Never too late? Returning to university after completing secondary education as adults

Linn Karlsson*†

September, 2021

Abstract

Complementary adult education provides a second chance for those who, for various reasons, did not complete their upper secondary education. Little, however, is known about the economic gains of those who continue on to higher education. This paper aims to study the effect of university education on economic outcomes among individuals who initially attained low levels of education, and then participated in adult education. Swedish longitudinal population register data from 1990–2015 was used to estimate the effect on income and employment among those who participated in adult education in 1994 and enrolled at university in 1996–1998. Difference-in-difference propensity score matching was used to account for non-random selection to university education. The results reveals significant gains in terms of earnings for those who proceeded on to university, and also their probability of employment increased. Additional heterogeneity analyses showed minor differences between students of different gender, and little to no differences between young and old students.

JEL Codes: I21, I23, I26

Keywords: Adult education, tertiary education, second-chance education, propensity-score matching, earnings

1 Introduction

The OECD and the European Council have often emphasised the importance of lifelong learning and adult education (AE) in helping people adapt to the future world of work,

*Department of Economics, Umeå School of Business, Economics and Statistics, Umeå University, E-mail: linn.karlsson@umu.se

†I am very grateful to Olle Westerlund, Peter Bäckström, Niklas Hanes, David Granlund and Magnus Wikström for valuable comments and suggestions. I extend a special thanks to Olle for access to the data used in this study. Research grants from Handelsbanken (ref. P2016-0299), the Swedish Research Council (ref. 2014-01990) and (ref. 2015-01706) are gratefully acknowledged.

which requires a higher level and a broader set of skills (European Union, 2016; OECD, 2012). Against the background that 20–30% of the students in each cohort did not meet the entry requirements for higher education across OECD countries (OECD, 2020), allowing lower-educated individuals to complete their upper secondary education in AE serves as an important policy tool to stimulate investment in human capital and improve the individual's labour market prospects. There is a popular notion that AE provides a second chance for those who, for various reasons, lack basic schooling. A natural question is whether there is empirical evidence to support this claim. Thus far, the literature has mainly focused on the return to AE among low-educated individuals or the return to university education among older students. Little, however, is known about the economic gains of those who proceeded from AE to university.

This study contributes to the existing literature by answering the following question: what is the effect of university education on the earnings and employment prospects of those who participated in AE? Despite a thorough search of the literature, I have not found this question to have been answered. Therefore, the findings of this study are of great importance to policymakers who want to evaluate AE. In addition, a deeper knowledge of the effects of university studies can help individuals make informed decisions about whether it is worthwhile to invest in a university-level education after participating in AE.

This paper relates to the literature on the effects of completing upper secondary school as adult as well as the literature on completing a late post-secondary education.[1] The effects of completing upper secondary school in AE are somewhat mixed. In the context of Scandinavia, Albrecht et al. (2005) evaluated the effect of one or two semesters of AE that took place between 1997–1998 in Sweden, and analysed the change in wage earnings and employment among those adults between 1994–2000. The results did not reveal any significant effect on earnings, but there was a higher probability for men to find employment after AE. In contrast, other studies on Swedish AE found that there were positive effects for those who were experiencing long-term unemployment (Stenberg & Westerlund, 2008) and for older workers (aged 42–55) (Stenberg et al., 2014). The effects were especially large for women with children. A recent study by Bennett et al. (2020), conducted using Norwegian data, exploited a policy reform that was implemented during the 2000/2001 academic year that enabled adult individuals above the age of 25 to attend high school to analyse the returns to completing high school later. They focused on cohorts born between 1964–1970 and found that completing a high school education had positive effects on earnings and employment probabilities, especially for female students.

Other studies investigated the effects of taking a delayed tertiary degree compared to those who continued on to higher education short after secondary school or those who only experienced upper secondary schooling. Unsurprisingly, completing a degree later in

[1] Other related articles have investigated the effect of training among adults, e.g., Cerqua et al. (2020).

life negatively affected life-long earnings compared to those who completed it earlier, since those individuals had less time to establish themselves on the labour market (e.g., Holmlund et al., 2008; Monks, 1997). However, older workers participating in tertiary education still gained economic benefits compared to those who only attained an upper secondary education (Hällsten, 2012; Stenberg & Westerlund, 2016). Both studies used Swedish data and found larger positive effects on earnings and employment outcomes for women than for men. Stenberg and Westerlund (2016) highlighted the importance of collecting data over a longer period of time, since most effects only emerged after ten years. Using data from the United States, Jacobson et al. (2005a,b) analysed the effect of community college participation on earnings among displaced or older workers.[2] One academic year increased the long-term earnings by about 9% for men and 13% for women (Jacobson et al., 2005a). Other studies, such as Böckerman et al. (2018) and Jepsen et al. (2014) focused on participation in vocational education at a post-secondary level and found positive effects on annual gross earnings and employment. In contrast, Silles (2007) and Jenkins et al. (2003) concluded that there were small to no positive returns for undertaking a late higher-degree education in the UK.

Empirical studies on returns to schooling are typically plagued by the problem of self-selection, where the individuals entering education are likely to be different from those in the general population. Failing to control for self-selection will most likely provide biased estimates on the effects of a return to education. Previous studies used instrumental variables, policy changes, or utilised matching methods to reduce potential bias caused by self-selection. This study uses difference-in-difference propensity score matching to account for self-selection. The main focus of this paper is to analyse the economic outcomes of a return to education in terms of annual gross labour earnings, but I also investigate its effects on employment outcomes in terms of employment probabilities.

This study utilizes nationwide Swedish administrative data covering a period between 1990–2015. The data contain information about socioeconomic background, education level, and yearly gross labour earnings. The sample was restricted to individuals who were between 20–50 years old who did not complete upper secondary school prior to AE enrolment. The study revealed that there was a positive average treatment effect on those who started university education in terms of annual wage earnings. The treatment effect on annual earnings was up to SEK 75,000, which corresponds to 28% higher gross earnings than the matched comparisons 17 years after university enrolment. The estimated effects are considerably larger than similar studies that investigated the economic effect of a late university education. Furthermore, the samples analysed in this study were, on average, 10–20 years younger than the cohorts in Hällsten (2012) and Stenberg and Westerlund (2016), which is another factor that could potentially affect the estimated treatment effect. The results also

[2]A community college education corresponds to an education level between the upper secondary and university level in Sweden.

indicate that university education increased the employment probabilities by, on average, 6–8 percentage points.

The paper is organised as follows. First, I describe the institutional setting, such as the economic environment and the Swedish education system, in Section 2. Section 3 presents the empirical strategy and the available data. Finally, Sections 4 and 5 present the results and conclusions of this study, respectively.

2 Institutional setting

Promoting lifelong learning and supporting AE has long been an important policy goal in Sweden, and the institutional barriers to complete or begin adult or tertiary education later in life are low. Education at the AE and university level is free of charge, and students can apply for financial support in the form of student grants and loans. Furthermore, the employers are required by law to permit workers to take an unpaid leave of absence to study without breaking any employment contracts (SFS, 1974).

Adult education expanded during the 1990s when Sweden faced an economic crisis and increased levels of unemployment. The unemployment rate rose from 3% at the beginning of the 1990s and peaked at 11% in 1997. Since then, the unemployment rate has varied between 6–9%. Swedish policymakers implemented a major adult education programme called the "Knowledge Lift" in 1997–2002 to raise the skill level of workers. The programme included municipal adult education to enhance the general skills of the unemployed, low-skilled adults. Participants in the "Knowledge lift" and other AE initiatives were eligible for different forms of income grants and financial support for their studies. Unemployed individuals, entitled to unemployment insurance at the start of the programme, were eligible for special education support (UBS) equivalent to the individuals' income insurance. Furthermore, the programme greatly increased the number of available study positions at AE institutes. At the beginning of 1990, the number of AE participants was about 150,000; however, this increased to well above 300,000 students at its peak in 1997, when the demand for education increased in conjunction with the implementation of the "Knowledge Lift" (Albrecht et al., 2005; Stenberg, 2019).

Three years prior to the "Knowledge Lift", the cohorts recorded by this dataset enrolled in AE for the first time. At the time, the education system consisted of nine years of compulsory schooling followed by two or three years of upper secondary school. A policy change in 1994 made all upper secondary programmes three years long, but the sample used in this study was not directly affected by this policy change. Upper secondary schooling was not mandatory, and preparatory programmes for both vocational and academic career options were available. To enter university, students, in most cases, needed to have completed three years of upper secondary education.[3] In addition to the general entry requirement, some university degrees had specific entry requirements. For example, STEM fields[4] required additional mathematics courses, which were only provided by science programmes or as additional elective courses in other programmes.

Students that did not fulfil the entry requirements could complete the missing grades during AE.[5] Municipal adult education was, and still is, the most common route by which an individual can finish incomplete compulsory and upper secondary education. Municipalities are obligated by law to provide elementary and secondary education to adults, and AE offers both general courses such as mathematics, English and Swedish, as well as other, more specialised courses. The institutional notation of this is *komvux*, but will instead be denoted as AE henceforth. Even if the most common use of AE is to complete upper secondary school, AE is also open to students who wish to raise their GPA in courses they have already completed.

3 Data and empirical strategy

Data and sampling strategy

This paper uses several sources of nationwide Swedish administrative data which consists of detailed yearly information between 1990–2015 about an individual's characteristics linked to additional information related to education, such as educational attainment, registration data on AE, and university education. In addition, data on earnings and transfers related to the social insurance system such as unemployment insurance, parental leave, sick benefits, and student grants are available. Treatment is here defined as being registered at university and completing at least one semester and a detailed description will be provided in the upcoming paragraph. A complete list of the variables and sources of data can be found in Table A1 in the Appendix. The data were collected by Statistics Sweden and is of high quality; it was not self-reported, not top-coded, and does not distinguish between the data collected from treated and untreated individuals. Furthermore, observations with zero earnings are also included in the data, and are clearly distinguished from missing observations. Thus, there are very few potential sources of bias caused by measurement

[3]Between 1979–2008, due to the so-called 25:4 rule, low-educated students could also be admitted if they were 25 years or older, had at least four years of work-life experience, and a passing grade in Swedish and English (Askling et al., 2001). Today, students with incomplete upper secondary schooling can be admitted by a criterion called "real competence", which proves that the student has gained experience from work or other relevant activities. The number of students admitted by this criteria is, however, small. Based on my own calculations and data from the Swedish Higher Education Authority, only 0.3% of students were admitted by this criterion in 2006.

[4]Science, technology, engineering, and mathematics.

[5]Adult education includes several types of institutions, such as municipal adult education and folk high schools. The folk high schools are less flexible in their courses and mainly focus on the core subjects of Swedish, English and mathematics.

errors in the dataset (Heckman et al., 1998).

Treatment is defined as being registered at a university, completing at least one semester (30 credits) within three years, and occurs at year $t = 0$. I used a combined measure of registration and one completed semester to define treatment since registration data alone did not reveal whether the students actually attempted to complete a university education. Furthermore, I used one completed semester instead of a completed degree as an indicator of treatment because I wanted to study the effect of starting a university education – this criterion includes dropouts in the treatment category. The data do not reveal the intended length of study and many students drop out before the end of a bachelor programme. Using degree completion as a measure of treatment would omit students who potentially could be considered to be treated but found a job before graduation. In the studied sample, around 80% of the students completed 30 credits within one year, and only a few completed their first semester later.

I set several restrictions to more clearly define the profile of the samples studied. First, the samples were restricted to individuals with at least one completed course in AE by 1994. Second, individuals must not have more than two years of upper secondary education before 1994, as I wanted to focus on those who used AE to gain entrance into university while excluding individuals who used AE to only improve their GPA. Third, individuals with less than two years of upper secondary education at $t = 0$ were excluded to ensure that the control group was comparable to the treatment group in terms of university eligibility after participation in AE. Using three years of upper secondary education would have resulted in a relatively large loss of observations because inaccurate reporting of the education level in the data. Finally, I only included individuals between 20–50 years old at the time of AE participation. By having these age restrictions, I was able to ensure that there would be a follow-up period that was long enough to estimate the long term effects of university education after treatment. I also chose to set a lower age limit of 20, partly because it was the minimum age of enrolment into AE[6], but also because pre-treatment earnings can be somewhat misleading for younger individuals when using the difference-in-difference framework. The difference-in-difference setup used in this study relies on information from the averages calculated from pre-treatment earnings and employment. As the pre-treatment earnings and employment are most likely very low for young individuals, these variables are less successful in capturing individual time-invariant unobserved characteristics and provides less information than older individuals.

The outcome variables, such as change in annual gross earnings, are defined as $\Delta Y_{i,t} = Y_{i,t} - ((Y_{i,-1} + Y_{i,-2} + Y_{i,-3})/3)$, where $Y_{i,t}$ indicates the annual gross earnings for individual i at year t, and where $t = 0$ denotes the year when the treated individuals enrolled at university. This difference-in-difference approach is used to capture unobserved time-invariant

[6] A student can enrol in AE the second half of the calendar year of which they turn 20.

individual characteristics that affect annual gross earnings. The other outcome variable of interest is employment. Employment can be measured in several ways, where the most ideal metric would have been data on the number of hours an individual has worked. Unfortunately, this information was not available in the current dataset. Previous studies, such as Hällsten (2012) and Stenberg and Westerlund (2016), used an indicator variable that was equal to one if the annual earnings for the individual exceeded the median earnings of the population at age 45, and zero otherwise. I defined a relatively simple measure of employment, which was a dummy variable that was equal to one if the individual's yearly gross labour earnings was larger than zero; the difference-in-difference outcome for employment is defined as $\Delta E_{i,t} = E_{i,t} - ((E_{i,-1} + E_{i,-2} + E_{i,-3})/3)$. I use this wider definition since a majority of the sampled individuals were considerably younger than the studied samples in Hällsten (2012) and Stenberg and Westerlund (2016).

