Mobility and Youth Unemployment

Does internal migration influence earnings following youth unemployment?

Evidence from the Swedish Labor Market

Pontus af Burén

Student
Semester 2016
Bachelor Thesis, 15 ECTS
Business and Economics

Abstract

The rise of youth unemployment in Sweden has highlighted the need for more research investigating the effects of unemployment on future earnings. Earlier research suggests that youth unemployment will leave labor market scares on a person's future income. Therefore, in this thesis I investigate the question if migration can increase an individual's future earnings and hence work as a cure for scared individuals. My theoretical argument is that migration is an investment in human capital which will lead to higher future income and may heal scars. To test my argument, I use Swedish panel data from the ASTRID database, examining unemployed individuals born 1979-1983 after finishing their education in Sweden. A nearest-neighbor propensity score matching method is applied to estimate the casual effects. However, the results show only few significant results over time and age cohorts in the data. Therefore, my conclusion is that migration barley increases income of young unemployed individuals.

Table of Contents

1. Introduction

The rising youth unemployment during the last decade in Europe has underlined the importance for society as well as research to gain more and broader knowledge in this area. This is especially true for Sweden. The youth unemployment rate for the cohort of individuals 15-24 years old in Sweden was 21.7% in April 2016 (SCB 2016). This is rather high in comparison to other EU countries such as Germany (7.1%) or Denmark (11.1%) (ec.europa 2016). It highlights the urgency to examine and to understand more about the effects of youth unemployment in general, and particularly in Sweden. There are different approaches of political reform trying to increase income for individuals who have or had spells of unemployment. In this paper, I investigate if there are other methods which do not tackle the unemployment itself. Hence, I will apply an innovative approach by introducing an original variable to the field: the impact of internal migration on future earnings.

Studies that investigate income effects after unemployment show that youth unemployment may persistently punish one's future income, the term frequently used for this phenomena since early literature is: *scarring effect* (Ellwood 1982). It is deduced from the word *scar* meaning that an individual receives a labor market scar from youth unemployment. An easy way to describe scarring effects is to distinguish two types of income losses which derive from unemployment: If an individual is unemployed, it will (1) have instantaneous earnings losses compared to an employed individual; but also (2) future earnings losses even when back in employment. The latter effect is called scarring effect.

Given that scarring effects actually exist, my research question is: *Can migration help mitigate income scarring effects of youth unemployment?*

My main theoretical argument is that by moving, an individual who is unemployed will invest in human capital. This will increase income for migrants, and hence scarring effects are likely to be reduced. In order to break down the large field, I focus exclusively on future income as central dependent variable in this analysis and investigate only young individuals. Thus, I study future incomes of individuals in Sweden who have finished upper secondary school education (Swedish *gymnasium*) and became unemployed afterwards. Data was collected for individuals born between 1979 and 1983 to create five different cohorts. The data is organized as panel data and is sourced from the ASTRID database. The database includes a rich set of observable characteristics which allows using a nearest-neighbor propensity score matching method to investigate if these positive effects from migration on young unemployed individuals exist empirically.

The study's focus is on the cohort with the longest follow-up time: individuals born in 1979 in Sweden. The results indicate a positive trend on income from migration for the cohort in focus. However, the results for some of the follow-up years after migration show negative estimates and far from all observed years have significant results. Additionally, when comparing the income from migration between the cohort with individuals born in 1979 and the other cohorts, rather large differences in the estimates are observed. Hence, my conclusion is that the positive trend for the cohort in focus is partly due to randomness and there is apparently barely any income gain from migration for individuals in my dataset.

The reminder of this thesis is organized as follows: In the next section previous studies regarding internal migration as well as scarring effects are reviewed and my contribution is explained. In section 3, I state my theoretical assumptions and deduce my main theoretical argument. The data and operationalizations are presented in section 4 and in section 5 the method is clarified. Section 6 contains the description of the results, whilst the conclusions are drawn in section 7.

2. Background

In the following, I first, present the two main concepts of the thesis: internal migration as well as scarring effects. I then define the terms and review the relevant literature and finally, explain how this paper contributes to the field.

2.1 Internal Migration

Internal migration is defined as a movement from one specified area to another (or a move of some defined distance) in the same county (Greenwood 1997: 650). It is convenient to classify the migration literature into two broad areas of research. One branch deals with determinates of movement (e.g. DiVanzo 1978; Greenwood 1997; Pissarides and Wadsworth 1989; and Gordon 1985), while the other one treats the consequences of migration (e.g. Nakosteen and Zimmer 1980; Hunt and Kau 1985; Nakosteen and Westerlund 2004; and Boman 2011).

The purpose of migration can arises from a wide range of different factors. Studies trying to explain determinates of migration include differential characteristics of both sending and receiving regions mixed with individual and/or family variables. Regional factors can be housing markets, taxes, unemployment, the availability of public goods, etc. Climatological and environmental amenities can also play a part in the decision to move. Different life-cycle aspects are e.g. marriage, divorce, finishing schooling, entering the labor market, birth, home ownership, aging, leaving home of children, and retirement. Other personal/family characteristics and treats are important, such as earnings, employment status, education, age, sex, etc.

One of the best documented correlations is the relationship between age and migration (Greenwood 1997: 665). The probability for migration is highest in the mid-twenties and declines after this age. More contested is the correlation between education and migration, but the general trend is an increase in movement with education. Another correlation is observed between distance and migration. The migration decreases with growing distance from a certain starting point. A way measuring this can be using "distance elasticity of migration" which is the percentage change of migration from *k* to *l* that results from a 1% change in the distance between *k* and *l*, other factor ceteris paribus. The elasticity usually ranges between -0.1 to -2.0 (Greenwood 1997: 667).

Furthermore, research has found interesting relations between migration and different employment statuses (e.g. personal unemployment, regional unemployment, and national unemployment). DaVanzo (1978) was one of the first using microdata from the U.S. and found that unemployed persons are more likely to move compared to employed. Additionally, she concludes that higher unemployment rates in the area of residence lead to higher propensity of migration for the individuals who are unemployed. In contrast, the regional unemployment rate does not have an effect on employed persons. A reason why economists have problems finding a relationship concerning migration and regional unemployment rate, is that unemployment is a very small fraction of the labor force (at least in the past).

Another explanation is brought forward by Fields (1976). He states that the actual variable of interest is wrongly measured and should consist of job-turnovers instead of the actual unemployment rate. Gordon (1985) uses data on Great Britain and finds evidence for lower migration during slumps and vice versa for recoveries. Pissarides and Wadsworth (1989), also examining Great Britain, conclude that unemployed individuals have a lower probability to migrate if national unemployment is high.

Moving the focus to the consequences of migration, most studies use some kind of earning model evolving from this simple model:

$$\ln w = X\alpha + D\beta + Z\gamma + \varepsilon$$

where w is wage, X is a vector of personal characteristics, D a vector of migration characteristics (e.g. a dummy for migration or not), Z is a vector of regional characteristics, and ε is the error term. Nakosteen and Zimmer (1980) use a switching regression model controlling for selection bias and find strong positive effects on income for individuals who migrated. Also Robinson and Tomas (1982), using Canadian data, point out the importance of controlling for a sample-selection bias and find higher wages on migrants only after they corrected for the bias. Hunt and Kau (1985) find no effect on income for first-time movers, but instead for two time movers who receive a 13% wage gain. Grant and Vanderkamp (1980) conclude that the earning drops directly after migration and take a long time until positive effects on earnings can be observed.

Jacobsen and Levin (1997) study differences in income for different sexes, martial statuses, and education levels. They find evidence that single females, especially if highly educated, have higher incomes due to migration. The opposite holds for low educated men, whilst higher educated have a positive income due to migration. In contrast for couples, only men have a higher income after they migrated as a couple whereas women tend to have lower incomes. Latest research with Swedish data finds positive returns on income from migration for higher educated individuals, the effects on lower educated is not significant (Tano 2014). Korpi et al. (2010) point out the importance of using regional housing cost when estimating income changes from migration. They found that people moving from rural to urban areas increase their income more significantly than individuals moving the other direction. When they also included the regional housing cost into the model, the results of migration on disposal income were still positive. However, moving from an urban to a rural area increases disposal income more than the other way around.

