Swedish Institute for Social Research (SOFI)

Stockholm University

WORKING PAPER 2/2015

FLEXIBILITY AT A COST – SHOULD GOVERNMENTS STIMULATE TERTIARY EDUCATION FOR ADULTS?

by

Anders Stenberg and Olle Westerlund

Flexibility at a Cost – Should Governments Stimulate Tertiary Education for Adults? *

by

Anders Stenberg♣

Olle Westerlund⊗

May 8, 2015

Abstract: Most OECD countries experience high unemployment rates and declining growth in higher educational attainment. An often suggested government policy is therefore to allocate resources towards formal schooling for adults. However, returns on such investments are uncertain and the foregone earnings are potentially large. We use Swedish population register data from 1982 to 2011 to estimate average long run earnings returns on higher education for 29- to 55-year-olds who enrolled 1992-1993. We find substantial positive estimates, but these only fully emerge after approximately ten years. Nevertheless, calculations indicate that the benefits for society exceed the costs also under fairly pessimistic assumptions.

Keywords: Adult education, Human capital, Earnings

JEL classification: H30, H52, I20, J24, O30

* The authors gratefully acknowledge the financial support of The Swedish Research Council through the Ageing and Living Condition (ALC) program and the Bank of Sweden Tercentenary Foundation.
♣ SOFI, Stockholm University, SE-106 91 Stockholm, Sweden. Linneaus University, IZA, Bonn. anders.stenberg@sofi.su.se
⊗ Department of Economics, Umeå University, SE-901 87 Umeå, Sweden, olle.westerlund@econ.umu.se

1 Introduction

The increase in educational attainment across OECD countries is slowing down, with the current generation predicted to just barely surpass the educational attainment of the preceding generation, while in the US this rate is actually decreasing (Goldin and Katz 2008, OECD 2012). As it is widely believed that education is a key factor for economic growth, upgrading one's skills during later stages of the working life may become more important. Neumark et al. (2011) project that, in the coming decade, adults aged 30 to 54 in the US will represent 20 to 25 percent of the influx of workers with at least a bachelor's degree. Although the optimal timing of some educational investments may indeed occur rather late in life, e.g., to mitigate negative effects of unforeseen changes in employment prospects, it is also true that individuals' adult schooling intentions could face formidable obstacles due to opportunity costs and/or credit constraints. For this reason, the OECD and the EU have long encouraged governments to stimulate adult education to adjust workers' skills to technical changes (OECD 1998, 2001, EU 2000, 2001). Relatedly, Pissarides (2011) recommends regular education for adults as a counter-cyclical public employment policy tool because the opportunity costs of education decrease during economic downturns.[1] However, there are few countries where such policies have been applied on a large scale, and the research in economics on formal adult education is quite limited.

The aim of this paper is to assess the long-term effects of post-secondary adult education on earnings. We use Swedish population register data on education and annual earnings from 1982 to 2011 to analyze a sample of first-time enrollees aged 29 to 55 when registering for higher education in 1992-1993. Average treatment effect on the treated (ATT) is estimated using propensity score matching based on data that are unusually rich in detail and with a difference-in-differences set-up that accounts for individual time invariant (fixed) unobserved characteristics correlated with earnings. With regard to time varying unobserved characteristics, we estimate models under different assumptions and check the stability of the results. To this end, we exploit information for the years prior to education on earnings dynamics, transitions in the labor force status and changes in social security payments

[1] A recession may also hamper or delay the returns on the investments (Kahn 2010, Oreopoulos et al. 2012).

related to unemployment, sick-leave, social welfare, early retirement and parental leave. To check for potential ability bias, our models are re-estimated for individuals aged 29 to 37 (41 for males) adding measures of grade point averages from school and, for males, military enlistment test scores on cognitive and non-cognitive skills. The main implications of our results are robust. Overall, due to potential endogeneity in course lengths, our analyses focus on estimates of ATT where treatment is defined as assignment to treatment.

Earlier studies of adults in education (henceforth, AE) have primarily been concerned with community college enrollees in the US aged below 30. A reference point for these articles is Kane and Rouse (1995) who, for individuals of typical ages when attending education, found a year of completed studies at a community college to yield wage returns of approximately 5 percent for males and between 6 and 9 percent for females. The estimates were relatively similar for annual earnings. Light (1995) and Monks (1997) analyze individuals returning to college after a few years of work experience. Both studies find that wage gains from education decline with the age of completion, but results in Light (1995) and Leigh and Gill (1997) indicate that the wage returns became similar to those of the younger graduates about five years after completion.

For education among older individuals, Jacobson, Lalonde and Sullivan (2003, 2005a, 2005b) study workers aged 25 to 59 who were laid-off between 1990 and 1994 in Washington State, 15 percent of whom registered at community colleges. Individual fixed effects estimates of quarterly earnings from 1987 to 2000 indicate that a year of studies increased earnings by 7 to 9 percent for males and by 10 to 13 percent for females. The benefits appear sufficient to cover the total costs, although the calculations are sensitive to assumptions regarding the foregone production value. Jepsen et al. (2014) report results for students completing community college certificates, diplomas or associate degrees in Kentucky. The comparison group consists of enrollees who did not accomplish the respective awards (approximately 70 percent). The reported returns for diplomas and associate degrees imply estimates that, when compared with Jacobson et al., are similar in size or considerably higher. With regard to European data, there are several Swedish studies of low-skilled adults in upper secondary education who are ineligible for higher education at the outset. Stenberg and Westerlund (2008) find a payoff of between 15 and 20 percent for the long-term unemployed, but the size of the effect was inflated by the low average earnings of the sample. For a broadly defined sample, aged 24-43 at the time of first registration in AE, Stenberg (2011) reported a 2.3 and 5.1 percent payoff on annual earnings of one year of completed studies for males and females, respectively. Calculations indicate that the benefits just about cover the total costs to society. Stenberg et al. (2014) study an older sample, aged 42 to 55, and find no earnings effects for males but positive effects for females, although insufficient to cover costs to society.[2] Thus, while these studies question the rationale for governments to stimulate AE, at least based on pecuniary arguments, the US-based studies have reported more beneficial effects. There are several potential explanations. First, returns on AE may be higher in the US due to a wider dispersion of wages and/or skills (e.g., IALS 2000, Harjes 2007). Second, the institutional set-up in Sweden encourages AE participation, potentially attracting individuals with lower expected returns on average. Third, the skill levels of the participants in the respective studies differed. Evidence of job polarization from both US and Europe suggests that the demand for medium-skilled workers to perform routine tasks has decreased. This implies lower returns to education for low-skilled workers upgrading to medium-skilled status compared with making the transition from medium- to high-skilled.[3] To the best of our knowledge, Hällsten (2012) is the only previous study to analyze European data on older individuals investing in tertiary level education. For a sample of Swedish workers 30 years or older between 1985 and 2003, conditioned on degree completion and on stable employment after treatment, the estimated returns were around 2 percent per year of studies on log income (including social insurance transfers from parental leave and sick leave). The present study differs from Hällsten (2012) in several respects. First, the samples studied are restricted by two pre-treatment conditions as all individuals in our samples are eligible for tertiary level schooling, and that no-one has been registered in education in 15 years prior to 1992. Second, treatment is defined as enrolment, which means treated include all

[2] In Sweden, both policy debate and research have been focused on low-skilled individuals for whom municipalities are by law obligated to offer adult education (Albrecht et al. 2009, Stenberg 2011). Schwerdt et al. (2011) analyze individuals aged 20 to 60 in Switzerland who were subject to a randomly distributed voucher system. Participants completed, on average, 42 hours of courses with no significant effects on average labor market outcomes one year later.