An alternative approach to estimating the effects on employment is by using the yearly sum of unemployment benefits as the dependent variable. The results from this specification are reported Figure B1 and B2 in the Appendix. In contrast to the metric of employment analysed in this study, this measure is dependent on income and can be viewed as a sensitivity analysis.

The entire sample contains 10,033 individuals, of which 1,861 were considered to be treated and 8,172 were considered to be untreated. Note that the control group does not include observations that became treated before or after the years of study. Additional analysis of the observations in this control group, not reported in this paper, did not alter the main results.[7] I split the sample into three subsamples to perform separate matching for the cohorts entering university in 1996, 1997, and 1998. I specifically studied the treatment effects during these years due to several reasons. First, the sample contained individuals as young as 20 in 1994. As previously discussed, the pre-treatment earnings of these individuals can be somewhat misleading in the difference-in-difference setup, and it is better to study those who began treatment at least two years after AE enrolment. Second, the 1995 cohort would only include those who had finished their upper secondary education within one year. Third, the number of treated observations decreases substantially in later cohorts; 941, 591 and 329 individuals began their university education in 1996, 1997 and 1998, but only 250 and 232 were treated in 1999 and 2000, respectively.

Note that the potential control group was comprised of the same individuals in each cohort, but was matched on pre-treatment variables for different years depending on the year of treatment for each cohort. For example, the treated and control groups in the 1996 cohort were matched on variables from 1990–1995, while the same control group and treated group from the 1997 cohort were matched on variables between 1991—1996. Similarly, the

[7] 2,644 students entered university before or after the studied years. The majority of these students entered university in 1995 or 1999, and had completed at least three years of university studies at the end of the follow-up period.

treated and control groups in the 1998 cohort were matched on variables from 1992–1997. However, some observations in the control group may have been dropped from the earlier cohorts if they had less than two years of upper secondary schooling in 1996 but had achieved this higher level of education in 1997 or 1998.

Empirical strategy

Participation in higher education after AE was not randomly assigned, and self-selection into treatment is likely to be influenced by expected gains of further education and other various characteristics, such as ability or socioeconomic background. For example, individuals of higher ability may find academic studies easier and therefore face fewer costs during their studies and also receive greater returns from higher education. Thus, simply comparing the earnings and labour market outcomes of those who proceeded to higher education with those who chose not to do so is likely to yield biased estimates and overestimate the effects of education. The main strategy used in this paper to account for self-selection biases was to use difference-in-difference propensity score matching (PSM), which was used to obtain credible counterfactual outcomes for the treated groups, for a given set of variables that influence the selection into treatment (e.g. Austin (2011) and Rosenbaum and Rubin (1983)). Compared to other matching methods, PSM is particularly appropriate when many covariates are used and when they are continuous (Stuart, 2010). Other matching algorithms, such as exact matching, would only result in a small number of matches in this set-up.

The first step is to estimate the probability of treatment, also known as the propensity score, using pre-treatment characteristics. Treated individuals are then matched to controls with similar propensity scores, since these individuals are assumed to have the same probability of being treated. The final step is to estimate the average treatment effect (ATT). The ATT compares the outcome of an individual in the treatment group with the counterfactual outcome; i.e. the outcome of a comparable individual in the control group. A formal way of expressing the ATT is: $ATT = E[Y_{1i} - Y_{0i}|D = 1]$, where D denotes the treatment status; specifically, $D = 1$ if the individual was treated (i.e. proceeded on to higher education), and $D = 0$ if the individual remained untreated and did not enter university. Thus, Y_{1i} is the outcome (for example, annual gross earnings) for an individual i if they were treated, and Y_{0i} is the outcome the individual would have experienced in the absence of treatment.

Propensity score matching relies on three key assumptions: conditional independence, common support, and the stable unit assumption. Conditional independence, $(Y_1, Y_0) \perp D|X$, means that the potential outcomes are independent of treatment status when conditioned on the covariates X. That is, controlling for X, the assignment of individuals to treatment is effectively random. This assumption allows the untreated units to be used to construct an unbiased counterfactual outcome for the observations in the treatment group (Rosenbaum & Rubin, 1983). The second assumption, common support, assumes that there is sufficient overlap between the characteristics of the treated and untreated such that good matches can be found. Since I was estimating the ATT on members of the treated group, the common support assumption can be relaxed to $P(D = 1|X) < 1$, meaning that there only needs to be similar untreated observations to the treated observations for PSM to be feasible. Finally, the stable unit assumption requires that the control group be unaffected by the treatment; treated individuals should not influence the outcomes of the control group (Caliendo & Kopeinig, 2008). Furthermore, I used matching with replacement; this means that each untreated unit could potentially be matched with several treated units. Matching with replacement gives similar results as matching without replacement when the sample size is large, but yields better matches for small sample sizes (Dehejia & Wahba, 1999). The next decision was to choose the number of matches that would be assigned to each treated observation, which is a trade-off between variability and bias. Bias increases when the number of observations is increased, but more matches yield better precision. Following Rosenbaum (2020), I set the algorithm to match each treated unit with its four nearest neighbours.

In the matching procedure, I estimated the propensity score using attributes such as age, foreign background, and socioeconomic status. The latter was measured using variables such as the highest education of the individual's parents, gender, marital status, number of children, region of residence, and the education level at the time of AE.[8] These characteristics were considered to be the most influential with regards to the enrolment decision and the labour market outcomes that are of interest to this study. For example, I matched individuals on age since, for younger individuals, there are lower opportunity costs to studying and they have more time to accumulate their human capital after their studies; these individuals were thus more likely to enrol. I also match on gender, since a common observation from previous studies, confirmed by the descriptive statistics in this paper, is that women are over-represented in adult and post-secondary education. Other important factors that could have affected selection to treatment, and should be included in the model, are family attributes. For example, childbearing and household responsibilities are likely to influence the decision to enrol into higher education (Lechner & Wiehler, 2011), and probably affect future earnings if the individual decreases his/her labour supply. I also include socioeconomic background, measured using the highest level of education attained by an individual's parents, since there is a well established association between parents education and children's education (Black et al., 2005). Finally, the region of residence captures local labour market conditions so that the treated and untreated face the same economic environment before treatment (Blundell & Costa Dias, 2000). To ensure that any potential matches had similar educational backgrounds to the treated observations, I include the education level in 1994 in the matching model.

[8] The complete list of variables can be found in Tables A1 in the Appendix

Ability is also assumed to affect both the selection into education and the economic returns. It is, to some extent, reflected by pre-treatment labour market outcomes such as previous earnings and employment status, but is also by socioeconomic status. To ensure that the results were not driven by confounding factors that were not captured by these variables, I performed additional robustness checks using data from the military enlistment test scores for cognitive and non-cognitive skills. This data, however, was only available for men.

In addition, I included a set of variables that measured changes in earnings and employment probabilities six years before treatment. Formally, the change in earnings the year before treatment was defined as $\Delta earnings\ -2\ to\ -1 = (Y_{-2} - Y_{-1})/(Y_{-2} + Y_{-1})$. The second lag, capturing the change between year $t = -3$ and $t = -2$, was defined as $\Delta earnings$ $-3\ to\ -2 = (Y_{-3} - Y_{-2})/(Y_{-3} + Y_{-2})$; the same was done for the three remaining lags. A positive change indicated an increase in earnings compared to the previous year. When analysing employment outcomes, I extended the model and included five lags to calculate the pre-treatment changes in employment probabilities.

Descriptive statistics

Table 1 reports the descriptive means and standard deviations for a selection of pre-treatment variables organised by treatment status and cohort. Systematic differences between treated and untreated can be clearly observed. For the majority of the variables, values are reported at $t = -1$, while average earnings contain longer lags. Annual gross earnings and other values were measured in 1000s of SEK and at 2015 values; at that time, one Euro was worth approximately SEK 9.35.

Overall, there is an over-representation of women in both groups. The treated tended to be younger and have fewer children than the untreated; this is also reflected by the lower proportion of treated individuals who received income from parental leave. In addition, a smaller proportion of the treated was married. Furthermore, the highest level of completed education as of 1994 was found to be somewhat higher among the treated. The variable was measured in the spring semester of 1994, meaning that some individuals completed the final upper secondary within one year. These individuals probably only missed a few courses and were therefore eligible for university relatively quickly. However, these individuals only comprised a few per cent of both groups. The fraction of individuals with at least one moderately or highly educated parent[9] was also larger among the treated (76% compared to 64% of the treated and untreated individuals in the 1996 cohort, respectively).

When focusing on the pre-treatment indicators of labour market outcomes, the fraction of individuals with unemployment insurance was larger among the treated. However, it

[9] Moderately or highly educated parent is defined as having one parent with at least two years of upper secondary education or more.

Table 1: Descriptive mean statistics for selected variables in cohorts at $t = -1$ unless stated otherwise. Earnings and other transitions are measured in 1000s of SEK. The means and variances of the cohorts are also presented in Tables A4, A5 and A6 in the Appendix.

	1996				1997				1998			
	Untreated		Treated		Untreated		Treated		Untreated		Treated	
Variables	Mean	sd	Mean	sd	Mean	sd	Mean	sd	Mean	sd	Mean	sd
Gender (woman)	0.63	0.48	0.57	0.50	0.63	0.48	0.59	0.49	0.63	0.48	0.71	0.45
Age	30.87	7.60	27.35	5.71	31.88	7.61	27.99	5.43	32.84	7.59	28.95	5.91
Foreign background[a]	0.08	0.27	0.05	0.23	0.08	0.27	0.07	0.25	0.08	0.27	0.07	0.26
Parental education[b]	0.64	0.48	0.76	0.43	0.64	0.48	0.77	0.42	0.65	0.48	0.81	0.40
Married	0.30	0.46	0.19	0.39	0.30	0.46	0.22	0.41	0.31	0.46	0.20	0.40
Number of children	0.93	1.17	0.63	1.05	0.94	1.16	0.73	1.15	0.97	1.16	0.72	1.08
High. level of educ. in 1994												
Less than 9 years	0.0002	0.01	0	0	0.0005	0.02	0	0	0.0005	0.022	0	0
9 years of compulsory	0.005	0.072	0.03	0.16	0.006	0.09	0.03	0.18	0.008	0.087	0.042	0.20
2 years of upp. secondary	0.97	0.17	0.93	0.25	0.97	0.18	0.94	0.24	0.97.	0.18	0.91	0.27
3 years of upp. secondary	0.025	0.16	0.044	0.20	0.025	0.16	0.04	0.19	0.026	0.16	0.04	0.19
Frac. unemp. insurance	0.52	0.50	0.59	0.49	0.48	0.50	0.59	0.49	0.44	0.50	0.58	0.49
Unemp. insurance if >0	32.44	25.45	26.59	19.80	37.52	27.31	30.57	21.39	39.34	27.50	31.46	21.67
Frac. student income	0.50	0.50	0.80	0.40	0.21	0.41	0.65	0.48	0.17	0.37	0.59	0.49
Student income if >0	28.63	18.51	32.44	17.67	35.33	22.46	35.69	19.36	36.55	22.40	38.99	21.25
Frac. sickness benefit	0.14	0.34	0.08	0.28	0.16	0.37	0.10	0.30	0.15	0.35	0.10	0.30
Sickness benefit if >0	26.42	38.97	27.11	44.44	20.36	32.95	15.25	24.16	21.76	33.76	17.21	26.55
Frac.parental leave	0.22	0.42	0.14	0.35	0.26	0.44	0.17	0.38	0.28	0.45	0.16	0.37
Parental leave if >0	15.63	25.62	14.81	25.92	17.54	26.00	15.40	22.75	17.84	26.13	21.92	25.25
Non-cognitive test score	4.72	1.66	5.18	1.43	4.71	1.66	4.86	1.46	4.71	1.66	5.21	1.67
Cognitive test score	4.66	1.58	5.37	1.34	4.66	1.58	5.20	1.54	4.66	1.58	5.13	1.43
Average annual earnings[d]	71.19	62.75	60.27	46.44	76.36	66.81	53.90	51.72	90.40	74.52	64.77	55.45
n	8,150		941		8,161		591		8,172		329	

Note: A complete list of variable definitions can be found in Table A1 in the Appendix.
[a] Dummy variable equal to one if foreign-born or if both parents were foreign-born.
[b] Dummy variable equal to one if at least one parent had two years of upper secondary education or more.
[c] Dummy variable equal to one if highest level of education was at least two years of upper secondary education.
[d] Average annual gross earnings between $t = -6$ and $t = -1$

should also be noted that the amount received from unemployment insurance was smaller on average, indicating that the time spent unemployed was probably shorter among the treated. The treated also had lower average earnings, fewer sickness benefits, and more income from student loans and grants than before treatment.

The average annual gross earnings and employment rate are reported in Table 2. The earnings formed a U-shape over the years, with a trough in 1994, reflecting the subjects' participation in AE at that time. Employment, constructed as a dummy variable indicating if yearly earnings were larger than zero, followed a similar pattern where the employment was largest at the beginning and the end of the studied years.

Table 2: Average annual wage earnings, Y, and employment rate, E, 1990–2015.