2.2 Scarring effects

Generally, the evidence suggests that scarring effects exist. However, it is still unclear how persistent the effect is. Variation in the countries' labor markets and the difficulties of controlling for individual heterogeneity are likely reasons for the ambiguous results.

Already early, Heckman and Borjas (1980) investigated the effect of unemployment and pointed out the difficulties to distinguish between casual effects and unobserved heterogeneity. For example if some individuals are observed as being (un)employed and having (low)high wage constantly over time, it can be because of scarring effects or because there are differences in unobserved characteristics (e.g. motivation). Granted that these differences between units continue to be uncontrolled, the estimations will be misleading. As a result of the importance to control for unobserved characteristics, most of the literature regarding scarring effects has focused on different methods to approach the heterogeneity problem (e.g. Mroz and Savage 2006; Arulampalam 2001; Gregg and Tominey 2005; and Gregory and Jukes 2001).

The parallel branch of literature focuses on the probability of future employment after an individual's youth unemployment instead of earnings differences. Here the phenomenon is rather referred to *structural* or *state dependence* instead of scarring effects. Arulampalam et al. (2000) use a random effect model on the English labor market and find strong evidence for state dependence. Burgess et al. (2003) apply another technique with a cohort analysis and find significant wage losses, in particular for the unskilled workforce, in a cohort entering the labor market with high youth unemployment. Other research on the English labor market includes Gregg (2001) who focuses on aggregated unemployment for men as an instrumental variable and finds evidence for structural dependence for an individual's unemployment.

Elwood (1982) uses data from National Longitudinal Survey of Youth (NLSY) and studies the American labor market. He finds only minor structural dependence on unemployment from early joblessness but major scarring effects on wages. In addition, Heckman and Borges (1980) find little evidence for true state dependence. Mroz and Savage (2006), also using the NLSY data, examine the long term effect of youth employment. They conclude that there exist labor market scars, but they find also a "catch-up" effect on both: the probability to get employed as well as on future earnings. Hence, their findings suggest that scarring effects decrease over time. Furthermore, they find evidence that the "catch-up" effect is bigger on the likelihood of finding new employment than the effect on future wages.

Literature focusing on earnings includes Arulampalam (2001). She uses a difference in difference matching model with help of a rich dataset from the British Household Panel Survey to control for systematic selection into unemployment. The estimates show a 6% wage penalty of re-entering the British labor market after a spell of unemployment. This number increases to 13% after three years until it declines afterwards. Moreover, she also investigates three effects which potentially could increase the wage penalty: cause, duration, and previous spell of unemployment. Persons, who experienced spell of non-employment after being made redundant, were less scared than others. No effect of the spell's duration was found, and previous spell of unemployment also carries a wage scar but not as strong as the first period of joblessness. A study using an administrative data set (Gregory and Jukes 2001) estimates the effect to be around 10% lower wages for an individual after returning to work. However, this effect declines steadily over time. Additional, Gregory and Jukes (2001) find empirical proof that young unskilled workers suffer least from scars and that the length of unemployment period increases scars. Gregg and Tominey (2005) focus on long lasting scars (20 years) of youth unemployment and find significant scars for (9-11%) lower wage for an individual at the age of 42 from youth unemployment.

Literature on the Scandinavian labor market includes e.g. Nordström Skans (2004). He applies a sibling fixed effect method on post-graduation unemployment to capture the unobserved individual attributes and finds 17% reduction in annual income after five years. Furthermore, Hämäläinen (2003) uses a special type of random effect model and finds state dependence in Finland, especially for low-educated workers. More recent research by Gartell (2009) finds major scars of 30% lower earnings five years after post-graduation unemployment from university, while Nilsen and Holm Reiso (2011) observe true state dependence with data from the Norwegian labor market, using a nearest-neighbor propensity score matching method.

There are different theoretical arguments explaining the empirical results. But the most prominent approach is the human capital insight (Becker 1964). Becker argues that wages are determined by marginal productivity, whereas marginal productivity is influenced by human capital. An individual who is jobless will not only lose firm specific skills compared to a person with employment, but also obtain deprecation of general skills. These skill losses will therefore result in lower wages. However, the re-entry into the labor market will increase human capital again, hence as long there are diminishing returns of extra tenure, and scarring effects are likely to disappear over time.

Moreover, scarring effects can partly be explained via discrimination. Since marginal productivity can be hard to measure for employers, they take unemployment history as indicator to sort employees into groups of accomplishment and performance (Lookwood 1991). This will result in relative lower wages if the employer decides to pay less for individuals with unemployment history.

However, there are theories challenging these arguments. One competitive argument concentrates on the matching between employee and firm perspective to explain scarring effects. If a worker is badly matched with a company, the worker can through unemployment find a better match, this will increase the marginal productivity and wages respectively (Gregory and Jukes 2001). Lastly, other factors than the unemployment itself can explain the empirical findings. The most frictionally used is the initial labor market condition. A worker entering the labor market with high aggregated unemployment will have lower wages in the future if these labor market conditions are persistent. However, it can be assumed that if there is an insider-outsider effect, constrains of persistent labor market do not necessary need to be fulfilled to observe scarring effects. The insider-outsider theory can be explained by the fact that the group of individuals (insiders) dominates the salary negotiation. When the demand for labor increases, the insiders have no interest to reduce unemployment. Instead they want the better demand to increase wages (Björklund et al. 2006: 317).

2.3 Contribution of the Thesis

There is plenty of evidence suggesting that scarring effects actually exist. In contrast, the magnitude and the persistence of the effect are more unknown and rather contested. Additionally, it is not certain how different subgroups of the population are affected by youth unemployment and how scarring effects are affected by labor market conditions. Regarding internal mobility, different factors may affect the migration decision, but most of the studies find an increase in income from migration after controlling for individuals' characteristics.

To the best of my knowledge, there are no studies connecting scarring effects with mobility or migration. Arulampalam et al. (2000) argue and conclude that younger workers who are more mobile will suffer less from scarring effects. But since they actually do not estimate migration, the conclusion derives solely from arguments based on theoretical assumptions. There are previous studies on Swedish data indicating that unemployed individuals gain income from migration (e.g. Nakosteen and Westerlund 2004). Boman (2011) finds positive effects from migration for men and negative effects for women following job displacement. However, no studies so far have focused entirely on young unemployed individuals. Therefore, my contributions are that I connect scarring effects with migration and my main contribution is investigating the income effects from migration on young unemployed individuals.

3. Theory

One of the most applied theories explaining scarring effects is the loss of human capital. Loosely based on Becker (1964), human capital can be explained as a stock of personal characteristics or traits that affects the ability to create work. A commonly used example is that education is an investment in human capital which will increase an individual's marginal productivity. A measure to tell if the education made the productivity raise could be to look at earnings before and after the education treatment or to compare earnings between individuals who undergo education and the ones who do not.

According to human capital theory, a person who got unemployed before the decision to migrate or not, will lose an amount of their human capital. Major explanations behind the lower future earnings for young unemployed from a human capital perspective are: 1) the lower level of human capital in the future compared to the persons who are employed, since individuals increase their human capital while working. 2) Persons can reduce their human capital if they do not use specific parts of their knowledge regularly.

To connect scarring effects with migration, I will use the argument first brought forward by Sjaastad (1962). He argues that migration is a positive investment in human capital. This will be displayed for example in the future income for migrants and non-migrants. Following this, I assume migrants to have a higher income. Thus, migration is a factor which may mitigate scarring effects.

The correctness for using a human capital model for migration has been widely discussed and depends on the validity of the assumption that migration is first and foremost an economic investment. Both Bertel (1979) and Hunt and Kau (1985) support this assumption. To further build up my theory, every other factor that affects the migration decision ceteris paribus, the present value of investment in human capital can be measured as followed:

$$PV_{kl} = \sum_{t=1}^{T} \left[\left\{ \frac{1}{(1+r)^t} \right\} [(Y_{lt} - C_{lt}) - (Y_{kt} - C_{kt})] - C_{kt} \right.$$

In this case the Y´s represent the earnings in areas *k* and *l* at time t, r is the discount rate, T is a denotation for an individual's remaining period of life, C is the region costs in *k* and *l* at time t, and C_{kl} is the cost moving from region *k* to region *l*. An individual living in *k* is rational and will select that destination where PV_{kl} is maximized. The existence of these positive investments in human capital is due to different earnings/costs in different regions.

