	(1.09)	(0.87)	(-0.26)
Indonesia	22.58***	22.83***	15.39**	-0.0625***
	(4.06)	(4.05)	(2.42)	(-2.81)
Iraq	-30.67***	-27.14***	-31.03***	-0.0264**
	(-4.73)	(-4.36)	(-4.73)	(-2.17)
Jordan	9.452	11.49	9.767	0.103*
	(0.14)	(0.17)	(0.15)	(1.79)
Cambodia	-9.810	-10.49	-10.15	-0.0653***
	(-1.46)	(-1.56)	(-1.47)	(-4.05)
Kazakhstan	57.79*	62.03*	53.19*	0.0239
	(1.82)	(1.94)	(1.71)	(0.34)
China	-26.36***	-25.44***	-29.49***	-0.0214
	(-3.31)	(-3.03)	(-3.43)	(-1.32)
Kuwait	-36.43***	-33.65***	-31.01**	-0.00159
	(-2.73)	(-2.81)	(-2.22)	(-0.04)
Kyrgyzstan	-90.74*	-87.05*	-99.30**	-0.0425**
	(-1.90)	(-1.83)	(-2.04)	(-2.42)
Laos	-44.84***	-44.90***	-41.60***	-0.0603**
	(-4.18)	(-4.26)	(-3.97)	(-2.63)
Lebanon	6.083	9.658	1.562	0.00151
	(0.15)	(0.24)	(0.04)	(0.03)
Malaysia	40.00***	40.68***	36.36***	-0.0664***
	(9.01)	(8.35)	(8.93)	(-2.79)
Palestinian territories	-89.48***	-87.56***	-90.93***	-0.0676***
	(-7.12)	(-7.65)	(-7.70)	(-3.21)
Nepal	56.49***	57.42***	59.01***	-0.0506**
	(10.24)	(9.69)	(10.79)	(-2.41)
Pakistan	-3.254	-4.469	-7.436	-0.00947
	(-0.19)	(-0.24)	(-0.40)	(-0.34)
Saudi Arabia	6.802	9.011	6.511	0.0179
	(0.34)	(0.44)	(0.34)	(0.53)
Singapore	-22.86	-21.80	-21.92	0.0317
	(-0.43)	(-0.41)	(-0.41)	(0.33)
Tadzjikistan	116.5***	121.5***	118.1***	-0.0355

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(3.04)	(3.49)	(3.38)	(-1.57)
Turkmenistan	-17.48	-11.22	-20.40	0.0597
	(-0.94)	(-0.60)	(-1.08)	(0.80)
Uzbekistan	-15.17	-10.28	-16.64	-0.0292
	(-0.43)	(-0.28)	(-0.48)	(-1.11)
Syria	-49.52***	-46.30***	-51.76***	-0.0349
	(-5.27)	(-5.07)	(-5.38)	(-1.04)
Thailand	-12.65	-11.42	-12.95	-0.0523**
	(-0.82)	(-0.75)	(-0.83)	(-2.33)
Vietnam	23.63***	23.93***	21.70***	-0.0489**
	(3.20)	(3.36)	(2.83)	(-2.47)
Yemen	-6.620	-5.284	-14.58	-0.0633**
	(-0.62)	(-0.51)	(-1.26)	(-2.35)
Cuba	8.003	4.708	0.264	-0.0255
	(0.71)	(0.43)	(0.02)	(-0.93)
Dominican Rep	-63.94***	-63.74***	-62.98***	-0.0757***
	(-6.30)	(-6.76)	(-6.12)	(-3.63)
Chile	39.13***	37.26***	35.82***	-0.0494**
	(4.65)	(4.65)	(4.32)	(-2.04)
Colombia	9.121	7.258	9.039	-0.0397
	(0.20)	(0.16)	(0.19)	(-1.35)
Peru	93.51***	92.22***	91.16***	-0.0564**
	(11.31)	(11.01)	(11.16)	(-2.03)
YSM: 2	3.013	24.91**	-12.16	-0.0406
	(0.47)	(2.48)	(-1.43)	(-0.74)
YSM: 3	17.10	22.21	-15.29	-0.101*
	(1.60)	(1.39)	(-0.95)	(-1.74)
YSM: 4	53.25***	17.25	9.589	-0.0796
	(4.28)	(0.59)	(0.51)	(-1.08)
YSM: 5	85.34***	-1.271	37.62*	-0.0773
	(5.39)	(-0.03)	(1.71)	(-1.20)
YSM: 6	108.1***	25.11	65.20***	-0.0996
	(6.76)	(0.60)	(2.90)	(-1.65)
YSM: 7	123.8***	68.30	81.04***	-0.113*
	(6.97)	(1.56)	(3.76)	(-1.75)
YSM: 8	124.4***	97.47*	76.66***	-0.0954**
	(6.10)	(2.01)	(3.11)	(-2.06)
YSM: 9	127.6***	105.4*	77.45***	-0.130**

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Table 5 – continued from previous page

	Earnings	Earnings	Earnings	Moved
	(5.83)	(2.00)	(2.95)	(-2.36)
YSM: 10	128.9***	137.4***	83.75***	-0.151***
	(5.35)	(2.72)	(2.96)	(-2.80)
Constant	11.88	66.59**	-3.216	0.366***
	(0.74)	(2.31)	(-0.19)	(5.61)
Observations	57545	57545	57524	42902

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: Table shows full estimates from models of table 3. 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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