	Y		E	
Year	Mean	sd	Mean	sd
1990	73.66	56.32	0.93	0.25
1991	82.50	61.57	0.92	0.27
1992	84.10	64.76	0.88	0.32
1993	76.99	70.17	0.83	0.38
1994	62.55	66.51	0.80	0.40
1995	68.16	71.19	0.80	0.40
1996	84.36	79.10	0.81	0.39
1997	93.51	86.15	0.82	0.38
1998	104.87	91.28	0.85	0.36
1999	120.44	95.24	0.87	0.34
2000	139.65	100.98	0.89	0.32
2001	158.04	106.53	0.90	0.30
2002	168.72	111.08	0.90	0.30
2003	173.55	114.76	0.90	0.31
2004	180.58	119.89	0.88	0.32
2005	188.21	126.29	0.88	0.33
2006	199.64	130.03	0.88	0.32
2007	215.08	137.44	0.89	0.31
2008	229.84	146.05	0.89	0.32
2009	233.05	151.54	0.88	0.33
2010	241.21	160.96	0.87	0.33
2011	251.88	166.28	0.87	0.34
2012	260.98	171.80	0.86	0.34
2013	266.30	178.60	0.86	0.35
2014	272.99	184.39	0.85	0.35
2015	279.78	194.03	0.84	0.36

Note: Earnings and employment rates, 1996–2015. n= 10,033. Measured in 1000s of SEK (2015 values).

The data also shows the differences in educational choices between the treated and untreated groups in terms of gender. Looking at the highest level of education attained in Table 3, I noted that the majority of the treated group (88%) completed at least three years of education at the university level at the end of the follow-up period (2015). Women also tended to attain a higher level of education than men. Table 3 shows that only 9% of the treated women had less than three years of university education. In contrast, the corresponding value was 16% for men. Overall, the most common fields of education were the social sciences, business and law, and health, welfare, and social services (see Table A2 in the Appendix). As expected, there were gender differences in the selection of university programmes; men primarily chose an education in fields such as engineering, manufacturing, and construction, while more women tended to study for health, welfare, and social services degrees.

The enlistment test measures both cognitive and non-cognitive abilities, where the former is conducted using subtests measuring logical, verbal, and spatial ability, as well as technical comprehension. The enlistment test for non-cognitive abilities measures traits and abilities that are of importance in military duty. The test score is based on a psychological evaluation given by a psychologist, where abilities such as responsibility, independence, emotional stability, and hardiness are measured. The overall cognitive and non-cognitive ability is based on scores in the subtests and is reported on a stanine scale from 1–9. The literature on cognitive and non-cognitive ability shows positive associations between ability and economic outcomes and selection into education (Deming, 2017; Lindqvist & Vestman, 2011; Nybom, 2017). In this dataset, the level of cognitive and non-cognitive scores was, on average, higher among the treated in the studied cohorts; this can be observed in Figure B3 in the Appendix and Table 1 in the descriptive statistics.

Table 3: Highest level of completed education in 2015 among untreated and treated groups(%)

	Untreated			**Treated**		
High. level of education	Men	Women	Full sample	Men	Women	Full sample
Upper secondary, 2 years	63	57	60	0	0	0
Upper secondary, 3 years	37	43	40	0	0	0
University, less than 2 years	0	0	0	16	9	12
University, 3 years	0	0	0	60	62	61
University, 4 years or more	0	0	0	25	29	27
Total	100	100	100	100	100	100

Note: The highest level of completed education at the end of the follow-up period (2015). Students with only one completed semester falls within the first university category, while a student with a bachelors degree (three years) is placed in the second university category. Note that the column *Full sample* shows the highest level of education for both men and women in each treatment group.

4 Results

In this section, I present the estimated treatment effects from the PSM estimations. First, I briefly discuss balancing the covariates and probit estimates used to construct the propensity scores. I then present the trajectories of the earnings and employment outcomes, and visualise common trends for the treated group and the matched observations from the control group. Following this, the propensity score difference-in-difference estimates of the treatment effects are presented for the baseline model in Figures 2 and 3 for each separate cohort. Finally, I present an analysis of heterogeneity in treatment effects by gender and age, as well as the robustness checks with additional controls for ability.

Estimation of propensity score and balancing of covariates

In this paper, the propensity score was estimated using a probit model based on pre-treatment variables that could affect selection into treatment and the outcomes. The probit estimates of university education, reported in Table A3 in the Appendix, show that gender has little impact on the probability of university enrolment for the 1996 and 1997 cohorts, which might seem surprising since there is a well-documented female overrepresentation in tertiary education. However, this can be explained by the fact that the studied cohorts were derived from a sample of students that decided to participate in AE, where women are overrepresented as shown in Table 1. In addition, an individual's marital status and the number of children only had minor effects that were insignificant, respectively. Furthermore, as expected, the probability of university enrolment was positively related to a higher level of education in 1994 as well as the highest level of education attained by the individual's parents. Among the other results, it should be mentioned that the probability of university enrolment is positively affected by having student income at $t = -1$.

Balancing tests are commonly used for to determine whether the matched comparisons constitutes a appropriate counterfactual to the treated. Tables A4, A5, and A6 in the Appendix report the means and variances of the balancing tests on the unmatched and matched data after PSM. The standardised differences[10] in the matched sample are close to zero, and the standardised variances are close to one (Austin, 2009), indicating that the covariates in the samples are well balanced based on the propensity score estimates. Kernel density plots of the propensity scores are presented in Figure B4 and show that the overlapping assumption is not violated and that all treated units are supported.

[10]The standardised difference for continuous variables is defined by $d = \frac{(\bar{x}_{treatment} - \bar{x}_{control})}{\sqrt{(s^2_{treatment} + s^2_{control})/2}}$, where $\bar{x}$ denotes the sample mean of the covariate, and by $d = \frac{(\hat{p}_{treatment} - \hat{p}_{control})}{\sqrt{(\hat{p}_{treatment}(1-\hat{p}_{treatment}) + \hat{p}_{control}(1-\hat{p}_{control}))/2}}$ for dichotomous variables, where $\hat{p}$ denotes the prevalence or mean of the dichotomous variable (Austin, 2009).

Earning trajectories for treated and matched comparisons

Figure 1 displays average annual gross earnings and employment outcomes of the treated and matched untreated comparisons. Difference-in-difference estimations rely on the assumption of parallel pre-treatment trends to derive a causal interpretation, meaning that the difference between the treated and untreated groups should be constant over time in the absence of treatment. In terms of annual wage earnings, Figure 1 shows a distinct parallel trend until $t = 0$. Thereafter, the earnings among the treated drop when they start university. The treated earn less than their matched comparisons until four years after treatment, when their income recovers and surpasses the matched comparisons. At the end of the studied period ($t = 17$), there is a clear gap in earnings where the average annual wage earnings of the treated are well above SEK 300,000. In comparison, the matched untreated earn around SEK 270,000.

The average employment rate followed a similar trend and decreased between $t = -5$ and $t = -1$, but diverged at $t = 0$ as the treated entered university in the 1996 and 1997 cohorts. The pattern for the 1998 cohort was substantially different from the other years, which can partially be explained by the small number of treated observations. Although I observed a clear difference between the treated and matched comparisons after $t = 4$, similar to the other cohorts, the ATTs of 1998 may be biased, because for this cohort the pre-treatment trend differ across treated and matched untreated comparisons.

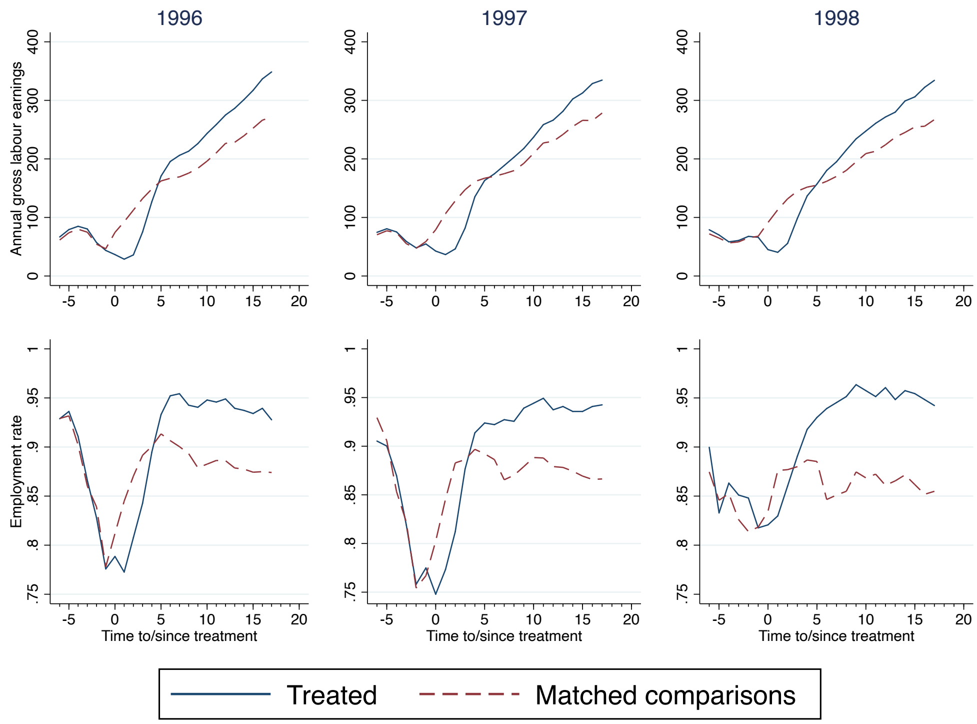

Figure 1: Average annual gross wage earnings for the treated and untreated matched comparisons. Earnings are measured in 1000s of SEK (2015 values) and employment is measured as a dummy variable that is equal to one if the individual's annual earnings are larger than zero.

Treatment effects according to difference-in-difference analyses

Figure 2 shows the difference-in-difference estimates of the ATT for earnings for each cohort in the baseline model. As observed in the earning trajectories shown in Figure 1, the earnings appear the same and follow a similar trend between $t = -6$ and $t = 0$, indicating that the annual earnings are well-balanced among the treated and matched untreated comparisons. Unsurprisingly, and as can be seen in Figure 1, there is an initial income drop related to the start of university studies. However, the drop is followed by increasing earnings, and the annual income catches up after approximately five years. Focusing on the 1996 cohort, the income drop at $t = 0$ is estimated to be approximately SEK 40,000, but at the end of the studied period ($t = 17$), the treated earns SEK 75,000 more on average than the untreated matched comparisons (corresponding to a 28% increase). It is notable that the estimated effects on earnings are substantially larger than those found by Stenberg and Westerlund (2016) and Hällsten (2012).

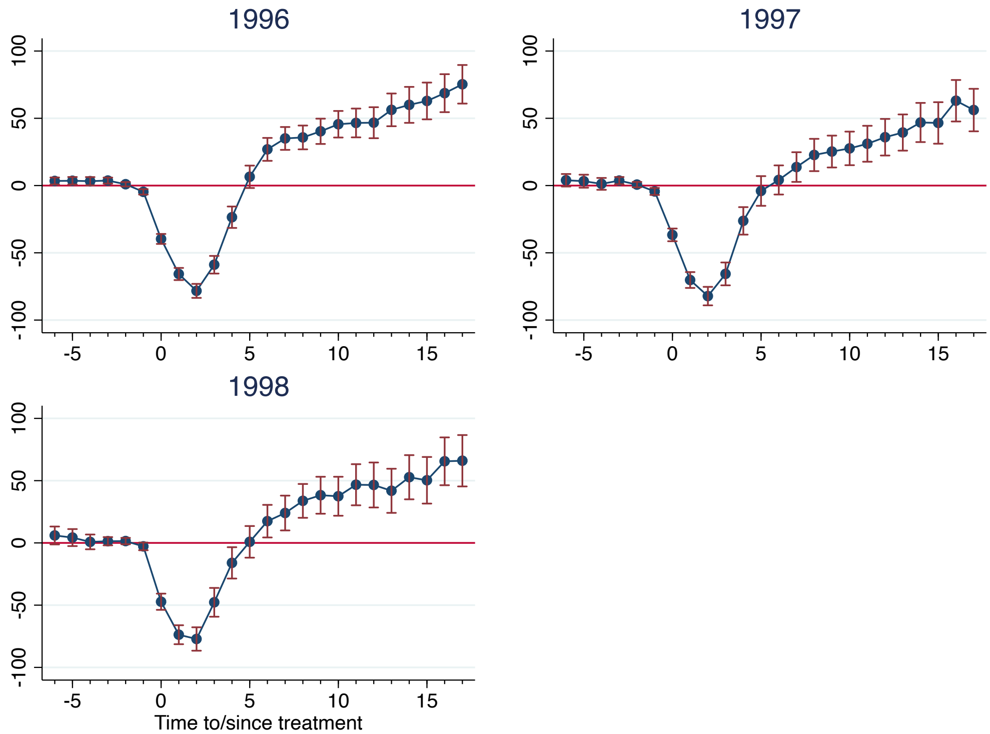

Figure 2: Difference-in-difference PSM estimates of annual gross wage earnings, measured in 1000s of SEK (2015 values). The dots represents the point estimates of the ATT and the error bars represent the associated 95% confidence intervals. The Abadie and Imbens robust standard errors as calculated by the *teffects psmatch* command in Stata 14.

The reported results in Figure 2 clearly show that university education has substantial positive effects on annual labour earnings in the long run. However, the results also suggest that university enrolment is associated with lower earnings during education. One central question is whether the increased wage in the future is worth the foregone earnings? To answer this question, the estimated treatment effects were used to calculate the net present value (NPV) of university education.[11] The NPV is reported in Table 4 and is positive for all cohorts at the end of the follow-up period ($t = 17$). The effects are smallest for those treated in 1997. Because of the relatively large income drop at the beginning of the treatment in 1997, in contrast to the other cohorts, university education only pays off 17 years after university enrolment. Furthermore, the NPV at $t = 17$ is substantially smaller for the 1997 cohort than the other cohorts, with a difference of up to SEK 158,000 compared to the 1996 cohort. The NPV first becomes positive at $t = 13$ for the 1996 cohort and $t = 17$

[11]The net present value is calculated using the following equation $NPV = \sum_{t=0}^{T} (ATT/(1+r)^t)$, where T varies from 0 to 17, and a discount rate of $r = 0.03$ is used.

for the 1997 cohort. Note that the estimated NPV of university education only covers the studied period and probably underestimates the effect on lifetime earnings and pensions.