The measurement of human capital means that all future incomes must be taken into consideration in the decision making about moving as I assume persons to take decisions on the labor market rationally and long-sighted. This means that migration can still be seen as an investment in human capital even if the first years after migration have a negative effect on earnings. However, since the presence of the discount rate in the investment in human capital formula, relative income increases for migrants should start to show at least ten years after the actual act of migration.

The correctness to only use income from working to investigate the effect of migration can be discussed as the formula shows. I argue that the costs of moving from one place to another are rather small, compared with the wins from the remaining life earnings. Moreover, the costs of living at different places can be taken into consideration calculating income (Korpi et al. 2010). However, I assume the wins from future earnings to be significantly higher than possible losses because of variations in the costs of living.

Summarizing up my theoretical approach, I argue that mobility, i.e. moving to another labor market, will increases individual's future earnings and will thus result in the decrease of scarring effects after youth unemployment. Hence, my hypothesis states: *Moving to another labor market reduces income scarring effects of youth unemployment.*

4. Data

To test the hypothesis empirically, longitudinal data from Swedish population registers is used. The source of data is the ASTRID database at the Department of Geography and Economic History at Umeå University. The original source of data includes e.g. the Swedish employment register (RAMS) which provides information about employment, as well as a longitude income register LISA that can link family members and contains information about demographic and socioeconomic factors.

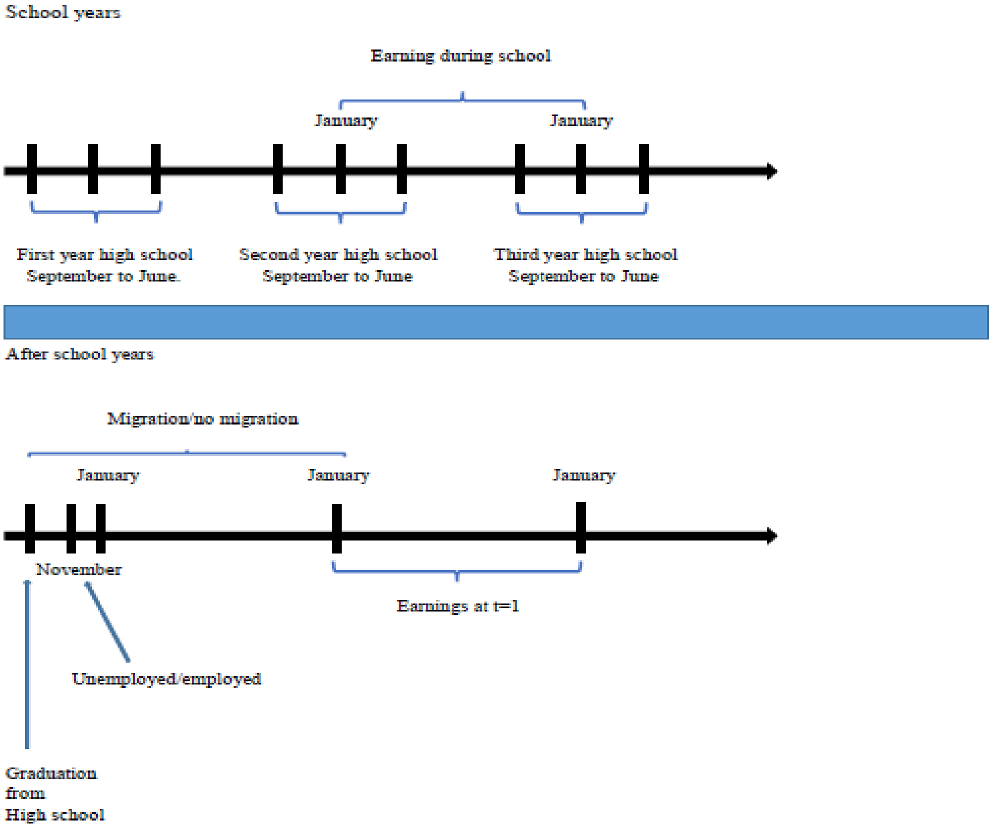
The data used in this thesis covers five cohorts of individuals registered in Sweden who did not enter university after graduating school. The reason for not including persons with a university degree is that these persons would have been perceived as not employed after finishing upper secondary school by the employment register. However, as a university degree results generally in higher income, the inclusion of university students would bias the estimation and work completely against the theory of scarring effects. The individuals born in the five years from 1979 to 1983 are represented in the five different cohorts of the analysis. The timing when measuring the variables is important, and can be hard to follow, therefore see figure 1.

The dependent variable is the future income effects of migration, annual earnings work as the best operationalization. Hence, the variable *earnings* is measured in order to draw conclusions about the dependent variable. Annual earnings[1] from successive years after the migration are measured. The point of time when measuring the yearly earnings will vary for the different cohorts. To be able to easily follow the results, t=1 is the notation for the first yearly earnings measured for all cohorts, t=1 is often referred to the first *follow-up year* and t=2 the second etc. All control variables are measured before the act of migration and the last follow-up year is 2012. For an individual finishing secondary school in beginning of June, the

[1] Earnings are calculated combining wages from work and income from self-employment.

factor employment after graduation is measured in November of the same year. In order to be coded as employed an individual need to be in work for at least four hours in November. Individuals who count as employed at this point of time were removed from the dataset before running the estimations that test income effects from migration.

Figure 1: **Time axis of the variables' measurements**



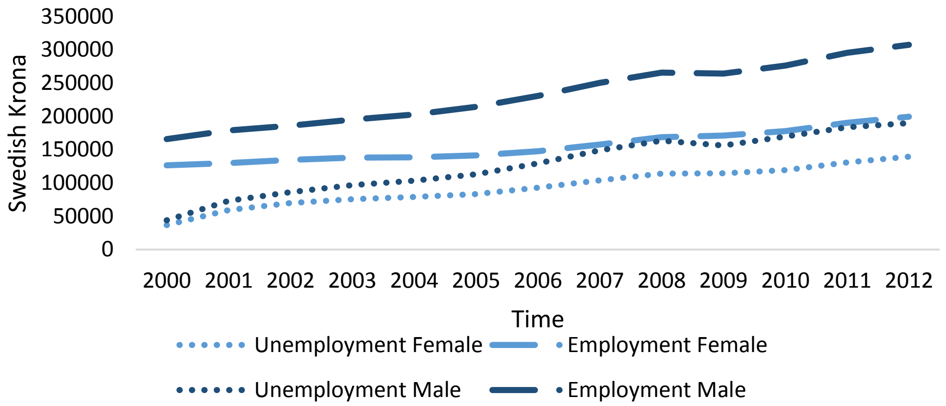
To highlight the prospect that scarring effects exist in my data, sample means of earnings are presented in figure 2. The graphs indicate that individuals who are unemployed receives lower income in the future, this is a trend that increase over time in real terms. To further prove that scarring effects exist, matched observations are presented in Appendix 1, the positive estimates for employed individuals, additionally proves that scarring effects exist in my data.

Due to reasons of operationalization, Sweden was divided into 100 different labor market regions (LMR)[2]. A LMR can be a municipality or different municipality put together. If a municipality has a lot of commuting to another municipality, they count as the same

[2] The division in to LMR changes every year since commuting changes. I will use the division of 1998 because it is around the time when the individuals in my data finished upper secondary school.

LMR[3]. The independent variable *internal migration* is coded as a dummy: when a person moves from one labor market to another, it counts as a migration. The reason for choosing this definition is the assumptions that migration decisions are more likely to be due to job causes.