[3] On US data, see Autor Levy and Murnane (2003) and Autor, Katz and Kearney (2008); on data from the UK and Europe, see Goos and Manning (2007), Goos et al. (2009), Spitz-Oener (2006), Dustmann et al. (2009).

older students in tertiary level education without conditioning on graduation. Third; our outcome variable is labor earnings measured (differenced) in absolute terms. This allows us to retain the full samples of treated except if outcomes are missing. Fourth; since labor earnings are not directly affected by social insurance transfers, we may assess the social returns by providing estimates which proxy the effects on productivity as well as opportunity costs in terms of foregone earnings.

The contributions of this paper are the estimation of the long-term earnings ATT of post-secondary education for middle aged and older adults. We also assess economic benefits and costs from the perspective of society (GDP). In summary, the results imply that the identification of positive returns requires a follow-up period of at least ten years after enrolment. Our preferred estimates imply positive effects on gross wage earnings of approximately 5.5 percent for males and 10 percent for females. It is difficult to compare the percentages reported with estimates from the returns to schooling literature because of relatively high drop-out rates and the fact that the estimates are partly driven by low earners increasing their employment hours. Back-of-the-envelope calculations indicate that the benefits for society exceed the costs even under pessimistic assumptions. The paper is organized as follows. Section 2 contains a discussion on enrolment in AE and explains why estimates conditioned on specific amounts of completed AE are problematic. Section 3 contains a description of the institutional framework for AE in Sweden, of the data used and of the sample we study. The empirical model and issues regarding the identification of causality are outlined in Section 4. The results are presented in Section 5 and set in relation to costs in Section 6. A summary and discussion concludes the paper.

2 Theoretical consideration

2.1 Who enrolls and why

Individuals are assumed to enroll in education if the expected net benefits are positive, either because AE increases future labor market options or because a consumption motive with primarily non-monetary gains exists. Ability reduces costs in effort, and may, together with lower age, increase the expected net returns. Within early representations of human capital models, the implication is that educational investments should be made early in life to allow as much time as possible to reap the rewards (Becker 1962). Other models acknowledge that continuously updated information affects dynamic optimizing behavior, which may also imply investments in formal education later in life (e.g., Comay et al. 1973, Cameron and Heckman 2001, Stange 2012). Economic fluctuations constitute one such source of information that potentially compels adults to enroll in education (Heckman and Urzua 2008, Ikenaga and Kawaguchi 2013, Pissarides 2011). More generally, various forms of changing conditions/new information could induce AE enrolment decisions by altering the expected returns and/or opportunity costs.[4] Changes which are expected may also affect educational investments, in particular child-rearing responsibilities for females. Thus, the decision to have a child may make AE enrolment less likely (Lechner and Wiehler 2011). In other cases, mothers' time constraints are relaxed as children grow older (e.g., begin day care), and it has been documented that AE enrolment is relatively frequent before returning to the labor market (Stenberg 2007, p15). It follows that the mechanisms behind AE enrollment may partly differ for males and females just like in the case of conventional schooling (Becker 1985, 1991; Mincer & Polachek 1974), e.g., due to differences in household responsibilities, the likelihood of career interruptions or because of gendered labor markets. Our analyses in the empirical section are conducted separately for males and females, as we carefully explore information on the dynamics of individual's pre-treatment social and economic conditions.

2.2 Estimating the returns to education for adults

Under conventional assumptions, enrolment in education is linked with costs in effort and indirect opportunity costs in the form of foregone earnings. These costs make the length of treatment endogenous, with ambiguous effects on the relation between ability and completed AE. One may expect individuals with both low motivation and high motivation (or ability) to be dispersed among early dropouts (lack of effort or high job-finding rate) as well as among those completing many credits (dismal job-seeking prospects or high study performance). In our data, the correlation coefficient between

[4] Specific changes that may be important include health issues (possibly work-related), borrowing constraints (Wallace and Ihnen 1975), relative wages (Killingsworth 1982, Monks 1998, Weiss 1971), skills depreciation (Ben-Porath 1967), preferences (Altonji 1993).

completed AE and GPA is relatively low, at .17 for males and .06 for females. In the population as a whole, the correlation between GPA and completed years of education is .55. The low correlations in our sample likely reflect that the direct and indirect (opportunity) costs of education are, on average, higher for 25- to 55-year-olds compared with conventional college age enrollees. This may hamper the possibilities for estimating the returns associated with specific amounts of completed AE.

This problem is well known. Jacobson et al. (2005a, p8) and Jepsen et al. (2014, p105) acknowledge that a shock factor may bias estimates of accomplished studies.[5] Authors have often faced difficulties with interpreting results that have indicated non-monotonic or decreasing returns in accomplished AE. With respect to Swedish data, Stenberg et al. (2014) report a U-shaped pattern in the returns per unit of completed studies.[6] For the US, the earnings returns reported in Jacobson et al. (2003, p69, 2005a, p289) display substantial non-linearity. For both males and females completing 11 to 20 community college credits, about one semester of studies, estimates are on par with (or above) those pertaining to groups completing 21 to 40, 41 to 75 and 75 credits or more. Jepsen et al. (2014) condition on achieved awards (certificates, diplomas or associate's degrees). Although the required credits for awards vary within each category, our reading of the estimates implies non-linear returns, with higher absolute returns for diplomas (requiring approximately 1.5 years of studies) than for associate's degrees (approximately 2.5 years).

In the present study, we generally define treatment as *assignment to treatment*, which is not affected by the endogenous length of treatment (e.g., Heckman et al 1998). However, to gauge our estimates, we also set them in relation to the average amount of completed AE. In a case of randomized assignment, it is reasonable to assume that the control group members would have completed an equal average amount of AE had they been enrolled. This is less obvious with non-experimental data, and

[5] For example, assume that a group of individuals drop out early from education after receiving wage offers from the higher end of the conditional distribution. For this subsample of treated individuals, who were lucky in the draw of job-offers, there is no meaningful control group even if assignment is randomized. Any estimator would risk reflecting reverse causality.

[6] With access to detailed data on the weeks of completed AE at the upper secondary level, the authors find a U-shaped pattern in the earnings returns for females aged 42 to 55. There was a strong positive association between AE participation and earnings for those who completed only a few credits (less than ten weeks). This positive relation gradually faded and, more consistent with conventional theory, individuals with more than six months of completed AE experienced a monotonically increasing earnings payoff of AE.

therefore, the credibility of our assessments is checked in several ways. Section 4 contains a detailed discussion.