Table 4: The NPV of difference-in-difference propensity score estimates.

t	1996	1997	1998
0	-39.7	-36.7	-47.3
1	-103.5	-104.8	-118.9
2	-177.3	-182.3	-191.6
3	-231.2	-242.4	-235.3
4	-252.1	-265.7	-249.5
5	-246.5	-269.2	-248.8
6	-224.0	-265.7	-234.2
7	-195.5	-254.5	-214.7
8	-167.3	-236.6	-188.0
9	-136.3	-217.2	-158.7
10	-102.4	-196.7	-130.8
11	-68.8	-174.2	-97.1
12	-36.0	-149.0	-64.4
13	2.3	-122.2	-36.0
14	41.9	-91.2	-1.1
15	82.3	-61.3	31.2
16	125.1	-22.0	72.0
17	170.7	12.0	111.9

Note: The NPV for each year is calculated by $NPV = \sum_{t=0}^{T}(ATT/(1+r)^t)$, using a discount rate, r = 0.03.

The other outcome of interest, employment probability, is reported in Figure 3. As expected, the employment probability is lower among the treated between $t = 0$ and $t = 4$ since the treated are eligible for students grants and loans during the education. The 1996 cohort fulfils the condition of parallel trends before treatment as shown by the near-zero coefficients, but the same does not hold for the 1997 and 1998 cohorts, which can partly be explained by the smaller sample sizes for these years (n = 591 and n = 329, respectively). Hence, I chose to mainly focus on the 1996 cohort. The average treatment effect on employment probability is -7 percentage points at $t = 1$ but increases and varies around 5 percentage points after $t = 5$.

Finding comparable results regarding the effects of university education on employment is difficult since previous literature used different set-ups in terms of sample studied and definition of the employment measure. One should be aware of the different settings when comparing the results with previous literature. The closest example, Stenberg and Westerlund (2016), found that enrolling at university was related to an positive effect on the employment probability of 2.8 percentage points for men and 12.3 percentage points for women. Note, however, that they defined employment as a binary variable taking the value of one if the income exceeded the median earnings of the population at age 45. Hällsten (2012) used the same definition of employment, and denoted it as "stable employment". He estimated an 18 percentage point increase in the probability of (stable) employment, but in contrast to Stenberg and Westerlund (2016), he used defined treatment as having at least three years of post-secondary education, and estimated the employment probabilities with a fixed-effects model. The average employment rate in his sample were around 80% and can be compared to the average employment rate of 87% in this study.[12] Nevertheless, that the employment estimate from Hällsten (2012) is higher than what is found in this paper is expected because I include those who dropped out before degree completion, while he studied the effect of having at least three years of education.

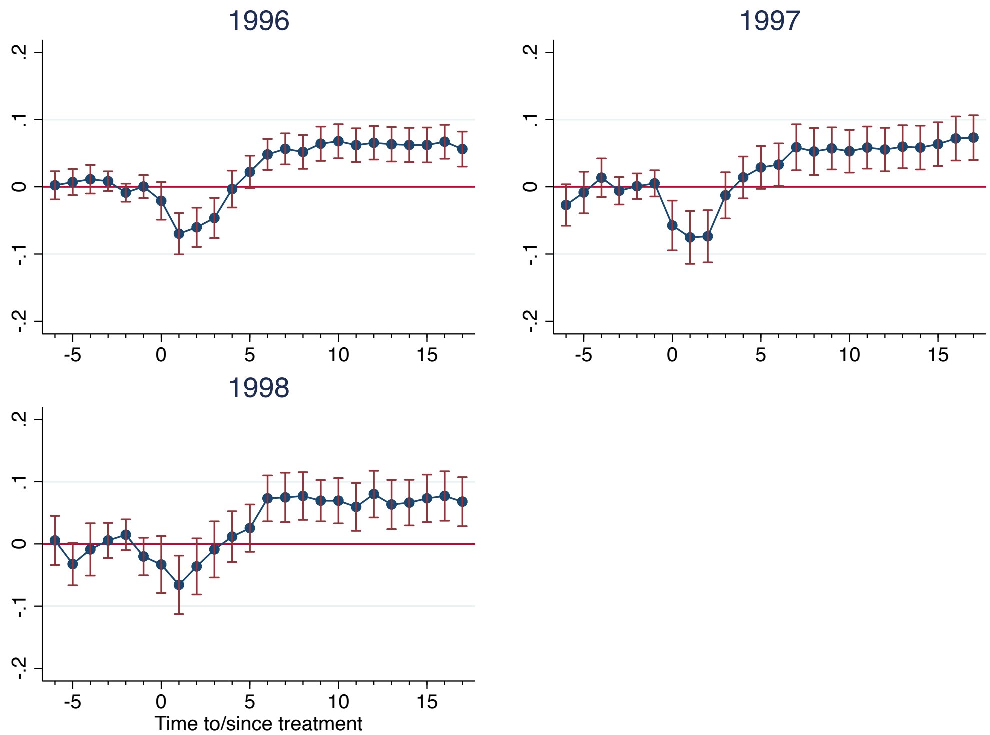

Figure 3: Difference-in-difference PSM estimates of employment probability measured in percentages. The dots represent the point estimates of the ATT and the error bars represent the associated 95% confidence intervals.

The difference-in-difference for point estimates of income (in Figure 2) and employment probabilities (in Figure 3) can be summarised over longer time periods. The average treatment effects of the outcome variables are reported for a larger set of years which begin at $t = 0$ until the end of the follow-up period at $t = 17$. The averaged treatment effects for

[12] Hällsten (2012) studied a sample of 796,054 individuals between 1981–2007.

shorter intervals before, during and after treatment are also reported. More specifically, I construct averages in the years $t = 0$ to 17, $t = -6$ to -1, $t = 0$ to 5, and $t = 6$ to 17. These results are presented in Table 5

In terms of annual gross earnings, I observed a negative effect on earnings during the initial period of the treatment, as reported in the second row, but the average estimates became positive after $t = 6$. The ATT covering the extended period (year 0–17) is SEK 18,900 and was statistically significant for the 1996 cohort, but considerably smaller in the other cohorts (SEK 7,000–14,400 and not statistically significant for the 1997 cohort). The overall effects for employment probabilities, reported in the last row of Table 5, indicates that the treated are 3–5 percentage points more likely to be employed than the matched comparisons. The average effects from $t = 6$ to $t = 17$ are significant, ranging between 6–8 percentage points, suggesting that a university education decreases the probability of having no income by approximately 50%.[13] Note, however, that the pre-treatment employment was imbalanced for the 1997 and 1998 cohorts. Thus, these results cannot be interpreted as causal effects for these cohorts.

In summary, the main results indicate large and positive effects on annual gross earnings, which is considerably larger than similar previous studies looking at the economic returns to university (Hällsten, 2012; Stenberg & Westerlund, 2016). Even if estimated effects on employment probabilities from previous studies are not fully comparable to the results in this paper, the estimates in this study do not contradict the findings from earlier work as I observe a positive effect of higher education and on the employment probability.

Table 5: Difference-in-difference PSM estimates of average annual wage earnings and employment probabilities.

	ATT earnings			ATT employment		
Time period	1996	1997	1998	1996	1997	1998
-6 to -1	1.70***	1.40	1.84	0.003	0.004	-0.01
	(0.61)	(1.05)	(1.50)	(0.004)	(0.006)	(0.01)
0 to 5	-43.27***	-47.50***	-43.52***	-0.03***	-0.02*	-0.01
	(2.39)	(3.09)	(3.90)	(0.01)	(0.01)	(0.01)
6 to 17	49.99***	34.38***	43.38***	0.06***	0.08***	0.08***
	(4.78)	(5.59)	(7.04)	(0.01)	(0.01)	(0.01)
0 to 17	18.90***	7.08	14.41***	0.03***	0.04***	0.05***
	(3.64)	(4.40)	(5.37)	(0.009)	(0.01)	(0.01)
n	9,091	8,752	8,501	9,091	8,752	8,501

Note: The outcome variables are the difference-in-difference earnings or employment probabilities averaged over larger time intervals. Abadie and Imbens robust standard errors in the parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[13] The probability of having no income for the matched comparison is approximately 13% according to Figure 1.

Heterogeneity analysis and robustness checks

Gender

Previous studies on the return to adult and university education often reported larger effects for women (Bennett et al., 2020; Böckerman et al., 2018; Hällsten, 2012; Stenberg & Westerlund, 2016). Thus, it was interesting to investigate whether there were any systematic differences between men and women in this setting. I analysed the heterogeneity of the treatment effects by gender by performing PSM for men and women separately. Since the parallel trends assumption was violated for employment in the 1997 and 1998 cohorts, I only looked at the effects of university education on annual gross earnings.

Overall, there were no substantial differences between genders in terms of annual gross earnings. Figure 4 reports the estimated treatment effects on earnings by gender. There is an initial income drop at the beginning of treatment similar to the main results. However, the income drop associated with university enrolment is smaller for women than for men, which is most likely explained by the overall income differences between the genders. For example, men have, on average, 27% higher earnings than women at $t = -1$ in the 1996 cohort (SEK 80,400 compared to SEK 63,300), thus making the income loss related to university enrolment larger in absolute terms.

In addition to the larger income drop, treated men take more years to catch up in earnings with their matched comparisons, partially explained by men taking more time to complete a degree (see Figure B5). The treatment effect was only larger for men enrolling at university in 1996, while the treatment effects for women were larger in the 1997 and 1998 cohorts. At $t = 17$ for the 1996 cohorts, men had 23.4%[14] higher earnings than their matched comparisons while women earned 32.2% relative to their comparisons[15].

[14] Calculated by using the average annual earnings for the matched comparisons which was SEK 324,700 and ATT^{men} = SEK 76,100

[15] The average annual earnings at $t = 17$ for the matched comparisons was SEK 234,400 and ATT^{women} = SEK 75,500

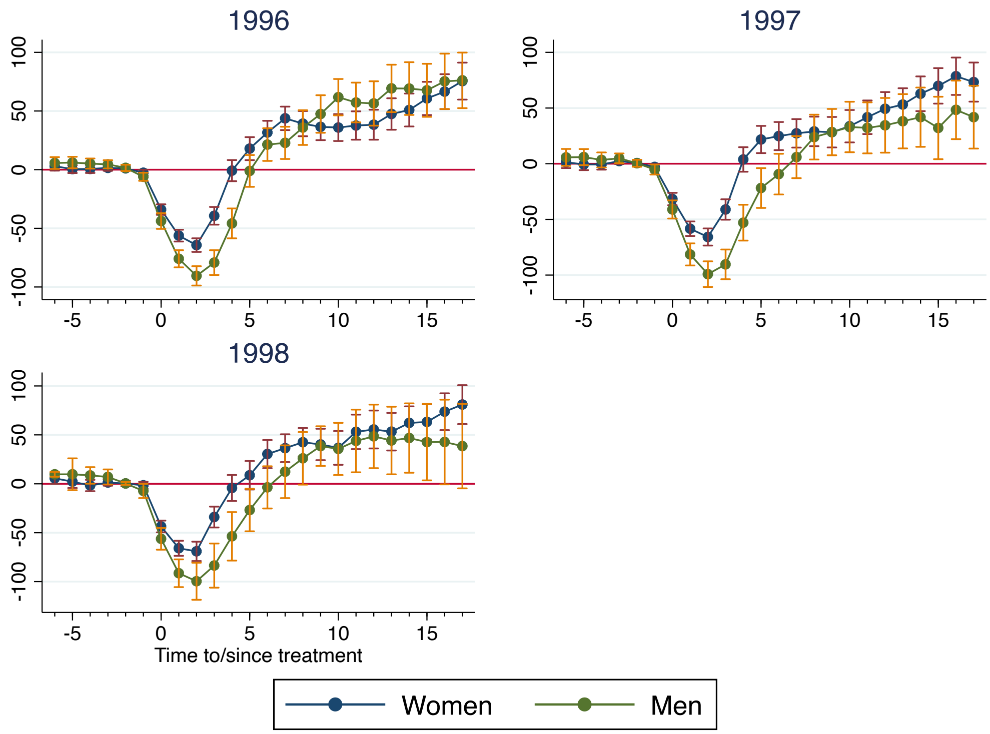

Figure 4: Difference-in-difference PSM estimates of annual gross wage earnings by gender. Measured in 1000s of SEK (2015 values). The dots represents the point estimates of the ATT and the associated 95% confidence intervals are shown by the error bars.

Age

There is a higher cost to studying in terms of foregone earnings for older individuals, and they also have less time to establish themselves in the labour market after their studies. Previous studies on the returns to adult education or a late post-secondary degree mainly focused on older individuals above the age of 29 (Hällsten, 2012; Stenberg et al., 2014; Stenberg & Westerlund, 2016). This study also included younger individuals between 20–29 which made it interesting to see whether the returns to education varied for different age groups. Therefore, I created two groups: one group contained individuals aged 20–29 at $t = 0$, while the other contained individuals aged 30–54. The groups were divided according to this criteria since the majority of the treated observations were aged 30 years or younger. However, additional analysis when the sample was divided into groups aged 20–35 and 36–55 (not reported in this study) did not have any major impacts on the result.

The results reported in Figure 5 show only minor differences between younger and older individuals in terms of annual earnings, and the estimated treatment effects follow a similar pattern as observed in the baseline model. Older individuals appeared to receive marginally higher returns to education than the young, especially in the 1997 cohort.