Figure 2: Earning trajectories 2000-2012, by employment and gender



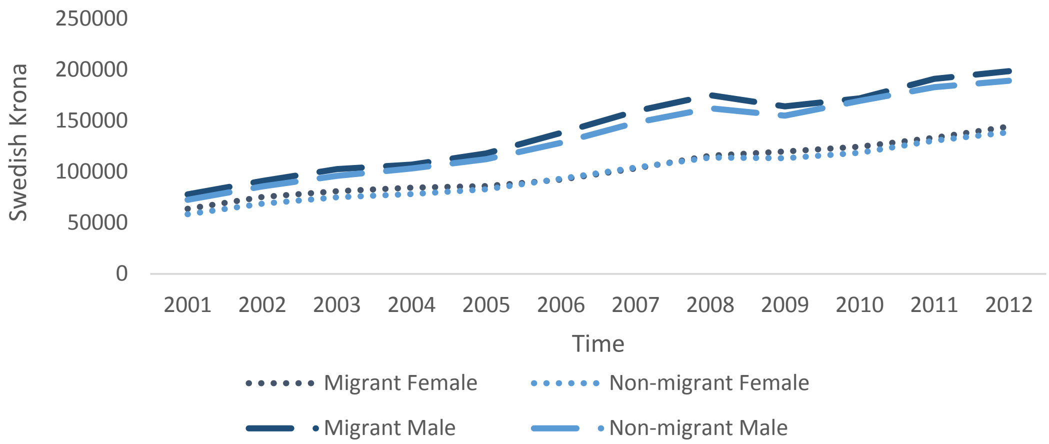
The migration dummy indicates migration if there is a change in the official registration of place of residence in the time between graduation and the end of the year after the graduation year. There are two minor shortcomings with this measurement: first, people who change the location of living not always officially change their registration of living. Second, changes in official registration are counted on a yearly bases meaning that individuals changing their place of residence during the final semester in school would also count as migrants. I argue that these are minor problems because most people who change location out of work reasons usually also change their official registration. Moreover, individuals in their final semester of upper secondary school do not have a lot of options changing school which is probably needed when moving between LMR because of long distances. Unconditional means of earnings for migrants and non-migrants are given in Figure 3 to show if there is any pattern in the data for migrants and non-migrants. The graphs show that internal migrants earn slightly more in the follow-up years after migration, this is more clearly for men.

The importance of the timing when measuring creates some complications. Individuals taking more than three years finishing their education would in general not move as often during the time measuring migration because of still ongoing education. If these individuals also have characteristics that make them earn less in the future, the inclusion will lead to

[3] An example can be the multiplicities close to Stockholm. They belong to Stockholm's LMR because a lot of persons commuting from these multiplicities to Stockholm.

biased results. In order to handle this problem, the individuals taking longer than three years were removed. Observations not studying at upper secondary school or studying a two year of upper secondary school are still included, but to control for possible effects of these individuals, a dummy variable was created. Moreover, persons who undergo military services are removed since the data only show if a person has done their military service, but not when in time they apply or finish. The group which does their military training is rather small and should not affect the results significantly.

Figure 3: Earnings trajectories 2001-2012, by migration and gender



After the cleaning, the dataset for the empirical analysis includes 16’951 observations for the cohort born in 1979, the differences amongst the amount of observations between the cohorts is rather small and should not affect the comparisons significantly. When subdividing by gender 9006 female and 7945 male observations exist.

The use of matching requires that individual-variables are measured before the migration. These include *gender, county of birth, marriage status, and numbers of children.* Since there is no information on grades available in order to control for ability, background information about the parents of the observed individuals are included. These background variables contain information about *disposable income* and *education.* Culture differences can further be captured by *county of birth* of the parents. Universally, graduates from upper secondary school have little working experience in Sweden. However, I use information of earnings during January in the second school year to January in the third school year, in order to create a variable that is supposed to capture both unobserved characteristics (e.g. high motivation) and effects of work-experience.

Aggregated covariates include a variable trying to capture the labor market condition for young adults. It is calculated for all LMRs and includes the percentage of employed individuals between 20-34 years old. A categorical variable containing the size of the population in each LMR is also included (big, medium, small). Some of the categorical variables (father/mother education and all birthplace variables) were first divided in to six and five categories respectively. However, some of the categories consisted of too few observations and were therefore combined into three categories. The extended definitions and units of the variables are found in the appendix (Appendix 2).

In the following, I focus on one cohort when presenting the results of my analysis. The reason for this is that including all cohorts would take up to much space. The cohort I will focus on is the one with individuals born in the year 1979 because this cohort has the longest follow-up time and thus most observations. The results presented are exclusively from this cohort, unless it is stated differently. Because I will compare the results between this cohort and the other in order to check for robustness and to find out about possible differences.

5. Method

With help of the rich dataset just explained, I test the hypothesis in the following sections. In order to do so, I will use a nearest-neighbor propensity score matching method. One of the best methods to estimate casual effects is the use of randomized experiments. However, these experiments are empirically not realizable to create and matching tries to replicate the experiment using observed covariates with non-randomized data. The parameter of interest is the average treatment effect on the treatment (ATT), i.e. the average earnings effect from migration for migrants. This can be expressed as: $ATT= E(Y_1|D=1)-E(Y_0|D=1)$, where D equals one if moving and vice versa for D equals zero. The problem is that an individual is just in one of the two possible states. The states are the outcome for the individual *i* under treatment (moving from one labor market to another), $Y_i(1)$, and the outcome if the individual *i* does not receive treatment, $Y_i(0)$. Meaning that $E(Y_0|D=1)$, i.e. what would have happened to the treated individuals had they not moved, which actually can never be observed. The outcomes of non-movers usually serve as an estimate of $E(Y_0|D=1)$.

To estimate ATT, I first match individuals on their propensity score. The propensity score is estimated by a logit model which can be defined as: $p(x) = prob(D=1|x)$, i.e. the conditional probability receiving treatment given pretreatment characteristics. When $p(x)$ is calculated, I will use a nearest-neighbor procedure to match the treated individuals (p_i) with individuals from the control group (p_j) with similar propensity score, (i.e. $\min | p_i-p_j|$).

The propensity score matching relies on the conditional independence assumption (CIA), where outcomes are independent of probability treatment, conditional on x: $(Y_1\, Y_0 \perp D|x)$. That means that the migration and non-migration groups are different only because of differences in observed characteristics. If unobserved characteristics influence both the selection and the outcome, the results will be biased. This is a strong assumption and it is untestable. This means that it is very important to have access to high informative data and choose variables wisely. Therefore, all variables known to be related with the outcome and the decision to move should be included. However, the cost including variables that is unassociated with the treatment assignment is low, because they will have little influence in the propensity score model Stuart (2010). Including variables which do not affect outcome can increase variance somewhat, but excluding an important variable can increase possible biases significantly Stuart (2010). Consequently, I will be generous when deciding which variables to include.

Another important assumption is the common support condition. This condition ensures that there are overlaps in the characteristics of untreated and treated observations, to find an adequate counterpart for every individual in the treatment group: (0 < prob(D=1|x) < 1). The evaluation discussion whether this assumption is satisfied can be found in the results section below. Other assumptions for applying matching are 1) that there should be no "spill-over" effect from the treatment. This implies that if some treatment generates higher income, this higher income should not in turn result in a higher income for individuals who did not receive a treatment. 2) Treatments of one individual cannot prevent another individual from getting the treatment. There is no reason to believe that these assumptions are not fulfilled for my analysis.

I will estimate subgroups separately in order to reduce heterogeneity within the sample as explained by Stuart (2010). When estimating ATT, I assume the different subsamples to differ regarding gender and birth-year-cohort. I will use matching with replacement which allows observations that received the treatment to match with the best fitting observation which did not receive the treatment. This procedure can potentially reduce biased estimations. In addition, the propensity score distribution is trimmed by 5%. In other words, 5% of the observations in the tails are removed. The rationale behind this is to exclude extreme values which can produce bad estimates and better control for the common support assumption. There is a tradeoff between efficiency and bias when choosing number of nearest-neighbors. Selecting only one nearest-neighbor will minimize bias since using the most similar observation. However, this will ignore a lot of information and will therefore be less efficient.

For me it is of most importance to minimize bias, since I rather would like to have unbiased results that is insignificant than the other way around, hence I will use one nearest-neighbor throughout.

The advantage of using matching over for example an ordinary least squares estimation (OLS) is that matching is not as sensitive to the functional form of the covariates. Another reason is that if you have observations in the control group which are not relevant, i.e. observations that would never receive treatment. These observations are ignored in matching whilst they are not in an OLS regression, because in OLS all the treatment and control observations are given the same weight.

6. Results

Firstly, the logit models are presented and the results are shortly interpreted. Secondly, the balance test is displayed in order to examine the matching quality. This is presented together with the interpretation on how satisfactorily the common support assumption is fulfilled. The second part of the method which includes the ATT estimates is shown afterwards. Finally, I present robustness tests, where certain specifications in the matching algorithm are changed.