3 Institutional background, data and sample

3.1 Institutional set-up

In 1992-1993, 22 percent of the individuals who registered in higher education in Sweden were aged 29 to 55. Several factors explain this relatively high fraction of adults. Education is free of charge, most often publicly provided, and since 1974, employees have had a legal right to take a study leave and then be reinstated by the employer with similar working conditions. Students are also eligible to apply for study allowances, which are roughly equal to €1000 per month, of which one-third is to be repaid within a period of 25 years (with some exceptions). On the supply side, tertiary education institutes (*Universitet* or *Högskola*) exist in about 30 cities in a population of 9 million, commonly offering study programs with one or two years of required courses before a major subject is chosen. Each course is assigned a number of credits that may yield earnings returns even if a major or a study program is not completed.

Figure 1 shows unemployment rates and attendance in higher education in Sweden since 1977. In the late 1980s, unemployment rates were low before the GDP decreased for three years in succession starting in 1990/1991. A series of events led to an overall dip in aggregate demand with unemployment rates between 1990 and 1993 soaring from 2.1 to 11.3 percent (ILO definition). The prices of real estate dropped sharply and a budget deficit mounted (for details on the Swedish downturn, see Englund 1999). During this period of severe cuts in public spending, the Swedish government nevertheless gave priority to substantial investments in education. The economic slump plausibly enhanced the demand for education, as the opportunity costs in terms of average foregone earnings decreased, but the expansion of the higher education sector continued as the unemployment rates began to decline.

3.2 Data and sample

Data originate from various registers of the Swedish population administered by Statistics Sweden. The information includes annual labor earnings from 1982 to 2011 and a wide variety of individual characteristics from 1990 onwards, notably family situation and transfers related to social insurance systems such as unemployment, sick-leave, social welfare, early retirement and parental leave.[7] We also have access to records of registrations in higher education from 1977 and yearly reports of the highest level of completed education. In addition, for the younger part of our sample, there is information on GPAs from schools and military enlistment test scores on cognitive and non-cognitive abilities.

The sample of our analyses is restricted to individuals who have three years of upper secondary schooling as their highest completed education in 1991. Thus, everyone in the sample is eligible for tertiary education.[8] To generate a clean sample, where both treated and untreated individuals have repeatedly rejected AE enrolment prior to 1992, we exclude all individuals who were registered in higher education at some point between 1977 and 1991. Individuals registered in AE 1992-1993 are defined as treated individuals. We further limit the sample to individuals who are aged 29 to 55 in 1992 (born between 1937 and 1963). The lower age limit is set to 29 as the expansion of higher education, which followed in the 1990s and onwards, makes younger cohorts difficult to assess because groups that were untreated in 1992-1993 often enrolled in higher education at a later point in time. This is still the case with the remaining sample, but at levels that are less problematic.

Figure 2 displays trajectories of the average annual earnings from 1982 to 2011. The earnings prior to AE are higher for untreated individuals, but the relation is reversed in the latter part of our observation window. At the end of the period, earnings tend to drop as the oldest cohorts in our sample re-

[7] Parents are entitled to 13 months of benefits, which are mostly utilized during the first two years after the birth of the child. The benefits correspond to 80 percent of the annual labor earnings in the previous year.

[8] Our priority is to avoid misclassifications. Upper secondary school in Sweden is conducted within different programs that vary in time length from one to three years, but only the three-year programs always fulfill the eligibility requirements for tertiary education. When restricting our sample to this group, the number of eligible individuals is underreported mainly because the classification system in the 1990s did not include course credits from AE at the upper secondary level.

tire from the labor force. In the empirical analysis, earnings prior to AE are used as control variables. However, among females, part-time work is relatively common, and annual earnings may be an imprecise measure of labor market productivity. Therefore, we also construct a variable reflecting the highest earnings rank recorded between 1982 and 1990, controlling for age, to potentially capture inherent labor market productivity.

Table 1 presents the descriptive averages of our samples of treated and untreated individuals for a selection of variables (a full list of variables available in the Appendix, Tables A2 and A3). The treated individuals are, on average, younger than the untreated. The gap in age is for males five years (38.6 vs. 43.7) and for females three years (37.5 vs. 40.4), explaining some of the differences in sample means (approximately half of the pre-AE earnings gap remains when adjusting for age). The treated individuals more frequently receive transfers, and they also experience greater changes in earnings and transfers post-1990. For example, the incidence of unemployment benefits increased between 1990 and 1992 among the treated from 8 percent to 24 percent and from 3 percent to 7 percent among the untreated. Negative changes, e.g., decreasing earnings or increasing levels of transfers, tend, on average, to be more pertinent among the treated, and especially so among males. In contrast, the treated individuals have higher GPAs, but the differences only correspond to five percentile ranks. The GPA is recorded at age 15 at the end of comprehensive school and is available for individuals born 1955 or later. Grades of attainment are set from one (lowest) to five (highest). In addition, for males born in 1951 or later, we also have access to military enlistment test scores on cognitive and non-cognitive skills, which were conducted at age 18 or 19. In Table 1, these scores are given as averages, but in the empirical estimations, we actually explore four measures of cognitive skills - induction, verbal, spatial, technical comprehension - and two measures of non-cognitive skills - leadership suitability and psychological capability (see Lindqvist and Vestman 2011 for details). The treated individuals score, on average, significantly higher than the untreated for all the aforementioned traits except spatial skills (or "metal folding", *p*-value .391). The last rows of Table 1 show statistics on AE attendance and completion. The average number of years of completed AE is 1.4 years for treated males and 1.9 years for treated females. It is further noted that a minority of the treated completed three years of higher education. Possible explanations include high opportunity costs of AE and the "cherry picking" of courses for labor market reasons or for consumption. Meanwhile, some of the untreated enrolled AE. Although these shares are small, with modest records of completed AE, they pose a potential problem for our estimations. We assess their impact by a) excluding them from the sample (violating the conditional independence assumption) and b) by taking into account completed AE among the non-treated individuals to adjust our estimates expressed in percentage terms per year of AE (see footnote 14). These approaches do not change the main implications derived from our results.

4 Empirical model and estimation

4.1 Difference-in-differences propensity score matching

The impact of AE on annual earnings is estimated by employing difference-in-differences propensity score matching (e.g., Rosenbaum and Rubin 1983, Smith and Todd 2005). The approach assumes selection on observables into treatment (enrolment in AE). Below, we present the employed estimator and motivate why the selection on observables assumption is reasonable in our context.