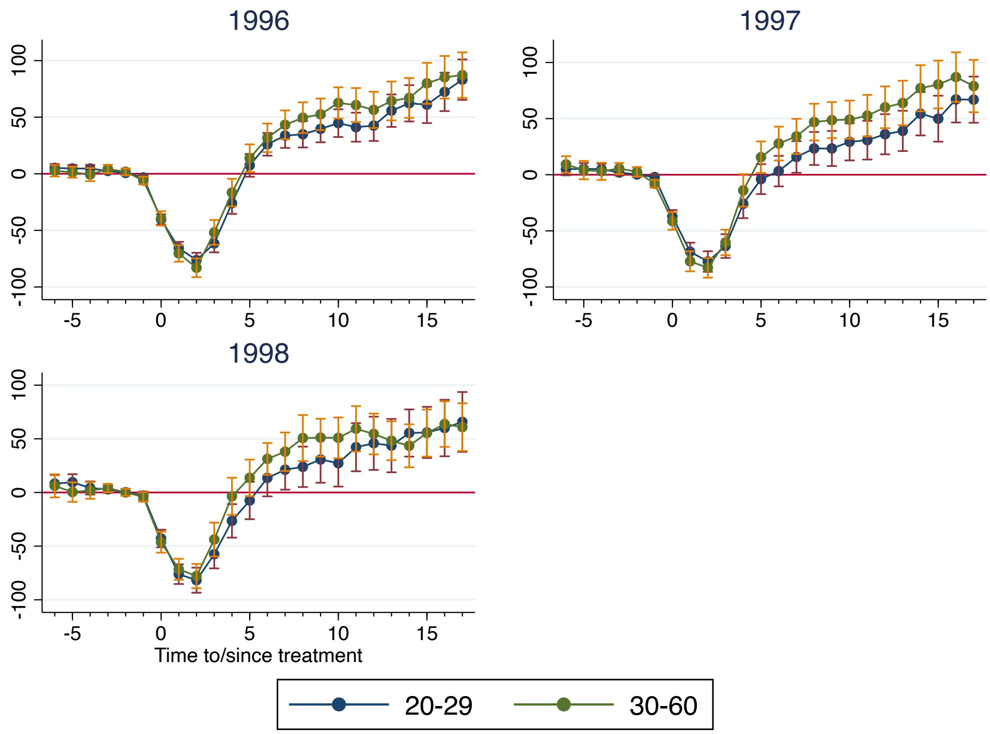

Figure 5: Difference-in-difference PSM estimates of annual gross wage earnings by age group, measured in 1000s of SEK (2015 values). The dots represents the point estimates of the ATT and the error bars represent the associated 95% confidence intervals.

Ability

Ability is probably one of the most influential factors in the decision to enrol in higher education and is also associated with economic outcomes; however, it is often difficult to observe. It is partly reflected by socioeconomic status (i.e. parental education) and previous labour market outcomes such as pre-treatment income and unemployment benefits. Military enlistment data was used to investigate whether there was still a difference in ability across the treated and matched comparisons that could affect the estimates. These data were available for men born between 1950–1974. However, several studies show strong associations between such tests and economic outcomes. I used controls for both cognitive and non-cognitive ability, since both dimensions of ability were found to explain economic outcomes and influence selection into treatment (Deming, 2017; Lindqvist & Vestman, 2011).

In theory, imperfect controls for ability should result in an upwards bias in the estimates, which means that the estimates of a model that includes better ability controls would give smaller ATTs. The point estimates for the cohorts of 1996 and 1997 displayed in Figure 6 are consistent with this understanding. The line representing ATT in wage earnings for the ability model was slightly below the baseline model, indicating a possible upwards bias in the treatment effects. The ATT in the alternative specification indicated an average of SEK 74,000 at $t = 17$ for the 1996 cohort compared to SEK 89,000 in the baseline model. The additional ability controls reduced the positive effect by SEK 15,000 at the end of the follow-up period, but the difference in ATT estimates at the income drop was SEK 11,000 at its lowest, indicating a greater loss in wage earnings when controls were added for ability. Nevertheless, the confidence intervals overlapped for all cohorts, and the differences in ATTs between the models were smaller than one standard deviation, indicating that the differences found were not statistically significant.

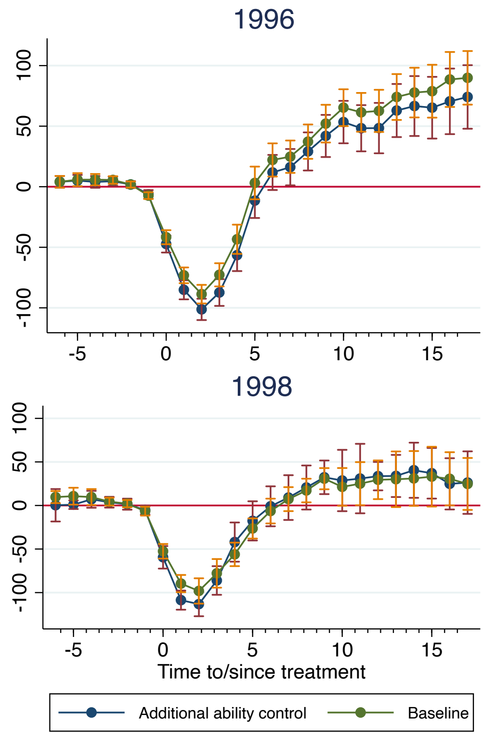

Figure 6: A comparison of the difference-in-difference PSM estimates of annual gross wage earnings in 1000s of SEK (2015 values) between the baseline model for men and a model with additional ability controls. The dots represents the point estimates of the ATT and the error bars represent the associated 95% confidence intervals.

5 Conclusions

The purpose of this study was to determine the effects of a return to university education among AE participants. Previous studies mainly focused on the economic returns derived from adult education or the effects of a late university degree. In contrast, this study contributes to the literature by analysing the effects of university education on those who used adult education as a second chance to complete upper secondary education.

Using difference-in-difference propensity score matching, I accounted for individual unobserved time-invariant characteristics and self-selection into treatment. The results suggest that there are large positive effects on annual gross earnings for those who studied at least one semester at university in cohorts that began their university studies between 1996–1998. In accordance with previous literature, the results highlight the importance of analysing the data over a longer time period, as the positive effects start emerging first after about five years, and steadily increase in the following years. However, the size of the point estimates was substantially larger than previous findings on the effects of a late university degree. For example, Stenberg and Westerlund (2016), found positive effects on earnings up to SEK 40,000 (19.6%) for women and SEK 20,000 (7.7%) for men.[16] To compare, the estimated ATT in this paper was SEK 75,000 at the end of the follow up period for the 1996 cohort. In terms of employment, the results indicate positive effects of 6 to 8 percentage points increased employment probability. Although Stenberg and Westerlund (2016) and Hällsten (2012) used a somewhat different set-up, they also found a effects of university education on employment.

One reason why the effects on earnings are larger in this study compared to similar studies might be because of different samples. This study looks at those who participated in AE while others restricted their sample to those who had at least three years of upper secondary education. Thus, the estimated results might overestimate the positive effect of university education with the effect of the third year in upper secondary school. However, supplementary analysis with controls for individuals who had at least three years of upper secondary schooling at the end of the follow-up period did not have any considerable impact on the estimates (not reported in this paper). This study also included younger individuals compared to Hällsten (2012) and Stenberg and Westerlund (2016).

Although no large differences were found between genders in the estimated ATTs in this study, we still observed that there was a larger relative gain for women (34.9%) than for men (27.7%). In addition, there were only minor differences between younger and older individuals. Additional controls for cognitive and non-cognitive ability from military enlistment data show that the main results are robust and that any bias related to ability is small or non-existent.

For future research, one could continue to study the effects of university studies among AE participants by considering multi-level treatment; this can be done by estimating a propensity score of multiple treatments, such as AE and university education. In addition, investigating heterogeneity in treatment effects between the fields of study at university could be an interesting contribution to the literature since the returns to education most likely vary across the different fields of education (Jepsen et al., 2014).

The results suggest that university education among adult education participants has substantial positive effects on economic outcomes in the long run. From a policy-making point of view, it is still expensive to provide AE and it is hard to determine the effect in equilibrium; however, these findings can still motivate investments into AE and is an important part of gaining further insights into the subtleties of AE.

[16] SEK 40,000 and SEK 20,000 corresponds to SEK 43,000 and SEK 21,800 when taking the inflation between 2011–2021 into account. The effects were estimated in 2011, 19 years after treatment.

References

Albrecht, J., Van den Berg, G. J. & Vroman, S. (2005). The knowledge lift: The Swedish adult education program that aimed to eliminate low worker skill levels. *Available at SSRN 673516*.

Askling, B., Christiansson, U. & Foss-Fridlizius, R. (2001). Livslångt lärande som idé och praktik i högskolan. Högskoleverket.

Austin, P. C. (2009). Balance diagnostics for comparing the distribution of baseline covariates between treatment groups in propensity-score matched samples. *Statistics in medicine*, 28(25), 3083–3107.

— (2011). An introduction to propensity score methods for reducing the effects of confounding in observational studies. *Multivariate behavioral research*, 46(3), 399–424.

Bennett, P., Blundell, R. W. & Salvanes, K. G. (2020). A Second Chance? Labor Market Returns to Adult Education Using School Reforms. *IZA Discussion Paper*.

Black, S. E., Devereux, P. J. & Salvanes, K. G. (2005). Why the apple doesn't fall far: Understanding intergenerational transmission of human capital. *American economic review*, 95(1), 437–449.

Blundell, R. & Costa Dias, M. (2000). Evaluation methods for non-experimental data. *Fiscal studies*, 21(4), 427–468.

Böckerman, P., Haapanen, M. & Jepsen, C. (2018). More skilled, better paid: labour-market returns to postsecondary vocational education. *Oxford Economic Papers*, 70(2), 485–508.

Caliendo, M. & Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of economic surveys*, 22(1), 31–72.

Cerqua, A., Urwin, P., Thomson, D. & Bibby, D. (2020). Evaluation of education and training impacts for the unemployed: Challenges of new data. *Labour Economics*, 67, 101907.

Dehejia, R. H. & Wahba, S. (1999). Causal effects in nonexperimental studies: Reevaluating the evaluation of training programs. *Journal of the American statistical Association*, 94(448), 1053–1062.

Deming, D. J. (2017). The growing importance of social skills in the labor market. *The Quarterly Journal of Economics*, 132(4), 1593–1640.

European Union, C. of the (2016). *Council Recommendation of 19 December 2016 on Upskilling Pathways: New Opportunities for Adults.*

Hällsten, M. (2012). Is it ever too late to study? The economic returns on late tertiary degrees in Sweden. *Economics of Education Review*, 31(1), 179–194.

Heckman, J., Ichimura, H., Smith, J. & Todd, P. (1998). Characterizing selection bias using experimental data. *Econometrica*, 66(5), 1017.

Holmlund, B., Liu, Q. & Nordström Skans, O. (2008). Mind the gap? Estimating the effects of postponing higher education. *Oxford Economic Papers*, 60(4), 683–710.

Jacobson, L., LaLonde, R. & Sullivan, D. G. (2005a). Estimating the returns to community college schooling for displaced workers. *Journal of Econometrics*, 125(1-2), 271–304.

— (2005b). The impact of community college retraining on older displaced workers: Should we teach old dogs new tricks? *ILR Review*, 58(3), 398–415.

Jenkins, A., Vignoles, A., Wolf, A. & Galindo-Rueda, F. (2003). The determinants and labour market effects of lifelong learning. *Applied economics*, 35(16), 1711–1721.

Jepsen, C., Troske, K. & Coomes, P. (2014). The labor-market returns to community college degrees, diplomas, and certificates. *Journal of Labor Economics*, 32(1), 95–121.

Lechner, M. & Wiehler, S. (2011). Kids or courses? Gender differences in the effects of active labor market policies. *Journal of Population Economics*, 24(3), 783–812.

Lindqvist, E. & Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: Evidence from the Swedish enlistment. *American Economic Journal: Applied Economics*, 3(1), 101–28.

Monks, J. (1997). The impact of college timing on earnings. *Economics of Education Review*, 16(4), 419–423.

Nybom, M. (2017). The distribution of lifetime earnings returns to college. *Journal of Labor Economics*, 35(4), 903–952.

OECD (2012). “Lifelong Learning and Adults”. *Education Today 2013: The OECD Perspective*. Paris: OECD Publishing.

— (2020). Education at a Glance. OECD iLibrary.

Rosenbaum, P. R. (2020). Modern algorithms for matching in observational studies. *Annual Review of Statistics and Its Application*, 7, 143–176.

Rosenbaum, P. R. & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1), 41–55.

SFS (1974). *Lag om arbetstagarens rätt till ledighet för utbildning [Employee’s Right to Educational Leave]*. SFS (1974:981).

Silles, M. A. (2007). Adult education and earnings: evidence from Britain. *Bulletin of Economic Research*, 59(4), 313–326.

Stenberg, A. (2019). *Kan utbildning för vuxna påverka jobbchanser och inkomster?* SOU (2019:48).

Stenberg, A., Luna, X. de & Westerlund, O. (2014). Does formal education for older workers increase earnings?—Evidence based on rich data and long-term follow-up. *Labour*, 28(2), 163–189.

Stenberg, A. & Westerlund, O. (2008). Does comprehensive education work for the long-term unemployed? *Labour Economics*, 15(1), 54–67.

Stenberg, A. & Westerlund, O. (2016). Flexibility at a cost–Should governments stimulate tertiary education for adults? *The Journal of the Economics of Ageing*, 7, 69–86.

Stuart, E. A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical science: a review journal of the Institute of Mathematical Statistics*, 25(1), 1.