The logit model estimates are presented in Appendix 3, some interesting differences between genders can be found here. Being married increases the likelihood for females to migrate, while the opposite holds for women with children. The sign in front of the estimates for males are completely opposite. However, the estimates for males are not significant at 5% level.

To inspect the matching quality, the balance test is provided in Appendix 4. For good matches the observed characteristics should be similar. After the matching, the p-values show no significant differences in their means. Hence, the matching has generated two groups which seemingly are comparable. As suspected when comparing the treatment group with the control group before the matching, the fact is highlighted that individuals migrate from LMRs with a low population density to LMRs with high density.

To be able to practice the matching method successively some underlying assumptions need to be satisfied. The common support assumption is of great importance, to evaluate visually how well it is fulfilled, the distribution of the estimated propensity score for both treated and the control group are showed in the appendix (Appendix 5). The distributions for the control and treatment group differ, however the distribution for the control group covers the range of the distribution for the treatment group for both genders. Furthermore, the maximum and minimum values for the treatment group are within the maximum and minimum values for the control group, hence the common support assumption should be fulfilled especially when trimming is included.

In the thesis only one of the twelve follow-up years after the migration decision is shown for the propensity score distribution, the balance test, and the logit model. One reason that results can vary over different years is that persons can move in and out from Sweden. The amount of observations for the dependent variable will therefore change over time. If there is only a certain kind of individuals moving in and out from Sweden this could lead to bias in the estimates. But as presented in Table 1 the differences in the quantity of observations between years are rather small and should not affect the results significantly. Furthermore, the results should not differ considerably between follow-up years because it touches almost the same individuals. When looking at different follow-up years, there is evidence supporting this e.g. only one of the variables, for one year and gender is not significantly similar after matching at 10% level.

Table 1: Estimates of the average treatment effect on the treated ATT, by gender

Time	Females		Males	
	ATT (S.E.)	$N_{treatment}$ ($N_{control}$)	ATT (S.E.)	$N_{treatment}$ ($N_{control}$)
t=1	**8088.590** (3369.713)	**937** (5,544)	**10672.271** (4820.782)	**635** (4,978)
t=2	**9577.865** (3753.388)	**936** (5,494)	**5616.447** (5457.875)	**639** (4,974)
t=3	**5228.992** (4133.567)	**929** (5,443)	**5017.792** (6110.720)	**638** (4,946)
t=4	**6490.692** (4392.308)	**927** (5,405)	**4349.668** (6508.746)	**635** (4,920)
t=5	**2369.659** (4569.304)	**926** (5,366)	**5521.926** (7006.596)	**633** (4,913)
t=6	**-2094.634** (4966.708)	**922** (5,329)	**15030.166** (7231.487)	**631** (4,895)
t=7	**-7343.935** (5164.180)	**920** (5,278)	**13189.899** (7726.720)	**625** (4,875)
t=8	**4156.636** (5474.724)	**920** (5,259)	**6318.959** (8221.942)	**627** (4,864)
t=9	**5393.135** (5720.908)	**919** (5,249)	**12743.456** (8634.530)	**627** (4,854)
t=10	**7280.963** (5942.380)	**917** (5,252)	**-500** (9237.772)	**625** (4,843)
t=11	**-99.885** (5966.995)	**916** (5,234)	**15564.418** (9560.216)	**624** (4,830)
t=12	**8398.160** (6371.958)	**915** (5,238)	**7761.460** (9914.563)	**619** (4,827)

Standard Errors in Parentheses, N= number of observations.

Note: The sample is the 1979 cohort and estimation were carried out using Stata 14 and psmatch2.

The average treatment effect on the treatment estimates are presented in Table 1. The results show positive effects from migration except for three of the follow-up years for females and one year for males. Though, a high proportion of the estimates are not significant at the 5%.

To investigate the strength of the estimates in order to make the right conclusions the estimates from Table 1 are compared with the estimates of the other cohorts for three different follow-up years (one cohort only two years), these are displayed in Table 2. The estimates are rather contradictory to my theoretical assumptions as around half of the coefficients show into the wrong direction (have negative values) and neither are the positive estimates significant. There might be two possible reasons explaining that the estimates vary for the different cohorts. Either it is the difference of the labor markets after the migration which create these variances in the estimation; or the fact that the estimates are not significant is partly random as some cohorts have positive estimates and some negative.

Table 2: Estimates of the average treatment effect on the treated ATT, by gender

	Females	Males
	ATT (S.E.)	**ATT (S.E.)**
t=1		
Birth year 1980	**3466.863**	**-49.099**
	(3973.398)	(5600.973)
Birth year 1981	**898.410**	**-3991.881**
	(3774.384)	(5226.953)
Birth year 1982	**-4850.201**	**-1921.230**
	(3566.542)	(4932.476)
Birth year 1983	**3703.205**	**3705.743**
	(3445.839)	(4847.444)
t=4		
Birth year 1980	**4338.796**	**-72.335**
	(5104.030)	(7458.172)
Birth year 1981	**-6598.107**	**-5139.121**
	(5106.939)	(7154.476)
Birth year 1982	**-2065.659**	**3087.701**
	(5165.285)	(7495.093)
Birth year 1983	**5446.023**	**-991.237**
	(5428.456)	(7366.900)
t=9		
Birth year 1980	**-6381.126**	**-1215.258**
	(7093.002)	(10695.473)
Birth year 1981	**-6121.238**	**-10931.578**
	(6691.679)	(10010.008)
Birth year 1982	**10847.018**	**-958.811**
	(6609.278)	(10607.897)

Standard errors are in parentheses.

Note: The samples are the 1980-1983 cohorts and estimation were carried out using Stata 14 and psmatch2.

Unemployment in Sweden under the age of 25 for the years after migration is displayed in Appendix 6. It shows a trend that unemployment increased over this time interval (even if it is a break in the time series between 2000 and 2001). A valid argument that can be applied is that the first cohort earns more from the migration because there labor market cycle timing was better due to lower aggregated unemployment Couch and Placzek (2010). However, even the cohort of individuals born in 1979 has only partly positive estimates which are significant for both genders. Overall, there are too few significant results on income from migration to state a likely relationship between them. Hence, according to the analysis of this dataset, migration barley increases earnings in the future.

Changes in the variables were made in order to try seeing if there were some problems with the included variables. Insignificant covariates from the logit model were dropped, variables were combined (e.g. mothers' and fathers' variables to parents' variables) and some continuous variables squared. Different combinations of variables were tested showing no increase in significance. Alternative attempts with a probit model instead of a logit model did not change the significances. A final test including all cohorts with cohort dummies to increase the quantity of observations in the treatment group were examined, also not showing any improvements, all these estimations are available from the author on request.

One possible reason for the bias in the estimates might be that the conditional independent assumptions is not fulfilled, i.e. there are unobserved variables that influence both the migration decision and the outcome. Difference-in-difference methods are commonly used to take advantage of the longitudinal data and increase the chance of satisfying CIA. Diff-in-diff captures unobserved time-invariant heterogeneity for the observations. The reasons for not using this method is that a lot of the individuals have zero income the first follow-up years after migration and the persons are young with volatile income. More discussions on the problem with estimating effects using young individuals follow in the conclusion section below.

Some robustness tests were made to guarantee stable estimates. These tests are shown in Appendix 7. Changing the trimming level or chancing the number of nearest-neighbor leads to relative large changes in some cases. Therefore, the estimates are rather unstable.

7. Conclusion

The main aim of this thesis was to investigate if there is a positive effect of migration on future income given youth unemployment. Therefore, I first reviewed the important literature on migration. I then present literature on scarring effects to highlight the negative effect from unemployment on future earnings. This paper concludes that scarring effects are likely to exist in my data and I argue that migration can mitigate these effects. I lastly tested this hypothesis with the help of a dataset from the ASTRID database by using a nearest-neighbor propensity score matching method.

The empirical analysis however has shown there is hardly any evidence for a significant positive effect from migration in the data. This means that my findings do not show that migration works to mitigate scarring effects. Changing variables in the models does not increase the results and significances. The robustness test indicates unstable estimates, but stable in the sense that they are insignificant. With more time, I would have tried another method e.g. some fixed effect OLS regression to see if the results may change.