Let Y_{it} be the annual earnings of an individual i in year t and $\Delta Y_{it+} = Y_{it+} - Y_{it-}$ denote the change in earnings when comparing before treatment (t-) and after treatment (t+). For each individual, there are two potential outcomes - ΔY_1 and ΔY_0 - in case of treatment and non-treatment (footindex i and t are suppressed to simplify). Let D = 1 denote the actually treated individuals and zero denote the actually untreated individuals. The average treatment effect on the treated (ATT) is:

$$\Delta \mathrm{Y}_{\mathrm{ATT}} = [\Delta Y_1 | D = 1] - [\Delta Y_0 | D = 1]$$

The potential (counterfactual) outcome ΔY_0 is not observed for treated individuals (D = 1) and must be estimated from observations of the untreated. To this end, the treated and untreated are matched on the conditional ex-ante probability of AE enrolment P(X), which is derived from Probit model estimates (the propensity score). X is a vector of covariates observed prior to treatment (defined below). The parameter of interest $\widehat{\Delta\mathrm{Y}_{\mathrm{ATT}}}$ compares the mean of changes in earnings over time between AE individuals and the matched comparisons:

$$\widehat{\Delta\mathrm{Y}_{\mathrm{ATT}}} = [\Delta Y_1 | D = 1, P(X)] - [\Delta Y_0 | D = 0, P(X)]$$

Under certain assumptions, $\widehat{\Delta\mathrm{Y}_{\mathrm{ATT}}}$ provides an unbiased estimate of the ATT also if there are heterogeneous treatment effects by avoiding extrapolations outside common support in the data.[9] Table A.1 in the Appendix displays the Probit model estimates of the propensity score, and Tables A.2 and A.3 present balancing tests of the matched samples.[10] The weights ascribed by the matching procedure should balance the treated and their matched comparisons on all observable variables. The tests confirm that equality between the treated and their matched comparison groups cannot be rejected.[11]

To give $\widehat{\Delta\mathrm{Y}_{\mathrm{ATT}}}$ a causal interpretation, one must assume the following: (1) the estimated propensity score is strictly positive; (2) individuals in the treatment group do not influence the outcome of those in the non-treatment group (the no interference assumption or the stable unit treatment value assumption, SUTVA); and (3) conditional on our covariates, unobserved differences between the treated and the controls that influence the decisions to enroll in AE are uncorrelated with future earnings streams. Under assumptions (1) to (3), any systematic differences in future average earnings between the treated individuals and the matched comparison groups are only influenced by AE.

The credibility of the key assumption (3) hinges, first, on the quality of the data (for detailed discussions on this topic, see Heckman et al. 1999, Biewen et al. 2014). In our model, the differenced set-up takes into account the unobserved time invariant individual characteristics (fixed effects) that affect annual earnings. The outcome variable for an individual in year t+ is defined as the difference such that $\Delta Y_{it+} = [Y_{it+} - (Y_{i1990}+Y_{i1989}+Y_{i1988})/3]$. Note that ΔY_{it+} does not consider earnings in 1991, i.e., the

[9] This presumes $(\Delta Y_1 - \Delta Y_0) \perp D \mid P(X)$. For a causal interpretation, see assumptions (1) to (3).

[10] Irrelevant covariates in the Probit step may increase bias and/or variance of matching estimators. We follow de Luna et al. (2011) and exclude variables associated with *p*-values above .2 if not essential for the balancing of the samples.

[11] One-to-one matching with replacement yields the least bias but at the cost of precision. We estimated both one-to-one and four-to-one matching with estimates, on average, differing by only +/- .1 percentage point. The presented balancing tests and results presented in Section 5 are based on four-to-one-matching.

year prior to the first enrolment in AE. The treated and matched comparisons are always balanced with respect to the educational track completed at the three-year upper secondary level (7 categories), employment sector (7 categories), age (30 categories), number of children at home (6), age of children (6), marital status (3), foreign born (2), dummies for region of residence (25 categories), and regional employment rate. Importantly, the samples are also balanced in terms of pre-treatment annual earnings and earnings trajectories (levels from 1982 to 1990) and, from 1990, family disposable income, five different types of social insurance benefits related to unemployment insurance (UI), parental leave, sick-leave, early retirement pensions and social welfare, applying both dummy variables (incidence of the various benefits) and continuous measures of the amounts in SEK. This will constitute our reduced model specification. Controlling for this rich set of covariates and accounting for unobserved individual fixed effects, our main concern regarding any potential remaining bias regards unobserved dynamic factors and unobserved ability. We discuss, in turn, three possible confounding factors: 1) ability, 2) events/changes prior to AE with temporary or permanent consequences and 3) decisions on labor supply vs. child-rearing (special case of change prior to AE).

4.2 Ability as a confounding factor

Ability is widely believed to affect the return to education and the selection into education. To assess if it is a confounding factor not reflected through previous earnings and other retrospective information on labor market outcomes, we use the available measures of ability as described in Section 3 for robustness checks. These include the GPAs for individuals born in 1955 or later and military enlistment test scores for cognitive and non-cognitive skills for males born in 1951 or later.

4.3 Temporary vs. permanent changes prior to adult education

An often inferred criticism of the difference-in-differences estimator is that a temporary (Ashenfelter's) dip in earnings among the treated may precede the participation in programs and cause an upward bias in estimates of earnings outcomes (Ashenfelter 1978). For this reason, the pre-enrolment earnings in ΔY_{it+} do not include earnings in 1991, and our reduced model specification presented above does not include control variables observed post-1990. However, a drop in earnings prior to AE enrolment may signal a shift that is permanent rather than temporary, and thus convey different implications (e.g., Heckman and Smith 1999, Heckman et al. 1999). To gauge the importance of these opposing hypotheses, we employ an extended model specification that includes an additional set of 27 control variables, reflecting changes in annual earnings, UI benefit payments and labor force status in 1990-1991 as well as the changes in social insurance benefits in 1990-1991 and 1991-1992.[12]

By construction, the reduced model does not condition on the variables reflecting changes in 1990-1991 and in 1991-1992 (Tables A.2 and A.3). Therefore, these variables are, in some cases, unbalanced when the reduced model is employed.[13] If changes post-1990 reflect temporary fluctuations, our reduced model estimates are unbiased. If, instead, they reflect changes that persist and have long-term effects on average earnings, e.g., following layoffs (Eliason and Storrie 2006, Davis and von Wachter 2011), the reduced model estimates will be biased downward, as the treated tend to experience negative changes prior to enrolment. By comparing the extended model results with the estimates of the reduced model, our analyses encompass two alternative scenarios – one where changes prior to AE reflect temporary events (reduced model) and one where they are assumed to have permanent consequences (extended model). Because the true mechanisms are unknown, the overall implications of the results hinge crucially on the robustness of the estimated outcomes.