A Appendix

Table A1: List of variables

Variable name	Description	Source
Gender (woman)	A dummy variable equal to one if the individual is female.	LISA[a]
Foreign background	A dummy variable equal to one if the individual is foreign-born or if both parents are foreign-born.	LISA
Age	Age at $t = 0$ unless stated otherwise.	LISA
Married	A dummy variable equal to one if the individual is married.	LISA
Moderately educated in 1994	A dummy variable equal to one if the individual has at least two years of upper secondary education in 1994.	LISA
Moderately or highly educated parents	A dummy variable equal to one if one parent has at least two years of upper secondary education.	The national education records
ALMP[b]	A continuous variable showing the amount of active labour market policies. Measured in 1000s of SEK.	LISA
Unemployment insurance (dummy)	A dummy variable equal to one if the unemployment insurance is larger than zero.	LISA
Unemployment insurance	A continuous variable showing the amount of unemployment insurance.	LISA
Sickness benefits (dummy)	A dummy variable equal to one if the sickness benefit is larger than zero.	LISA
Sickness benefits	A continuous variable showing the number of sickness benefits.	LISA
Parental leave (dummy)	A dummy variable equal to one if the parental leave is larger than zero.	LISA
Parental leave	A continuous variable showing the amount of parental leave.	LISA
Total number of children	The total number of children at home younger than 18 years old.	LISA

Table A1 continued from previous page

Variable name	Description	Source
Student income from grants or loans	A dummy variable equal to one if student income from grants or loans is larger than zero.	LISA
Student income from grants or loans	A continuous variable showing amount of student income from grants or loans.	LISA
Pos. Δearn (dummy)	A dummy variable equal to one if the change in earnings between $t = -2$ and $t = -1$ is positive.	
Δearnings, 6 variables	Six standardised variables measuring the pre-treatment change in earnings.	
Δunemp. benefits, 6 variables	Six standardised variables measuring the pre-treatment change in unemployment benefits.	
Avg. pre-treat.earnings	Average pre-treatment income between $t = -6$ and $t = -1$	
Avg. pre-treat employment	Average pre-treatment employment rate between $t = -6$ and $t = -1$	
Avg. pre-treat unemp. benefits	Average unemployment insurance between $t = -6$ and $t = -1$	
Avg. pre-treat student inc.	Average pre-treatment student income from grants or loan between $t = -6$ and $t = -1$	
Non-cognitive ability	Non-cognitive score from military enlistment test	Military enlistment data[c]
Cognitive ability	Cognitive ability from military enlistment test	Military enlistment data
Region of residence	Dummy variable indicating region of residence	RTB[d]
Stockholm/Uppsala	Stockholm, Uppsala	RTB
Mälardalen	Södermanland, Värmland, Örebro, Västmanland	RTB
Central-south	Östergötland, Jönköping, Kronoberg, Kalmar, Gotland, Skaraborg	RTB
South	Blekinge, Skåne, Kristiansand	RTB

Table A1 continued from previous page

Variable name	Description	Source
West	Halland, Västra Götaland, Älvsborg	RTB
Central-north	Dalarna, Gävleborg, Västernorrland, Jämtland	RTB
North	Västerbotten, Norrbotten	RTB

Note:

[a] Longitudinal integrated database for health insurance and labour market studies (LISA) from Statistics Sweden.

[b] Active labour market policy.

[c] Enlistment data from the Swedish Conscription Agency.

[d] The Total Population Register (RTB) from Statistics Sweden.

Table A2: Field of study by gender at t+4 (%)

	Gender		
Field of study	Men	Women	Total
General education	0	0	0
Teacher and education science	5	11	9
Humanities and arts	11	16	14
Social Sciences, business administration and law	25	25	25
Science, mathematics and statistics	22	11	16
Engineering, manufacturing and construction	26	5	13
Agriculture, forestry and veterinary	1	0	1
Health, welfare and social services	7	30	21
Services	2	1	2
Total	100	100	100

Note: Fields of study among the treated groups in the full sample.

Table A3: Probit model estimates of the propensity score; for the baseline model, used for estimating the effects on earnings, and for the extended model, used for estimating the effects on employment probabilities.

	1996		1997		1998	
	Baseline	Extended	Baseline	Extended	Baseline	Extended
Gender (woman)	-0.06	-0.07	-0.0002	-0.0007	0.23***	0.22***
	(-1.44)	(-1.47)	(-0.00)	(-0.01)	(3.47)	(3.35)
Foreign	-0.25**	-0.25**	-0.091	-0.093	-0.075	-0.074
background	(-3.03)	(-3.06)	(-1.01)	(-1.03)	(-0.69)	(-0.68)
Age	-0.03***	-0.03***	-0.04***	-0.05***	-0.03***	-0.03***
	(-7.06)	(-7.09)	(-8.63)	(-8.73)	(-5.17)	(-5.11)
Married	0.03	0.03	0.15*	0.15*	0.06	0.057
	(0.56)	(0.57)	(2.24)	(2.27)	(0.73)	(0.71)
Mod. educated in	0.839***	0.844***	0.704***	0.702***	0.781***	0.770***
1994	(-4.82)	(-4.81)	(-3.70)	(-3.68)	(-4.19)	(-4.12)
Mod. or high.	0.107*	0.107*	0.119*	0.122*	0.203**	0.202**
educated parents	(2.33)	(2.33)	(2.17)	(2.21)	(2.95)	(2.93)
ALMP	0.002	-0.0003	0.004**	0.003*	-0.001	-0.002
	(1.44)	(-0.24)	(2.91)	(2.00)	(-0.80)	(-1.24)
Unemp. insur.	0.11	-0.009	0.10	0.08	0.18*	0.10
(dummy)	(1.94)	(-0.14)	(1.82)	(0.96)	(2.09)	(1.06)
Unemp. insur.	-0.001	-0.004**	0.0009	-0.0006	-0.002	-0.003
	(-0.79)	(-2.62)	(0.62)	(-0.36)	(-0.87)	(-1.31)
Sickness benefits	-0.129	-0.125	-0.0359	-0.0382	-0.0384	-0.0378
(dummy)	(-1.70)	(-1.64)	(-0.44)	(-0.46)	(-0.37)	(-0.36)
Sickness benefits	0.002	0.002.	-0.0006	-0.0005	-0.001	-0.001
	(1.32)	(1.21)	(-0.26)	(-0.22)	(-0.49)	(-0.47)
Parental leave	0.00683	0.00625	0.0102	0.00687	-0.241**	-0.227*
(dummy)	(0.10)	(0.09)	(0.13)	(0.09)	(-2.58)	(-2.42)
Parental leave	-0.0009	-0.0008	-0.002.	-0.002	0.0003.	0.0002
	(-0.48)	(-0.42)	(-1.05)	(-1.00)	(0.15)	(0.10)
Total number of	-0.0161	-0.0209	0.0117	0.00842	0.0207	0.0186
children	(-0.62)	(-0.80)	(0.39)	(0.28)	(0.58)	(0.52)
Student inc.	0.523***	0.515***	0.875***	0.866***	0.761***	0.763***
(dummy)	(9.72)	(9.56)	(14.29)	(14.06)	(10.59)	(10.54)
Student inc.	0.0009***	0.001***	0.0005*	0.0005**	0.0003	0.0003
	(4.46)	(4.78)	(2.42)	(2.60)	(0.11)	(0.09)

Table A3 continued from previous page

	1996		1997		1998	
	Baseline	Extended	Baseline	Extended	Baseline	Extended
Avg. pre-treat.	0.002*	0.001*	0.001	0.001	-0.0001	-0.0002
earnings	(2.50)	(2.07)	(1.58)	(1.36)	(-0.22)	(-0.26)
Avg. pre-treat.	0.0798	0.104	0.253	0.262*	0.568***	0.558***
employment	(0.65)	(0.84)	(1.95)	(2.01)	(3.66)	(3.58)
Avg. pre-treat.	-0.003	0.003	-0.006*	-0.003	-0.001	0.001
unemp. benefits	(-1.50)	(1.15)	(-2.56)	(-0.92)	(-0.43)	(0.30)
Avg. pre-treat.	0.001	0.0008	-0.0002	-0.0003	0.0008	0.0008
student inc.	(1.38)	(1.17)	(-0.28)	(-0.45)	(1.46)	(1.57)
Pos. Δearn	0.21**	0.21**	-0.02	-0.03	0.04	0.03
(dummy)	(3.22)	(3.13)	(-0.30)	(-0.38)	(0.43)	(0.39)
Δearnings	0.14**	0.14**	0.11*	0.11*	-0.008	-0.001
-6 to -5	(2.93)	(2.91)	(2.06)	(2.00)	(-0.14)	(-0.03)
Δearnings	0.0904	0.0856	0.0491	0.0450	0.0222	0.0379
-5 to -4	(1.81)	(1.71)	(0.91)	(0.82)	(0.38)	(0.63)
Δearnings	0.0622	0.0522	-0.0445	-0.0480	-0.106	-0.0938
-4 to -3	(1.28)	(1.07)	(-0.87)	(-0.92)	(-1.84)	(-1.60)
Δearnings	-0.0498	-0.0491	-0.120*	-0.118*	-0.124*	-0.113
-3 to -2	(-1.10)	(-1.06)	(-2.48)	(-2.40)	(-2.04)	(-1.84)
Δearnings	-0.0828	-0.0749	-0.0594	-0.0569	-0.127	-0.134
-2 to -1	(-1.50)	(-1.36)	(-0.89)	(-0.85)	(-1.55)	(-1.62)
Δunemp. benefits		0.0441		-0.0672		0.130*
-6 to -5		(0.83)		(-1.20)		(1.96)
Δunemp. benefits		0.0445		0.0279		0.0800
-5 to -4		(0.84)		(0.47)		(1.22)
Δunemp. benefits		0.0820		0.00840		0.108
-4 to -3		(1.43)		(0.14)		(1.52)
Δunemp. benefits		0.173**		0.0621		0.135
-3 to -2		(2.91)		(0.94)		(1.68)
Δunemp. benefits		0.219***		0.113		0.0613
-2 to -1		(3.33)		(1.49)		(0.66)
Stockholm/Uppsala	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Mälardalen	0.289***	0.294***	0.0813	0.0836	0.168	0.169
	(4.28)	(4.35)	(0.96)	(0.99)	(1.74)	(1.75)

Table A3 continued from previous page

	1996		1997		1998	
	Baseline	Extended	Baseline	Extended	Baseline	Extended
Central-south	0.134*	0.141*	0.0759	0.0737	-0.0747	-0.0732
	(2.14)	(2.26)	(1.01)	(0.98)	(-0.75)	(-0.74)
South	-0.194**	-0.187**	0.0425	0.0412	-0.0212	-0.0156
	(-2.74)	(-2.64)	(0.55)	(0.53)	(-0.23)	(-0.16)
West	0.0545	0.0667	-0.0756	-0.0751	-0.0436	-0.0361
	(0.85)	(1.03)	(-0.98)	(-0.97)	(-0.51)	(-0.42)
Central-north	0.105	0.105	0.137	0.133	0.0907	0.0856
	(1.36)	(1.36)	(1.54)	(1.49)	(0.85)	(0.80)
North	0.469***	0.477***	0.421***	0.425***	-0.00856	-0.00450
	(6.11)	(6.20)	(4.64)	(4.68)	(-0.07)	(-0.04)
Observations	9091	9091	8752	8752	8752	8752

Note: Measured at $t = -1$ unless stated otherwise.

Abadie and Imbens robust standard errors in parentheses

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A4: Means and variances of the balancing test – 1996 cohort

	Unmatched			Matched		
Means	Treated	Untreated	Std.Dif	Treated	Untreated	Std.Dif
Gender (woman)	0.567	0.626	-0.119	0.567	0.558	0.018
Foreign background	0.054	0.079	-0.098	0.054	0.058	-0.017
Age	27.346	30.829	-0.519	27.346	27.230	0.017
Married	0.187	0.298	-0.261	0.187	0.192	-0.011
Mod. educated in 1994	0.974	0.995	-0.163	0.974	0.981	-0.056
Mod. or high. educated parent	0.762	0.646	0.257	0.762	0.773	-0.023
ALMP	7.420	7.729	-0.016	7.420	7.679	-0.014
Unemp. insur. (dummy)	0.594	0.521	0.147	0.594	0.601	-0.014
Unemp. insur.	15.798	16.945	-0.051	15.798	15.594	0.009
Sickness benefits (dummy)	0.083	0.137	-0.173	0.083	0.079	0.014
Sickness benefits	2.247	3.597	-0.085	2.247	2.869	-0.039
Parental leave (dummy)	0.138	0.223	-0.223	0.138	0.135	0.008
Parental leave	2.047	3.493	-0.117	2.047	1.877	0.014
Total number of children	0.631	0.928	-0.267	0.631	0.617	0.013
Student inc. (dummy)	0.801	0.503	0.659	0.801	0.806	-0.010
Student inc.	16.028	5.296	0.617	16.028	16.953	-0.053
Positive Δearn (dummy)	0.553	0.348	0.419	0.553	0.539	0.027
Δearn -6 to -5	0.112	0.040	0.171	0.112	0.117	-0.012
Δearn -5 to -4	0.017	-0.032	0.111	0.017	0.014	0.008
Δearn -4 to -3	-0.065	-0.105	0.083	-0.065	-0.077	0.026
Δearn -3 to -2	-0.145	-0.099	-0.089	-0.145	-0.141	-0.008
Δearn -2 to -1	-0.128	0.056	-0.329	-0.128	-0.113	-0.028
Avg. pre-treat. earnings	68.623	76.298	-0.156	68.623	65.149	0.071
Avg. pre-treat. employment	0.874	0.855	0.088	0.874	0.867	0.032
Avg. pre-treat. unemp. benefits	9.971	11.447	-0.111	9.971	9.879	0.007
Avg. pre-treat. student inc.	4.968	2.094	0.565	4.968	5.340	-0.073
Mälardalen	0.153	0.114	0.114	0.153	0.151	0.006
Central-south	0.182	0.174	0.020	0.182	0.175	0.017
South	0.101	0.150	-0.149	0.101	0.106	-0.014
West	0.151	0.164	-0.036	0.151	0.166	-0.040
Central-north	0.092	0.094	-0.004	0.092	0.094	-0.004
North	0.117	0.066	0.178	0.117	0.106	0.039