Like Jacobsen and Levin (1997), as well as Tano (2014), I cannot find positive effects from migration for low-educated individuals. Eliasson et al. (2007) neither find evidence that low-educated individuals gain higher income in the future from migration using Swedish data. These findings indicate a no effect on low-educated and can be a cause why I cannot discover any effect from migration in my data either. Migration may however still work for young high-educated individuals. This is actually more likely since their investment in human capital from migration should be higher, due to the fact that they are more attractive on the labor market. Hence, I recommend a further going study on this group of individuals regarding migration and scarring effects, but understand the difficulties to find enough observations of individuals who are unemployed in this group.

The insignificant results can also be due to the problem of estimating rather young individuals. Young individuals tend to have high volatility in income and most of them have not made the lifecycle decisions yet (e.g. getting married or having a child). For further research this is a major obstacle to overcome. If my model is correct and there is in fact no positive effect from migration for young unemployed individuals, a valid reason for this may be that the highest paid jobs are in the most costly areas to live. This can potentially force migrates to choose lower paid jobs. Another reason for not finding positive effects for migration in Sweden can be that there is only a small investment in human capital from migration because earnings differ relative little in different labor market due to of less individual wage negotiations compared to other countries.

Although the empirical analysis does not support my theoretical argument, I argue that this paper contributes to the research on labor market economics in three ways: 1) the fact that I focuse exclusively on young unemployed individuals; 2) the innovation of connecting scarring effects and migration; and 3) the presentation of empirical results showing that migration barley leads to an increase in income according to the observations in my data. If the results are biased, I pointed out possible reasons and problems to overcome for further studies on the topic.

References

Arulampalam, Wiji. 2001. Is unemployment really scarring? Effects of unemployment experiences on wages. *The Economic Journal 111* (475): 585-606.

Arulampalam, Wiji, Booth, Alison and Taylor, Mark. 2000. Unemployment persistence. *Oxford Economic Papers* 52 (1): 24-50.

Bartel, Ann. 1979. The migration decision: what role does job mobility play? *The American Economic Review* 69 (5): 775- 786.

Becker, Gary. 1964. *Human Capital*. New York: Columbia University.

Björklund, Anders, Edin, Per-Anders, Holmlund, Bertil and Wadensjö, Eskil. 2006. *Arbetsmarknaden*. 3th ed. Stockholm: SNS förlag.

Boman, Anders. 2011. Does migration pay? Earnings effects of geographic mobility following job displacement. *Journal of Population Economics* 24 (4): 1369-1384.

Burgess, Simon, Propper, Carol, Rees, Hedley and Shearer, Arran. 2003. The class of 1981: the effects of early career unemployment on subsequent unemployment experiences. *Labour Economics* 10 (3): 291-309.

Couch, Kenneth and Placzek, Dana. 2010. Earnings losses of displaced workers revisited. *The American Economic Review* 100 (1): 572-589.

DaVanzo, Julie. 1978. Does Unemployment Affect Migration? Evidence from Micro Data. *The Review of Economics and Statistics* 60 (4): 504-514.

ec.europa. 2016. *Unemployment statistics*. http://ec.europa.eu/eurostat/ statistics-explained/index.php/Unemployment_statistics (Accessed 2016-03.11).

Eliasson, Kent Westerlund, Olle and Åström, Johanna (2007). Flyttning och pendling i Sverige. *Statens Offentliga Utredningar* 2007 (35).

Ellwood, David. 1982. Teenage unemployment: Permanent scars or temporary blemishes? *The youth labor market problem: Its nature, causes, and consequences.* Chicago, University of Chicago Press, 349-390.

Fields, Gary. 1976. Labor forces migration, unemployment and job turnover. *Review of Economics and Statistics* 58 (4): 407-415.

Gartell, Marie. 2009. Unemployment and subsequent earnings for Swedish college graduates: A study of scarring effects. working Paper 2009:10, IFAU.

Gordon, Ian. 1985. The cyclical interaction between regional migration, employment and unemployment: A time series analysis for Scotland. *Scottish Journal of Political Economy* 32 (2): 135-158.

Grant, Kenneth and Vanderkamp, John. 1980. The effect of migration on income: a micro study with Canadian data 1965-1971. *The Canadian Journal of Economics* 13 (3): 381-406.

Greenwood, Michael. 1997. Internal Migration in Developed Countries. In Mark R. Rosenweig and Oded Stark (eds.), *Handbook of Population and Family Economics Vol 1B.* Amsterdam: Elsevier, 647–711.

Gregg, Paul. 2001. The Impact of Youth Unemployment on Adult Unemployment in the NCDS. *The Economic Journal* 111 (475): 626-653.

Gregg, Paul and Tominey, Emma. 2005. The wage scar from male youth unemployment. *Labour Economics* 12 (4): 487-509.

Gregory, Mary and Jukes, Robert. 2001. Unemployment and Subsequent Earnings: Estimating Scarring Among British Men 1984–94. *The Economic Journal* 111 (475): 607-625.

Heckman, James and Borjas, George. 1980. Does unemployment cause future unemployment? Definitions, questions and answers from a continuous time model of heterogeneity and state dependence. *Economica* 47 (187): 247-283.

Hunt, Janet and Kau, James. Migration and wage growth: A human capital approach. *Southern Economic Journal* 51 (3): 697-710.

Hämäläinen, Kari. 2003. Education and Unemployment: State dependence in Unemployment among Young People in the 1990s. VATT-discussion papers 312.

Jacobsen, Joyce and Levin, Laurence. 1997. Marriage and Migration: Comparing Gains and Losses from Migration for Couples and Singles. *Social Science Quarterly* 78 (3): 688-709.

Korpi, Martin, Clark, William and Malmberg, Bo. 2010. The urban hierarchy and domestic migration: the interaction of internal migration, disposable income and the cost of living, Sweden 1993–2002. *Journal of Economic Geographic* 11 (6): 1051-1077.

Lockwood, Ben. Information externalities in the labour market and the duration of unemployment. *The Review of Economic Studies* 58 (4): 733-753.

Mroz, Thomas and Savage, Timothy. 2006. The Long-Term Effects of Youth Unemployment. *The Journal of Human Resources* 41 (2): 259-293.

Nakosteen, Robert and Westerlund, Olle. 2004. The effects of regional migration on gross income of labour in Sweden. *Papers in Regional Science* 83 (3): 581-595.

Nakosteen, Robert and Zimmer, Michael. 1980. Migration and income: the question of self-selection. *Southern Economic Journal* 46 (3): 840-851.

Nilsen, Oivind and Reiso, Katrine Holm. 2001. Scarring Effects of Unemployment. NHH Dept. of Economics Discussion Paper No. 26/2011.

Nordström Skans, Oskar. 2004. Scarring effects of the first labour market experience: A sibling based analysis. working Paper 2004:14, IFAU.

Pissarides, Christopher and Wadsworth, Jonathan. 1989. Unemployment and the Inter-Regional Mobility of Labour. *The Economic Journal* 99 (397): 739-755.

Robinson, Chris and Tomes, Nigel. 1982. Self-Selection and Interprovincial Migration in Canada. *The Canadian Journal of Economics* 15 (3): 474-502.

Sjaastad, Larry. 1962. The Costs and Returns of Human Migration. *Journal of Political Economy* 70 (5): 80-93.

Statistics Sweden (SCB). 2016. *Arbetslösa 15-74 år (AKU) efter arbetslöshetstidens längd, kön och ålder. Månad 2005M04 - 2016M04.* http://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__AM__AM0401__AM0401L/NAKUArblosaTM/?rxid=a15fc725-85a5-44dc-93c9-862d18b4706a (Accessed 2016-05-29).

Stuart. Elizaneth. 2010. Matching methods for causal inference: a review a look forward. *Statistical Science* 25 (1) 1-21

Tano, Sofia. 2014. Internal Migration of Young Adults: Heterogeneity in Effects on Labour Income by School Grades. *Umeå economic studies* 880.