4.4 Labor supply decisions and childrearing

For females, in particular, giving birth to a child and subsequent childrearing responsibilities may be a change that has long-term consequences for both AE enrolment decisions and labor supply. First, AE could be postponed or altogether rejected by the decision to have a child (Lechner and Wiehler 2011). This would imply higher fertility rates among the matched comparisons and could yield upward-

[12] When including benefit payments received in 1992 among the covariates, we must assume that AE does not cause increased sick-leave, early retirement, social welfare or parental leave.
[13] With the reduced model, unbalanced variables for matched samples include annual earnings from 1991 and the transition from employment to unemployment for 1990-1991. For males, samples are unbalanced with respect to changes in sick-leave benefits, which are higher for the treated 1991 and 1992. The samples of females are primarily unbalanced on indicators of family situation.

biased estimates. To address this issue, we present results pertaining to females with two children pre-enrolment. This should most often signal completed fertility due to a strong two-child norm. Second, females who are already responsible for childrearing may experience relaxed time constraints as their children grow older, which may induce an increase in labor supply. If females enroll in AE before an already planned increase in labor supply (or as part of that plan), it is possible that they are matched with comparisons who have no such intentions, thus generating a risk of upward-biased estimates. To some degree, we get around this issue by presenting estimates for females without children. For estimates pertaining to females without children, differences in fertility levels post- enrolment may be used as an indication of remaining bias. However, when we account for bias due to childbirth decisions (conditioning on two children), a weakness of the set-up is that estimates may still be biased due to the suggested mechanism linked with relaxed time constraints. Again, the robustness of our estimates is important for the overall implications of the results.

The two described mechanisms regarding childrearing that may confound our estimates are conceptually the same for active labor market programs (ALMP). Thus, it is interesting that Heckman and Smith (1999) only find modest bias in their non-experimental estimates of ALMP pertaining to females. They suspect that the bias remaining is due to family factors that were not measured well in their data. Our data include controls for both the number of children, their ages, incidence of parental leave benefits and the amount of parental leave benefits. Our extended model adds controls for changes 1990-1991 and 1991-1992 with respect to the level of parental leave, the incidence of parental leave benefits and indicator variables of whether there were changes in the number of children at home aged 0 to 3 years.

5 Results

5.1 Main results

Figure 3 shows the average earnings trajectories of the treated individuals and their untreated matched comparison groups. The earnings are well balanced from 1982 to 1990. Treated individuals thereafter show lower earnings while in AE before recovering and surpassing those of their matched comparisons. The gaps between the earnings trajectories from 1994 basically correspond to our difference-in-differences estimates, displayed in Figure 4. The point estimates in Figure 4 are significantly positive for males from 2001 and for females from 1997 and onward. This relatively sluggish recovery, foremost for males, is a pattern that has been observed in both the US and Sweden (e.g., Jacobson et al. 2003, p80, Stenberg 2011, p1266), and it accentuates the importance of analyzing long time-series. Above each set of results, we also present averages of the estimates in SEK for 2002 to 2011, the last ten years of observation. This average is almost SEK 20,000 for males (€2,200) and just above SEK 40,000 for females (we refer to reduced model estimates unless stated otherwise). In terms of percentages, it represents, for males, on average, 7.7 percent of annual earnings of matched comparisons and 19.6 percent for females. In Figure 4, we report these percentages divided by the average years of completed AE, yielding 5.6 percent for males and 10.3 percent for females.[14] Although the estimates expressed in percentage terms are interesting in their own right, we emphasize that they are difficult to compare with conventional returns to school estimates. This is due to relatively high drop-out rates and that results tend to be inflated by low pre AE earnings, for whom there is more leverage.

Figure 5 illustrates results for samples divided into a younger half, aged 29 to 41 in 1992, and an older half, aged 42 to 55. The estimates are positive throughout for the younger sample. The last ten years of the reduced model estimates yield on average SEK 28,735 (6.2 percent) for males and SEK 46,402 (9.6 percent) for females. For the older samples, there is a gradual increase in point estimates, which for males only becomes (insignificantly) positive towards the end of the observation window. This is partly explained by growing proportions reaching the age span of 62 to 67, where the transition into retirement is most frequent (in 2011, the youngest cohort of this sample is 61-years-old). For older males, the estimates in Figure 5 correspond to 4.3 percent of the average earnings. For older females, despite lower absolute estimates, the percentage earnings gain is relatively high at 15 percent. This is explained by their lower average earnings and by the fact that they complete about 40 percent less AE

[14] If we adjust (subtract) for completed AE among the untreated, percentages are 6.2 for males and 12.2 for females.

than their younger counterparts. In a companion paper that specifically analyzes the timing of retirement, we find statistically significant results indicating that both male and female AE participants retire about half a year later than their matched comparisons (Stenberg and Westerlund 2013).[15]

5.2 Robustness checks

The first of three sources of potential bias discussed in Section 4 addresses ability. If our estimates are flawed by ability bias, one would expect the inclusion of ability control variable(s) to generate some systematic change in the results. For males born in 1951 or later, we add military enlistment test scores, whereas the GPAs are added for females born in 1955 or later. Figure 6 displays reduced model results with ability controls (grey) and without these controls (black). The differences in the point estimates as we include or exclude the ability measures are shown separately, with the average divergence (2002 to 2011) at .30 percent (males) and -.06 percent (females). When using the extended model, the corresponding changes are -.17 percent (males) and .02 percent (females). Thus, the evidence implies that ability bias does not constitute a major concern.[16]

Next, we examine the potential failure to control for permanent effects of changes prior to AE. Figures 4 and 5 contain reduced model estimates, which ignore any changes post-1990, together with extended model estimates that take changes into account (as discussed in Section 4.3). Estimates for males are more sensitive to the choice between specifications (5.6 and 6.8 percent vs. for females 10.3 and 11.0 percent). This may reflect differences in the mechanisms behind decisions to enroll in AE related to the life-cycle patterns affecting human capital endowments, career choices and/or family responsibilities. However, overall, the different models yield qualitatively similar implications, thus indicating that the confounding of temporary vs. permanent effects of changes is not of first order importance for the results.

[15] In contrast, Stenberg et al. (2012) find no such effects for AE at upper secondary level.
[16] We obtain close to identical results for males if we include the GPA as a covariate, but one then needs to exclude 18 percent of the sample used for estimates in Figure 7, since GPAs only available for those born in 1955 or later.

Third, our estimated returns to AE may be confounded by 1) females' decisions to give birth to a child or by 2) the decreasing amount of time devoted to childrearing as children grow older. For the first of these issues, we restrict the sample of females to those with two children at home in 1991, intended as a signal of completed fertility, which is also the case for 92 percent of the treated and 89 percent of the comparisons. It is then less likely that the comparison group comprises a share of females who decide to give birth to a child rather than enroll AE. The average estimate for 2002 to 2011 is SEK 41,942 (9.6 percent), and if born in 1951 or later, on averages SEK 45,319 (8.6 percent). With respect to the second issue, childrearing may become less time consuming as children grow older (e.g., start daycare or school). One may then suspect a decision to enroll AE coincides with decisions to increase labor supply, with the latter potentially confounding our estimates of ATT. To avoid this, we condition the female sample to be without children at home in 1991. The estimates obtained are then, on average, SEK 26,130 (7.3 percent) for all females, and SEK 31,543 (7.3 percent) for those born later than 1951, implying that the qualitative results hold.[17] The slight divergence compared with the results in Figures 4 and 5 may reflect heterogeneous effects or indicate that we now better control for labor supply decisions. However, decisions to increase labor supply (and the probability to receive a job offer) could also reflect the very effects of AE in which we are interested.