Table A4 continued from previous page

Variances	Unmatched			Matched		
	Treated	Untreated	Ratio	Treated	Untreated	Ratio
Gender (woman)	0.246	0.234	1.049	0.246	0.247	0.996
Foreign background	0.051	0.072	0.708	0.051	0.055	0.932
Age	32.680	57.482	0.569	32.680	36.501	0.895
Married	0.152	0.209	0.727	0.152	0.155	0.981
Mod. educated in 1994	0.025	0.005	4.633	0.025	0.018	1.363
Mod. or high. educated parent	0.182	0.229	0.793	0.182	0.176	1.033
ALMP	378.956	342.898	1.105	378.956	323.855	1.170
Unemp. insur. (dummy)	0.241	0.250	0.967	0.241	0.240	1.006
Unemp. insur.	403.760	601.850	0.671	403.760	413.970	0.975
Sickness benefits (dummy)	0.076	0.118	0.644	0.076	0.072	1.050
Sickness benefits	217.697	287.751	0.757	217.697	260.744	0.835
Parental leave (dummy)	0.119	0.173	0.687	0.119	0.117	1.019
Parental leave	118.332	189.093	0.626	118.332	83.500	1.417
Total number of children	1.099	1.364	0.806	1.099	1.041	1.055
Student inc. (dummy)	0.159	0.250	0.638	0.159	0.157	1.018
Student inc.	417.391	186.978	2.232	417.392	581.575	0.718
Positive Δearn (dummy)	0.247	0.227	1.090	0.247	0.249	0.996
Δearn -6 to -5	0.175	0.184	0.948	0.175	0.199	0.877
Δearn -5 to -4	0.191	0.204	0.936	0.191	0.213	0.896
Δearn -4 to -3	0.225	0.243	0.924	0.225	0.246	0.913
Δearn -3 to -2	0.264	0.276	0.956	0.264	0.273	0.967
Δearn -2 to -1	0.321	0.304	1.055	0.321	0.351	0.915
Avg. pre-treat. earnings	1877.799	2957.886	0.635	1877.799	2230.325	0.842
Avg. pre-treat. employment	0.040	0.051	0.785	0.040	0.043	0.937
Avg. pre-treat. unemp. benefits	151.549	204.420	0.741	151.549	136.517	1.110
Avg. pre-treat. student inc.	32.803	18.964	1.730	32.803	46.924	0.699
Mälardalen	0.130	0.101	1.281	0.130	0.128	1.012
Central-south	0.149	0.144	1.036	0.149	0.145	1.029
South	0.091	0.128	0.712	0.091	0.095	0.960
West	0.128	0.137	0.935	0.128	0.138	0.928
Central-north	0.084	0.085	0.989	0.084	0.085	0.990
North	0.103	0.062	1.679	0.103	0.095	1.092

Table A5: Means and variances of the balancing test – 1997 cohort

	Unmatched			Matched		
Means	Treated	Untreated	Std.Dif	Treated	Untreated	Std.Dif
Gender (woman)	0.594	0.626	-0.066	0.594	0.609	-0.030
Foreign background	0.069	0.079	-0.035	0.069	0.076	-0.024
Age	27.992	31.839	-0.583	27.992	27.881	0.017
Married	0.218	0.304	-0.197	0.218	0.218	0.000
Mod. educated in 1994	0.975	0.993	-0.149	0.975	0.980	-0.044
Parent mod. or highly educated	0.775	0.645	0.289	0.775	0.772	0.007
ALMP	9.841	9.358	0.022	9.841	10.288	-0.020
Unemp. insur. (dummy)	0.594	0.482	0.226	0.594	0.617	-0.046
Unemp. insur.	18.154	18.097	0.002	18.154	18.681	-0.021
Sickness benefits (dummy)	0.102	0.159	-0.171	0.102	0.111	-0.029
Sickness benefits	1.549	3.236	-0.136	1.549	1.533	0.001
Parental leave (dummy)	0.174	0.264	-0.217	0.174	0.167	0.017
Parental leave	2.684	4.630	-0.145	2.684	2.696	-0.001
Total number of children	0.731	0.944	-0.184	0.731	0.742	-0.009
Student inc.	0.648	0.214	0.974	0.648	0.654	-0.014
Student inc.	15.942	4.005	0.655	15.943	15.707	0.013
Positive Δearn (dummy)	0.332	0.255	0.170	0.332	0.316	0.035
Δearn -6 to -5	0.045	-0.033	0.166	0.045	0.034	0.024
Δearn -5 to -4	-0.074	-0.105	0.062	-0.074	-0.064	-0.021
Δearn -4 to -3	-0.120	-0.099	-0.040	-0.120	-0.138	0.034
Δearn -3 to -2	-0.112	0.056	-0.298	-0.112	-0.114	0.003
Δearn -2 to -1	0.058	0.115	-0.111	0.058	0.078	-0.038
Avg. pre-treat. earnings	65.346	79.080	-0.263	65.346	63.772	0.030
Avg. pre-treat. employment	0.838	0.837	0.004	0.838	0.836	0.007
Avg. pre-treat. unemp. benefits	12.262	14.159	-0.124	12.262	12.655	-0.026
Avg. pre-treat. student inc.	6.379	2.585	0.568	6.379	6.334	0.007
Mälardalen	0.108	0.113	-0.016	0.108	0.105	0.011
Central-south	0.164	0.173	-0.022	0.164	0.170	-0.017
South	0.151	0.148	0.007	0.151	0.146	0.014
West	0.140	0.167	-0.073	0.140	0.158	-0.048
Central-north	0.107	0.092	0.048	0.107	0.100	0.021
North	0.112	0.066	0.162	0.112	0.106	0.019

Table A5 continued from previous page

Variances	Unmatched sample			Matched sample		
	Treated	Untreated	Ratio	Treated	Untreated	Ratio
Gender (woman)	0.242	0.234	1.032	0.242	0.238	1.014
Foreign background	0.065	0.072	0.893	0.065	0.070	0.923
Age	29.497	57.526	0.513	29.497	31.648	0.932
Married	0.171	0.212	0.807	0.171	0.171	1.001
Mod. educated in 1994	0.025	0.007	3.701	0.025	0.019	1.271
Parent mod. or highly educated	0.175	0.229	0.763	0.175	0.176	0.992
ALMP	542.182	442.479	1.225	542.182	481.578	1.126
Unemp. insur. (dummy)	0.242	0.250	0.967	0.242	0.237	1.021
Unemp. insur.	497.077	711.740	0.698	497.077	536.815	0.926
Sickness benefits (dummy)	0.091	0.134	0.684	0.091	0.099	0.924
Sickness benefits	79.625	228.382	0.349	79.625	78.434	1.015
Parental leave (dummy)	0.144	0.194	0.742	0.144	0.139	1.035
Parental leave	123.694	238.223	0.519	123.694	120.647	1.025
Total number of children	1.316	1.350	0.975	1.316	1.221	1.077
Student inc.	0.228	0.168	1.357	0.228	0.226	1.010
Student inc.	482.004	182.665	2.639	482.004	597.230	0.807
Positive Δearn (dummy)	0.222	0.190	1.170	0.222	0.216	1.027
Δearn -6 to -5	0.234	0.205	1.143	0.234	0.228	1.024
Δearn -5 to -4	0.240	0.244	0.986	0.240	0.254	0.948
Δearn -4 to -3	0.283	0.277	1.024	0.283	0.266	1.066
Δearn -3 to -2	0.329	0.304	1.081	0.329	0.335	0.981
Δearn -2 to -1	0.282	0.241	1.171	0.282	0.301	0.937
Avg. pre-treat. earnings	2136.281	3297.092	0.648	2136.281	2252.323	0.948
Avg. pre-treat. employment	0.054	0.060	0.898	0.054	0.056	0.962
Avg. pre-treat. unemp. benefits	192.370	273.447	0.703	192.370	182.011	1.057
Avg. pre-treat. student inc.	59.353	30.016	1.978	59.353	68.013	0.873
Mälardalen	0.097	0.101	0.962	0.097	0.094	1.029
Central-south	0.137	0.143	0.962	0.137	0.142	0.971
South	0.128	0.126	1.015	0.128	0.124	1.030
West	0.121	0.139	0.870	0.121	0.133	0.909
Central-north	0.095	0.084	1.139	0.095	0.090	1.057
North	0.099	0.061	1.619	0.099	0.095	1.046

Table A6: Means and variances of the balancing test – 1998 cohort

	Unmatched			Matched		
Means	Treated	Untreated	Std.Dif	Treated	Untreated	Std.Dif
Gender (woman)	0.711	0.626	0.182	0.711	0.719	-0.016
Foreign background	0.073	0.078	-0.021	0.073	0.067	0.023
Age	28.954	32.843	-0.572	28.954	29.115	-0.024
Married	0.204	0.311	-0.248	0.204	0.204	0.000
Mod. educated in 1994	0.957	0.992	-0.221	0.957	0.957	0.000
Parent mod. or highly educated	0.805	0.645	0.365	0.805	0.787	0.042
ALMP	6.942	8.959	-0.103	6.942	8.152	-0.062
Unemp. insur. (dummy)	0.578	0.437	0.283	0.578	0.573	0.009
Unemp. insur.	18.168	17.201	0.039	18.168	17.185	0.040
Sickness benefits (dummy)	0.100	0.146	-0.139	0.100	0.100	0.000
Sickness benefits	1.726	3.174	-0.114	1.726	2.101	-0.030
Parental leave (dummy)	0.164	0.280	-0.281	0.164	0.156	0.020
Parental leave	3.598	4.989	-0.096	3.598	3.629	-0.002
Total number of children	0.717	0.965	-0.222	0.717	0.727	-0.009
Student inc.	0.587	0.167	0.959	0.587	0.599	-0.028
Student inc.	12.207	2.683	0.552	12.207	11.869	0.020
Positive Δearn (dummy)	0.441	0.297	0.300	0.441	0.418	0.048
Δearn -6 to -5	-0.101	-0.105	0.009	-0.101	-0.098	-0.006
Δearn -5 to -4	-0.047	-0.099	0.098	-0.047	-0.050	0.006
Δearn -4 to -3	-0.006	0.056	-0.110	-0.006	-0.016	0.018
Δearn -3 to -2	0.044	0.115	-0.140	0.044	-0.003	0.093
Δearn -2 to -1	-0.036	0.058	-0.196	-0.036	-0.005	-0.064
Avg. pre-treat. earnings	66.933	82.715	-0.290	66.933	64.085	0.052
Avg. pre-treat. employment	0.852	0.822	0.120	0.852	0.839	0.054
Avg. pre-treat. unemp. benefits	15.087	16.348	-0.075	15.087	14.867	0.013
Avg. pre-treat. student inc.	6.823	2.833	0.525	6.823	7.080	-0.034
Mälardalen	0.140	0.113	0.082	0.140	0.142	-0.007
Central-south	0.112	0.134	-0.065	0.112	0.128	-0.046
South	0.140	0.148	-0.023	0.140	0.124	0.045
West	0.191	0.204	-0.032	0.191	0.201	-0.025
Central-north	0.103	0.091	0.041	0.103	0.087	0.054
North	0.061	0.065	-0.018	0.061	0.055	0.025

Table A6 continued from previous page

Variances	Unmatched			Matched		
	Treated	Untreated	Ratio	Treated	Untreated	Ratio
Gender (woman)	0.206	0.234	0.879	0.206	0.202	1.018
Foreign background	0.068	0.072	0.938	0.068	0.062	1.086
Age	34.928	57.554	0.607	34.928	37.047	0.943
Married	0.163	0.214	0.758	0.163	0.162	1.002
Mod. educated in 1994	0.041	0.008	5.101	0.041	0.041	1.002
Parent mod. or highly educated	0.157	0.229	0.687	0.157	0.168	0.937
ALMP	339.230	432.914	0.784	339.230	376.815	0.900
Unemp. insur. (dummy)	0.245	0.246	0.995	0.245	0.245	0.999
Unemp. insur.	512.729	711.384	0.721	512.729	514.969	0.996
Sickness benefits (dummy)	0.091	0.125	0.726	0.091	0.090	1.002
Sickness benefits	95.555	225.158	0.424	95.555	119.700	0.798
Parental leave (dummy)	0.138	0.202	0.683	0.138	0.132	1.045
Parental leave	169.184	255.004	0.663	169.184	211.429	0.800
Total number of children	1.167	1.340	0.871	1.167	1.142	1.022
Student inc.	0.243	0.139	1.746	0.243	0.240	1.011
Student inc.	468.430	127.663	3.669	468.430	506.250	0.925
Positive Δearn (dummy)	0.247	0.209	1.183	0.247	0.244	1.015
Δearn -6 to -5	0.220	0.244	0.903	0.220	0.280	0.786
Δearn -5 to -4	0.286	0.277	1.034	0.286	0.282	1.014
Δearn -4 to -3	0.329	0.304	1.080	0.329	0.326	1.007
Δearn -3 to -2	0.271	0.241	1.125	0.271	0.296	0.918
Δearn -2 to -1	0.255	0.213	1.196	0.255	0.308	0.829
Avg. pre-treat. earnings	2133.994	3773.927	0.565	2133.994	2358.429	0.905
Avg. pre-treat. employment	0.055	0.068	0.817	0.055	0.054	1.035
Avg. pre-treat. unemp. benefits	236.635	333.927	0.709	236.635	240.166	0.985
Avg. pre-treat. student inc.	75.525	39.939	1.891	75.525	125.245	0.603
Mälardalen	0.121	0.100	1.206	0.121	0.122	0.989
Central-south	0.100	0.116	0.864	0.100	0.111	0.898
South	0.121	0.126	0.956	0.121	0.109	1.111
West	0.155	0.162	0.956	0.155	0.161	0.965
Central-north	0.093	0.083	1.120	0.093	0.080	1.164
North	0.057	0.061	0.941	0.057	0.052	1.106