Appendix

Appendix 1: Estimates of the average treatment effect on the treated ATT, by gender

Time	Females	Males
	ATT (S.E.)	ATT (S.E.)
t=1	52562	83428
	(1710)	(2205)
t=2	47789	75652
	(1872)	(2490)
t=3	43379	70639
	(2030)	(2707)
t=4	37284	68557
	(2236)	(2913)
t=5	34580	69269
	(2326)	(3028)
t=6	32645	66380
	(2484)	(3191)
t=7	31378	69855
	(2646)	(3425)
t=8	30312	64691
	(2801)	(3610)
t=9	32522	71128
	(2871)	(3816)
t=10	36095	68924
	(2940)	(12243)
t=11	32385	67652
	(3045)	(4351)
t=12	35280	75203
	(3192)	(4345)

Standard errors are in parentheses.

Note: The treatment group is individuals that is employed and the control group individuals that is unemployed. The variables included except the dummy for employment status are all control variables see Appendix 2. The estimation were carried out using Stata 14 and psmatch2.

Appendix 2: Definition of variables

Variables	Descriptions
Dependent variable	
Income	Annual earnings (SEK)
Independent variable	
Migration	Dummy variable=1 if internal migration from one LMR to another, during the graduation year or the year after.
Control variables	
Married	Dummy variable=1 if married, 0 otherwise.
Children	Number of children under six year.
3-years upper secondary school	Dummy variable=1 if individual finished a three year long upper secondary school education, 0 otherwise (two year upper secondary school or not finishing upper secondary school)
In-school experience	Dummy variable=1 if income during the year before graduation, 0 otherwise.
County of Birth:	
Sweden	Dummy variable=1 if born in Sweden.
Europe	Dummy variable=1 if born in Europe (not Sweden) or USA, Canada, Japan, Australia, New Zeeland.
Outside Europe	Dummy variable=1 if not born in Europe, USA, Canada, Japan, Australia, New Zeeland.
Farther country of birth:	
Sweden	Dummy variable=1 if father is born in Sweden.
Europe	Dummy variable=1 if father is born in Europe (not Sweden) or USA, Canada, Japan, Australia, New Zeeland.
Outside Europe	Dummy variable=1 if father is not born in Europe, USA, Canada, Japan, Australia, New Zeeland.
Mother country of birth:	
Sweden	Dummy variable=1 if mother is born in Sweden.
Europe	Dummy variable=1 if mother is born in Europe (not Sweden) or USA, Canada, Japan, Australia, New Zeeland.
Outside Europe	Dummy variable=1 if mother is not born in Europe, USA, Canada, Japan, Australia, New Zeeland.
Income father	Father´s average disposal income between 1994-2002 (100*SEK)
Income mother	Mother´s average disposal income between 1994-2002 (100*SEK)
Education father:	
Elementary school	Dummy variable=1 if father´s highest education 2002 was elementary school.
Upper secondary school	Dummy variable=1 if father´s highest education 2002 was upper secondary school.
University	Dummy variable=1 if father´s highest education 2002 was university degree or doctoral degree.
Education mother:	
Elementary school	Dummy variable=1 if mother´s highest education 2002 was elementary school.
Upper secondary school	Dummy variable=1 if mother´s highest education 2002 was upper secondary school.
University	Dummy variable=1 if mother´s highest education 2002 was university degree or doctoral degree.
Size of population LMR:	
Small	Dummy variable=1 if LMR population lower than 20000, in age group 20-34.
Medium	Dummy variable=1 if LMR population higher than 20000 in age group 20-34. (20 regions)
Big	Dummy variable=1 if LMR population is double as big as the next biggest LMR population in age group 20-34 (3 regions)
% Employed/LMR	Percent employed individuals in age cohort 20-34 for each LMR.

Appendix 3: Logit model propensity score estimations by gender.

Dependent variable: Migration		
Independent variables:	**Females**	**Males**
Married	.798**	-.275
Children	-.437**	.211
3-years upper secondary school	-.143*	.026
In-school experience	.153*	.061
Country of birth:		
Sweden	.061	-1.107*
Europa[1]	.110	-.684
Outside Europa		
Farther country of birth:		
Sweden	-.113	-.100
Europa[1]	.139	
Outside Europa		-.292
Mother country of birth:		
Sweden		.869*
Europa[1]	-.050	.609
Outside Europa	-.217	
Income father	3.37e-05	1.5e-06
Income mother	5.58e-07	4.9e-06
Education father:		
Elementary school		-.268*
Upper secondary school	.120	-.192
University	.160	
Education mother:		
Elementary school	-.138	-.395**
Upper secondary school	-.000	-.211*
University		
Size of population LMR:		
Small	1.068**	.740**
Medium	.647**	.375**
Big		
% Employed/LMR	-1.270	1.50
Constant	-1.412	-2.669**

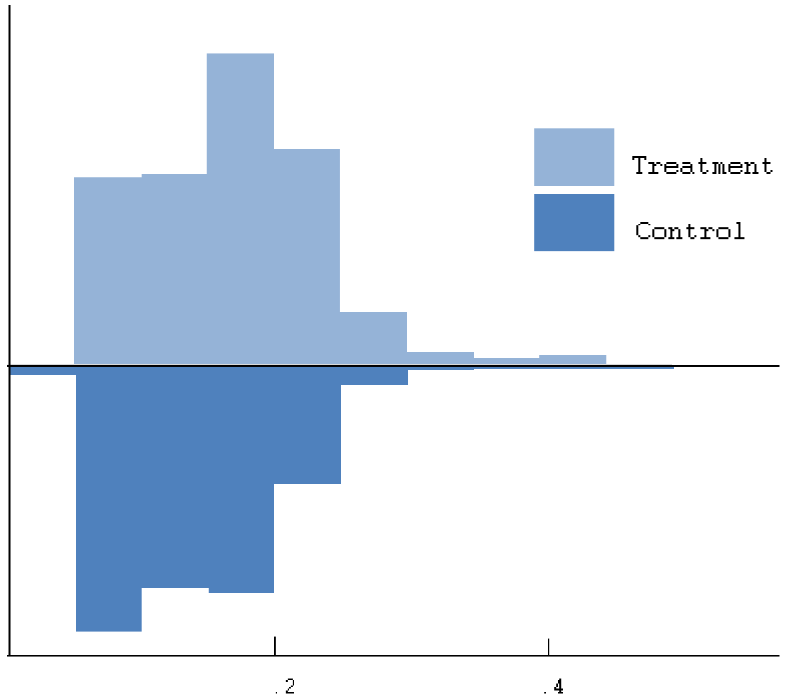
Note: ** significance at the 1% level. * significance at the 5% level. measured at t=1.

Appendix 4: Avreges for treatment and control groups, by gender, before and after matching