To examine employment probabilities, we define employed as a binary variable taking the value of one each year if annual earnings exceed SEK 100,000 (€11,000). Figure 7 show estimates indicating higher employment probabilities for AE individuals. The averages for 2002 to 2011 are 2.4 percent for males and 4.4 percent for females, i.e., the divergence in results between gender groups is similar to Figure 4.[18] With access to information on both wages and earnings, Jacobson et al. (2005a, 2005b) report for their sample of laid-off workers that two-thirds of the earnings returns reflect hours worked and one-third consist of increased wages. As a reference, in his survey of the returns to school litera-

[17] The number of children at home is about .10 higher among the comparisons, but the incidence of children 0- to 3- years-old is higher among those treated in both 2000 and 2005 (significant at a ten percent level).
[18] With a different set-up, Hällsten (2012) found similarly defined probabilities to increase by about 10 percent. If we also condition on individuals to have completed at least three years of AE, our estimates become for males and females respectively, 6.3 and 8.7 percent (reduced model) and 9.5 and 9.9 percent (extended model).

ture, Card (1999) reports the reverse relationship. Unfortunately, data are not available for us to determine these shares in our estimates.[19]

5.3 Heterogeneous effects

With access to measures of ability, it is also possible to examine whether estimates vary across the ability distribution. To do so, we continue to employ cognitive and non-cognitive test scores for males, and GPAs for females. Earlier studies have reported strongly heterogeneous returns to higher education in the overall population, which decrease with the level of the GPA (Öckert 2012) or with the level of military enlistment test scores (Nybom 2014). Figure 8 presents the results separately for those above median ability (black line) and those below median ability (grey line). The results are relatively similar with no strong indication of differences between these groups. For males, until 2004, the effects tend actually to be stronger for the treated with low ability scores. This is driven by the fact that low ability individuals also have lower earnings prior to AE. When we repeat the analyses with earnings in 1990 conditioned to be above SEK 100,000 (average 1988 to 1990), the difference between the groups is close to zero. The implication of these results is similar to the marginal returns to schooling estimated by Nybom (2014), which are relatively constant across the ability distribution. Hence, a possible interpretation is that AE participants constitute marginal enrollees, as they did not complete higher education at a young age.

For completeness, an overview of separate estimates for different amounts of completed higher education is provided in Table 2. It shows that the absolute estimates tend to increase monotonically with completed AE (average for 2002 to 2011), but decrease if expressed as percentage returns per year of studies. One may note that the estimated payoff is very high, 17 percent, for females who completed at least one but less than two years of AE.[20] At the other end of the scale, one might expect

[19] As a rudimentary way to obtain estimates reflecting wage increases, we followed Antelius and Björklund (2000) and excluded individuals with below SEK 100,000 (nominal values) in pre-program earnings (1988-1990). The results indicate that 35 to 40 percent of the estimates would reflect hours of employment, except for reduced model estimates for males, which imply 75 percent. This raises a number of issues that we, due to lack of appropriate data, must leave for future research.

[20] Hypothetically, as the registers only classify education as years completed, these individuals could have completed “almost two years” rather than one year of AE. The percentage returns would then be reduced by half.

high estimates associated with longer educations (Manski and Pepper 2000). It is not the case here. Referring to the discussion in Section 2, evaluating earnings effects of a given unit of completed AE may violate the conditional independence assumption if course lengths are endogenous (e.g., dropouts caused by arrival of high wage offers). In sum, we attach a limited analytical value to these estimates and emphasize that the returns to specific amounts of AE should be interpreted with caution.

6 The costs and benefits to society

To assess the policy implications of our estimations, we approximate the benefits and costs from society's perspective by calculating the internal rate of return. As this exercise relies on several untestable assumptions, we check the sensitivity to alterations of the most important assumptions. Overall, we find the benefits of AE to exceed the costs by a substantial amount, even if one applies fairly pessimistic assumptions.

The baseline assumptions are the following: *i*) benefits are based on reduced model point estimates, as illustrated in Figure 4, and the estimate obtained for 2011 is assumed fixed at that level until all individuals have retired; *ii*) the base year is 1992 and the internal rate of return is defined as the discount rate which sets costs equal to the present value of the future benefits; *iii*) all individuals retire at age 65; *iv*) the estimated earnings return reflects an increase in production, with no crowding out effects (Dahlberg and Forslund, 2005); *v)* the deadweight loss is 50 percent[21]; *vi)* public insurance payments are not affected by AE; *vii*) both earnings returns and foregone earnings are multiplied by 1.4, thereby taking into account payroll taxes (approximately 40 percent) to provide a better measure of a production value; *viii*) foregone earnings are calculated as the negative gap between earnings trajectories observed from 1992 in Figure 3; and *ix*) there are no positive externalities of AE.

The results from the computations are presented in Table 3 where we vary our assumptions on foregone earnings (three columns) and on social returns (three rows). As in earlier studies, the results are sensitive to how one defines the foregone production value. With the above assumption *viii*), we

[21] This follows Jacobson et al. (2005a), while Duflo (2001) assumes 20 percent.

overestimate the foregone production value if there is a slack in production and colleagues put in extra hours of work or non-employed individuals fill vacancies to compensate for the absence of someone who has enrolled in AE.[22] The social returns include potential general equilibrium effects, positive externalities of education on productivity, growth and non-pecuniary effects on health, equity and democracy. Our assumed multipliers are 1.0, 1.3 and 1.5, which is consistent with what has been suggested for some of these effects in isolation, though there is no consensus in the literature. Even with the most pessimistic assumptions, that there are no social returns and that there is no compensating labor supply for vacancies, the internal rate of return of the AE investments from the perspective society is 6.9 percent.

7 Summary and discussion

A policymaker who wishes to promote college enrolment should naturally focus on young individuals. Our study deals with a complementary option, asking whether adult acquisition of higher education yields benefits that exceed the costs to society. We evaluate the long-term earnings effects of tertiary level education for adults aged 29 to 55 at the time of their first enrolment. The major findings are the following. First, higher education is, on average, associated with earnings increases. Second, the increase is larger for females than for males. Third, the earnings increase is not evident in the short run – it takes approximately ten years before the earnings impact fully emerges. This finding underscores the need for a long follow-up period for a correct assessment. Fourth, the main implications of our results are robust to checks for ability bias and alternative assumptions regarding pre-treatment dynamics. Fifth, expressing returns in percentages of earnings is informative, but these are difficult to compare with results from the conventional returns to schooling literature. Sixth, the results from rough cost-benefit calculations indicate that the net benefits of education from society's perspective are non-trivial, even under fairly pessimistic assumptions, and despite substantial costs in terms of foregone earnings.

[22] According to the stable unit treatment value assumption (SUTVA), non-participants are completely unaffected by the program. While this is a very strong assumption, the earnings losses would then be an appropriate measure of foregone earnings (zero probability of finding a non-employed replacement). The opposite is to assume that *all* vacancies are replaced by non-employed, resulting in zero foregone production (Johnson and Layard 1986). In Table 3, we set the upper bound of the probability equal to the employment rate (.70).