B Appendix

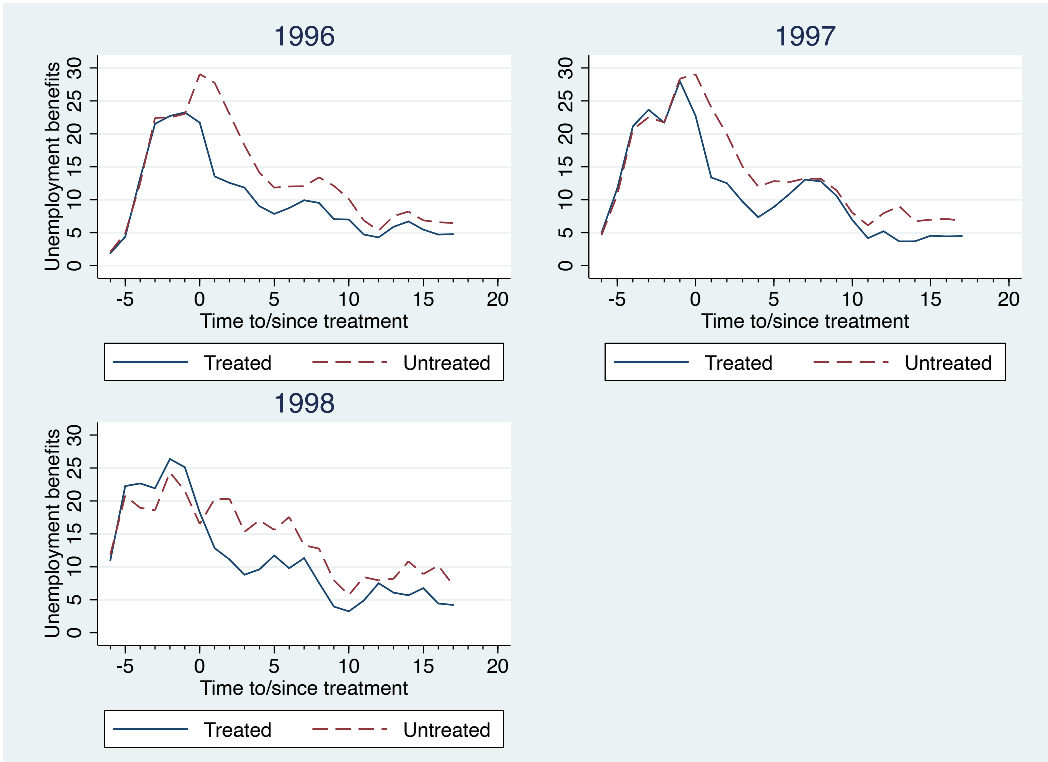

Figure B1: Average unemployment benefits among the treated and matched comparisons.

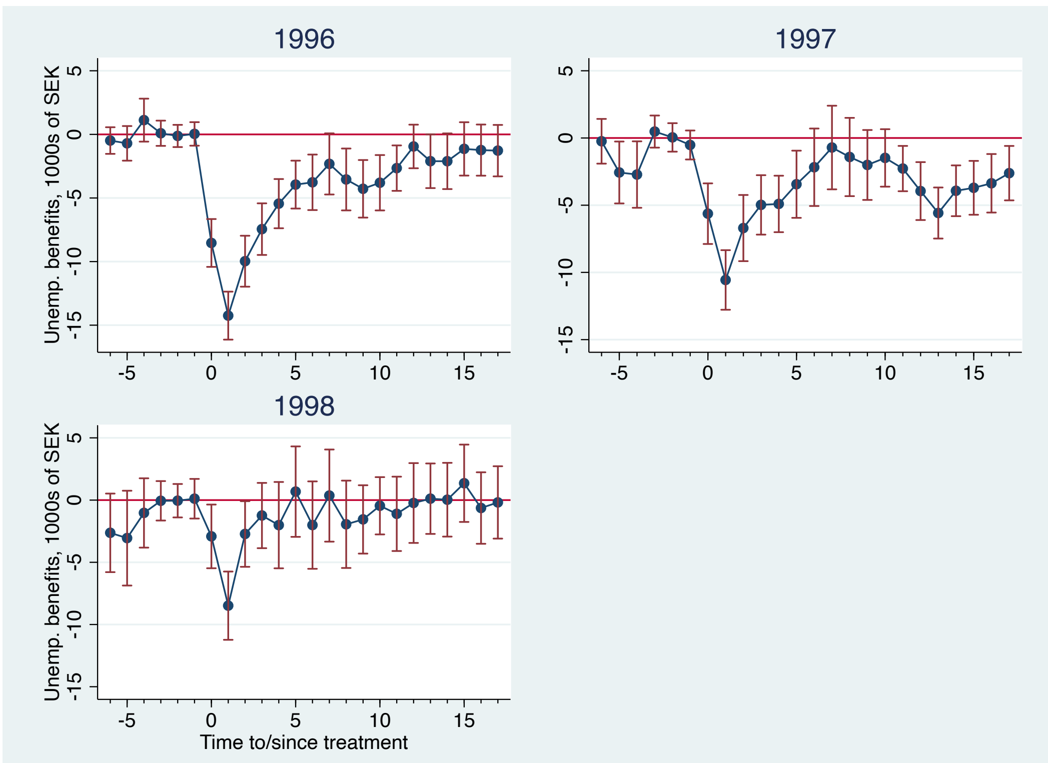

Figure B2: Difference-in-difference PSM estimates for unemployment benefits.

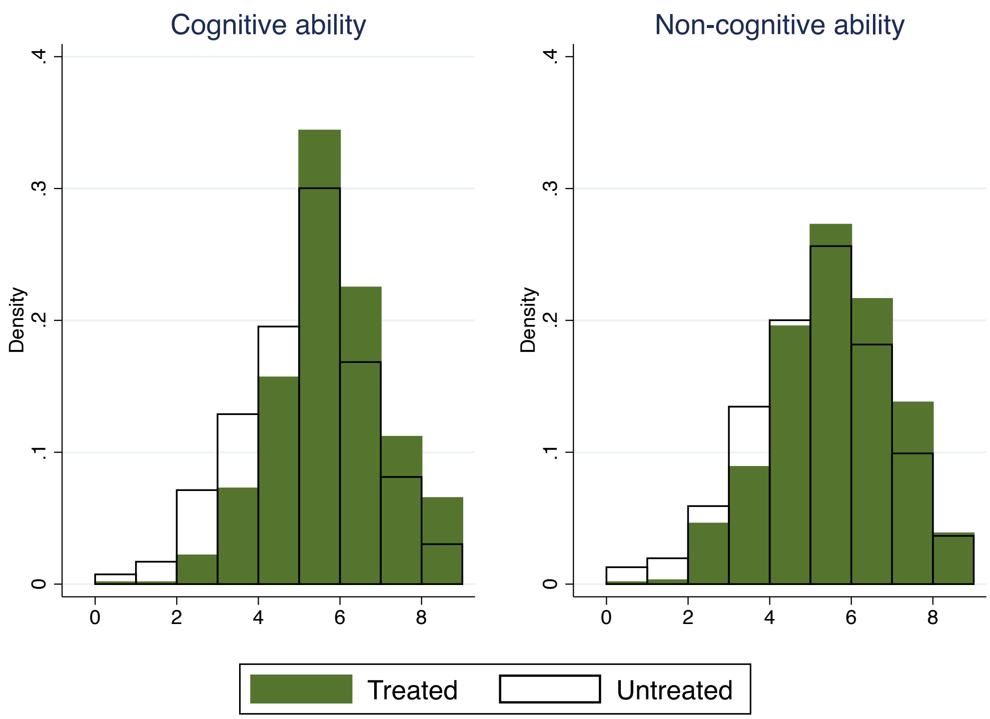

Figure B3: Distribution of cognitive and non-cognitive abilities for the treated and untreated groups in the full sample. Data is only available for men born between 1950–1970. $N^{treated} = 675$, $N^{untreated}$=2,653

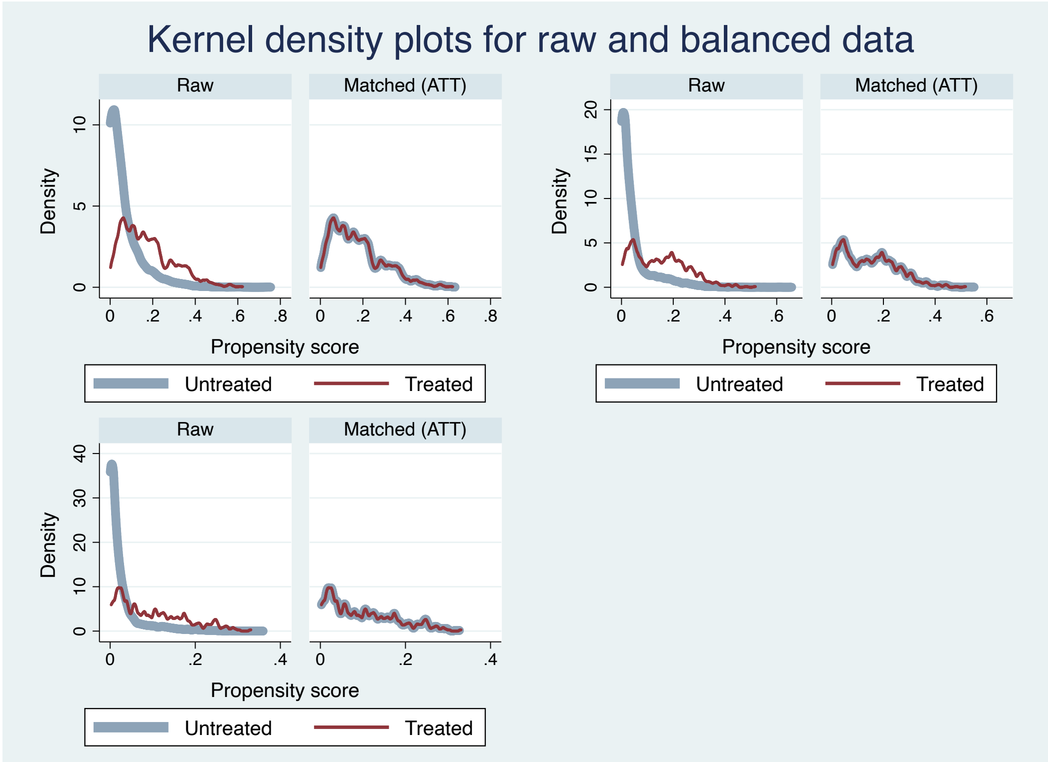

Figure B4: Density plots of propensity scores for the treated and untreated groups in unmatched and matched data.

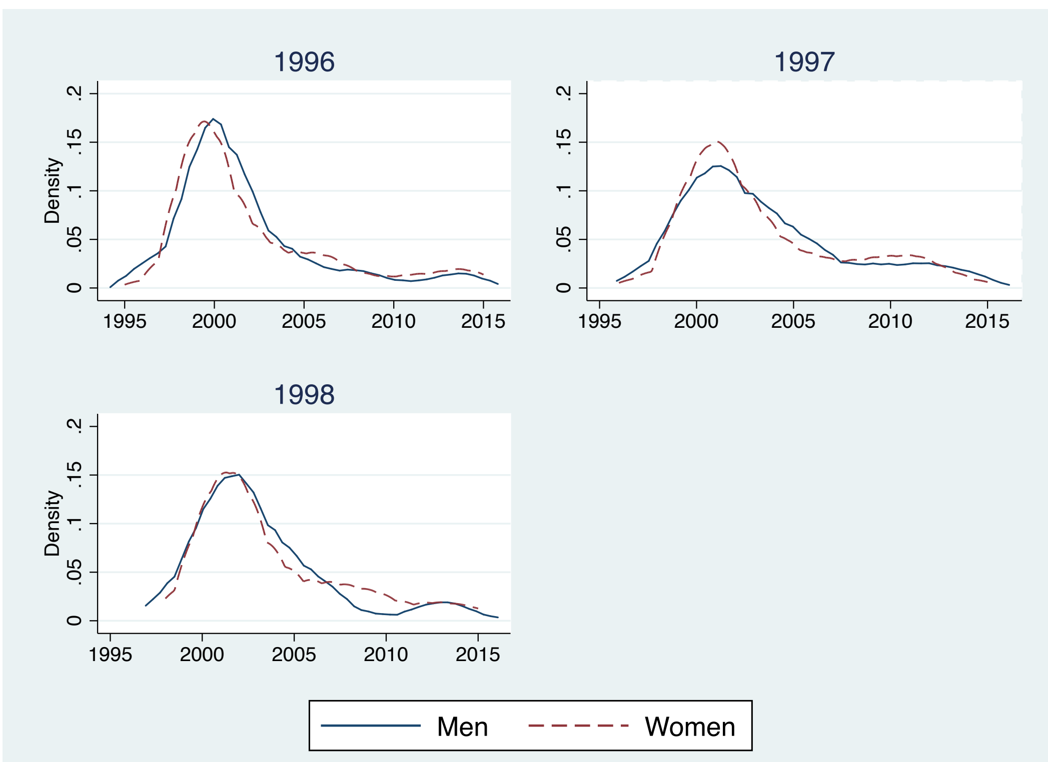

Figure B5: Graduation year among the treated by gender. Kernel density estimates for graduation year of the highest level of education among the treated measured at the end of the follow-up period (2015).