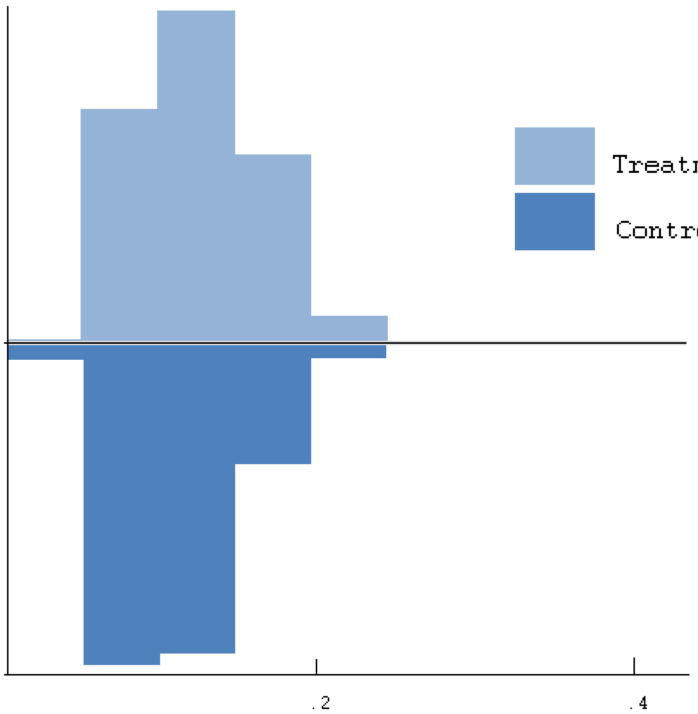
Variable	Females	Treatment	Control	t	p>|t|	Males Treatment	Control	t	p>|t|
Married	Unmatched	.062	.039	3.25	0.001	.004	.006	-0.57	0.571
	Matched	.035	.029	0.80	0.423	.004	.004	-0.00	1.000
Children	Unmatched	.112	.151	2.89	0.004	.031	.027	0.52	0.605
	Matched	.116	.102	0.93	0.351	.029	.019	1.01	0.313
3-years upper secondary school	Unmatched	.550	.560	-0.57	0.572	.453	.432	0.99	0.323
	Matched	.557	.571	-0.57	0.567	.450	.442	0.29	0.773
In-school experience	Unmatched	.543	.507	2.03	0.042	.491	.468	1.09	0.277
	Matched	.538	.556	0.76	0.447	.486	.476	0.35	0.730
Country of birth:									
Sweden	Unmatched	.907	.904	0.27	0.791	.877	.878	-0.11	0.913
	Matched	.914	.931	1.33	0.182	.879	.880	-0.09	0.930
Europa[1]	Unmatched	.052	.041	1.51	0.132	.064	.055	0.96	0.337
	Matched	.044	.039	0.59	0.556	.064	.054	0.73	0.466
Outside Europa	Unmatched	.040	.054	-1.73	0.084	.058	.066	-0.75	0.452
	Matched	.040	.029	1.29	0.196	.056	.064	-0.60	0.546
Farther country of birth:									
Sweden	Unmatched	.782	.782	-0.00	1.000	.765	.735	1.63	0.104
	Matched	.804	.797	0.36	0.722	.766	.759	0.27	0.787
Europa[1]	Unmatched	.163	.145	1.41	0.158	.165	.174	-0.58	0.564
	Matched	.142	.153	0.67	0.505	.167	.165	0.08	0.938
Outside Europa	Unmatched	.054	.072	-1.97	0.049	.069	.090	-1.75	0.080
	Matched	.052	.048	0.43	0.665	.066	.074	-0.56	0.574
Mother country of birth:									
Sweden	Unmatched	.797	.783	0.96	0.339	.792	.747	2.45	0.015
	Matched	.809	.818	0.49	0.627	.791	.796	-0.21	0.831
Europa[1]	Unmatched	.163	.145	1.41	0.158	.146	.169	-1.47	0.142
	Matched	.142	.153	0.67	0.505	.150	.144	0.32	0.746
Outside Europa	Unmatched	.048	.066	-2.15	0.032	.061	.082	-1.86	0.062
	Matched	.047	.043	0.34	0.733	.057	.059	-0.12	0.903
Income father	Unmatched	1534	1587	-1.22	0.221	1555	1542	0.30	0.766
	Matched	1548	1570	0.48	0.634	1547	1574	-0.44	0.659
Income mother	Unmatched	1316	1337	-0.40	0.686	1286	1289	-0.11	0.916
	Matched	1319	1280	1.56	0.119	1276	1281	-0.16	0.876
Education father:									
Elementary school	Unmatched	.346	.376	-1.71	0.087	.322	.362	-1.98	0.048
	Matched	.356	.355	0.05	0.961	.339	.332	0.24	0.808
Upper secondary school	Unmatched	.493	.464	1.61	0.107	.480	.476	0.17	0.864
	Matched	.481	.470	0.47	0.635	.476	.504	-0.98	0.328
University	Unmatched	.160	.159	0.06	0.950	.196	.160	2.33	0.020
	Matched	.161	.173	0.70	0.486	.183	.162	0.99	0.323
Education mother:									
Elementary school	Unmatched	.284	.314	-1.83	0.067	.270	.325	-2.79	0.005
	Matched	.281	.270	0.53	0.596	.284	.286	-0.06	0.949
Upper secondary school	Unmatched	.549	.520	1.68	0.093	.503	.492	0.52	0.774
	Matched	.548	.560	0.48	0.634	.509	.501	0.29	0.774
University	Unmatched	.165	.165	0.03	0.977	.225	.181	2.67	0.008
	Matched	.169	.169	-0.00	1.000	.205	.211	-0.28	0.777
Size of population LMR:									
Small	Unmatched	.352	.222	8.62	0.000	.351	.235	6.40	0.000
	Matched	.329	.341	-0.50	0.616	.319	.339	-0.73	0.463
Medium	Unmatched	.414	.381	1.88	0.059	.385	.384	0.07	0.940
	Matched	.424	.400	1.01	0.313	.403	.374	1.06	0.289
Big	Unmatched	.233	.395	-9.55	0.000	.262	.380	-5.81	0.000
	Matched	.245	.258	-0.60	0.549	.276	.286	-0.38	0.701
% Employed/LMR	Unmatched	.692	.690	1.23	0.220	.693	.689	2.60	0.009
	Matched	.691	.694	-1.48	0.138	.691	.690	0.26	0.794
Number of observations		937	5,544			635	4,978		

Note: Messured at t=1.

Appendix 5: Propensity scores for treatment and control group, by gender.

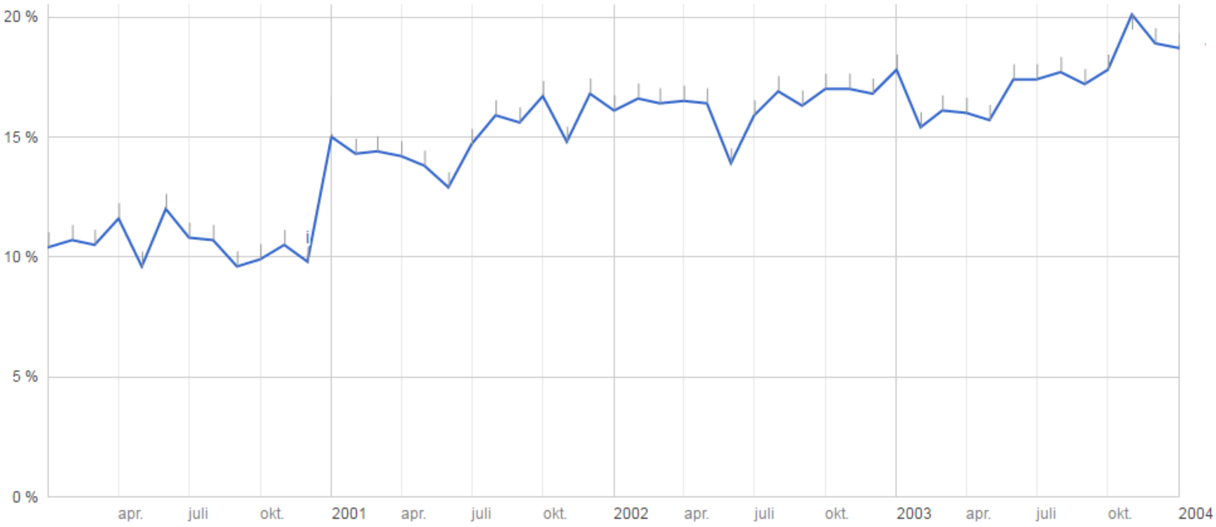


Note: Females, measured at t=1.



Note: Males, measured at t=1.

Appendix 6: Unemployment rates 2000-2004 for individual under 25.



Source: Eurostat

Appendix 7: Estimates of the average treatment effect on the treated ATT with different specifications, by gender.

	Females	Males
	ATT (S.E.)	ATT (S.E.)
t=1		
Trim 0%	7716.703	10799.773
	(3290.116)	(4690.919)
Trim 10%	8379.877	12184.080
	(3450.747)	(4908.938)
Matching 1:2	6928.786	7176.549
	(2956.153)	(4271.801)
Matching 1:4	8402.715	6625.801
	(2739.595)	(3915.842)
t=4		
Trim 0%	5700.431	1262.762
	(4303.531)	(6689.499)
Trim 10%	4878.562	1327.874
	(4508.517)	(6378.517)
Matching 1:2	6935.641	4508.278
	(3816.861)	(5622.673)
Matching 1:4	7565.777	4828.145
	(3522.431)	(5178.545)
t=9		
Trim 0%	3009.684	12210.047
	(5600.730)	(8466.972)
Trim 10%	4884.903	14552.389
	(5858.094)	(8769.713)
Matching 1:2	1853.260	11198.406
	(5015.728)	(7786.950)
Matching 1:4	5119.536	10961.283
	(4591.474)	(7134.221)

Standard Errors in Parentheses

Note: The sample is the 1979 cohort and estimation were carried out using Stata 14 and psmatch2.