The returns found in this study are more than twice the size of those reported for Swedish adults who enrolled AE at the upper secondary level (Stenberg et al. 2014, Stenberg 2011). The pattern in results is consistent with the job-polarization hypothesis, which states that earnings returns to different skill levels follow a U-shaped pattern. On this topic, one should avoid the potential misreading that middle-skill education is less important, as its absence forecloses the option of higher education (Acemoglu and Autor 2012). Also, increasing the supply of skilled workers is a channel to mitigate existing wage inequalities between skill levels (Nickell 2004, Goldin and Katz 2008).

While the relative clarity of our findings is encouraging, revealing a potential for policies to support adults in higher education, these need to be replicated in other contexts as marginal returns may vary substantially over time and between countries. Our results do not imply that other countries should increase their spending on adult education to Sweden's level, but they indicate that large educational investments relatively late in life may be associated with a long term positive payoff. This is of clear relevance to both economists and policy makers.

Figures and Tables:

Figure 1. Numbers in unemployment and higher education 1977-2009

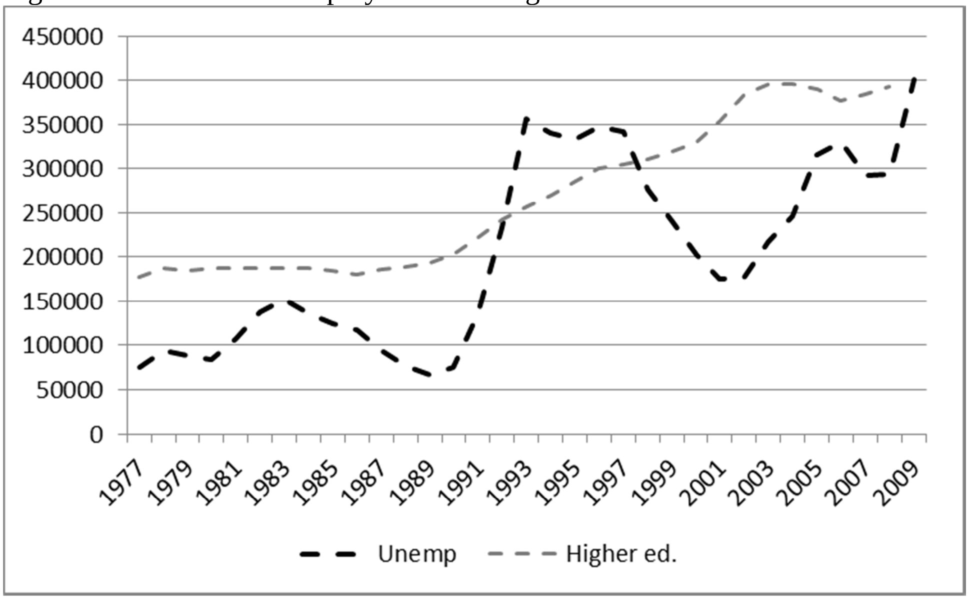
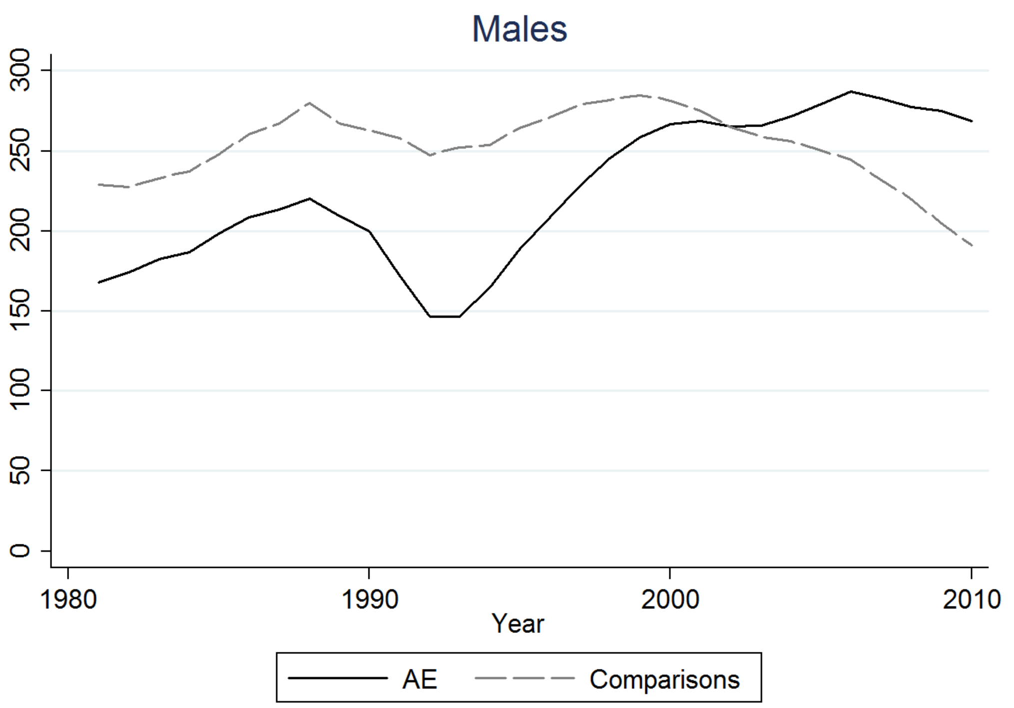
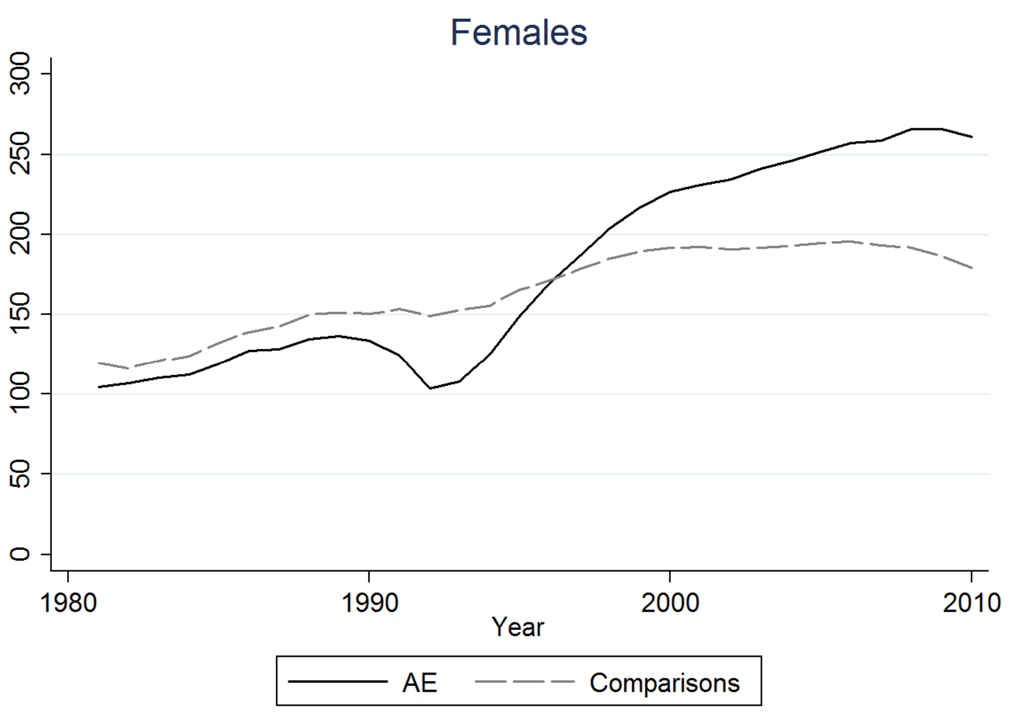


Figure 2. Annual earnings, 1982-2011; treated and non-treated, SEK in 1000s (2011 values).



Note: $N^{TREATED}$ = 1,624 and $N^{UNTREATED}$ = 174,667.



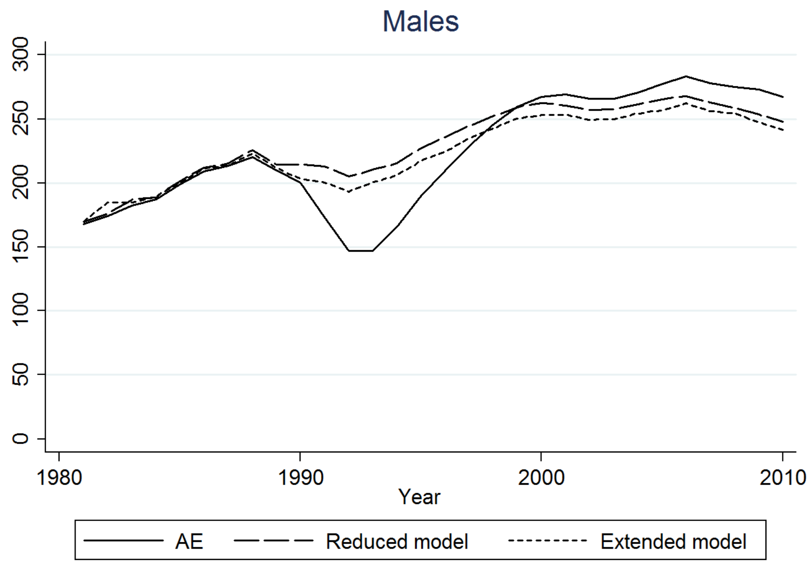
Note: $N^{TREATED}$ = 2,356 and $N^{UNTREATED}$ = 94,352.

Table 1: Descriptive mean statistics of treated and untreated - full samples. Earnings and transfers in 1000s of SEK (2011 values)

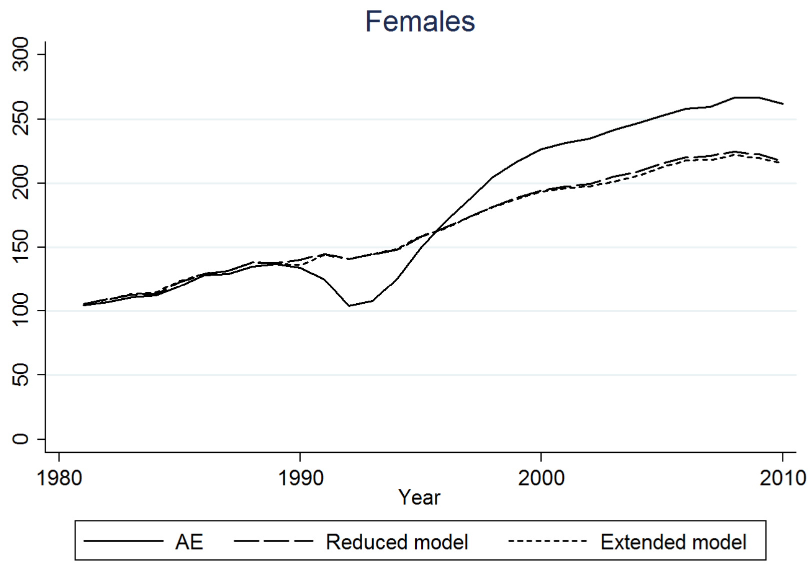
	Males		Females	
	Treated	Untreated	Treated	Untreated
Age	38.608	43.664	37.451	40.439
Born 1937	0.011	0.031	0.003	0.016
Born 1963	0.105	0.025	0.098	0.058
Humanities track	0.118	0.064	0.337	0.233
Business track	0.206	0.147	0.354	0.403
Science track	0.128	0.058	0.082	0.049
Engineering track	0.339	0.487	0.035	0.032
Vocational track	0.183	0.217	0.152	0.237
GPA	3.336	3.229	3.715	3.568
Cognitive skills	5.600	5.377	.	.
Non-cognitive skills	4.795	4.711	.	.
Children	1.152	1.224	1.572	1.360
Married	0.506	0.652	0.588	0.627
Foreign born	0.026	0.023	0.025	0.026
Inland*	0.061	0.048	0.076	0.040
Stockholm*	0.150	0.223	0.185	0.247
Regional employment	0.822	0.828	0.824	0.829
Construction	0.092	0.121	0.013	0.020
Manufacturing	0.258	0.266	0.113	0.120
Finance	0.111	0.140	0.119	0.165
Public sector	0.190	0.097	0.413	0.279
Max rank 1980s	.	.	0.702	0.675
UI benefits	0.062	0.022	0.091	0.051
ALMP**	0.059	0.014	0.053	0.025
Parental leave	0.079	0.042	0.201	0.153
Sick leave	0.684	0.600	0.738	0.696
Social welfare	0.040	0.022	0.051	0.026
Changes and transitions				
Earn. 1990-1988	-3.658	0.293	8.319	8.977
Earn. 1991-1990	-9.374	-4.396	-2.688	-0.875
UI 1991-1990	0.338	0.071	0.184	0.064
Sick 1991-1990	0.026	-0.021	-0.033	-0.043
Unemp-emp	0.063	0.022	0.087	0.051
Emp-unemp	0.148	0.035	0.139	0.058
Unemp-unemp	0.075	0.019	0.092	0.040
AE attendance				
Years of AE	1.372	0.064	1.871	0.189
Less than 1 year	0.363	0.958	0.254	0.911
At least 1 year	0.235	0.018	0.175	0.028
At least 2 years	0.169	0.005	0.176	0.013
At least 3 years	0.232	0.019	0.395	0.048
Observations	1624	174667	2356	94352

Notes: *The inland of Norrland is a sparsely populated area in the north of Sweden with permanently higher than average unemployment rates. Stockholm County hosts 20 percent of the population, and the overall employment level is higher than in any other region of Sweden.
** ALMP = Active Labor Market Program.

Figure 3. Annual earnings trajectories, treated and matched comparison groups.

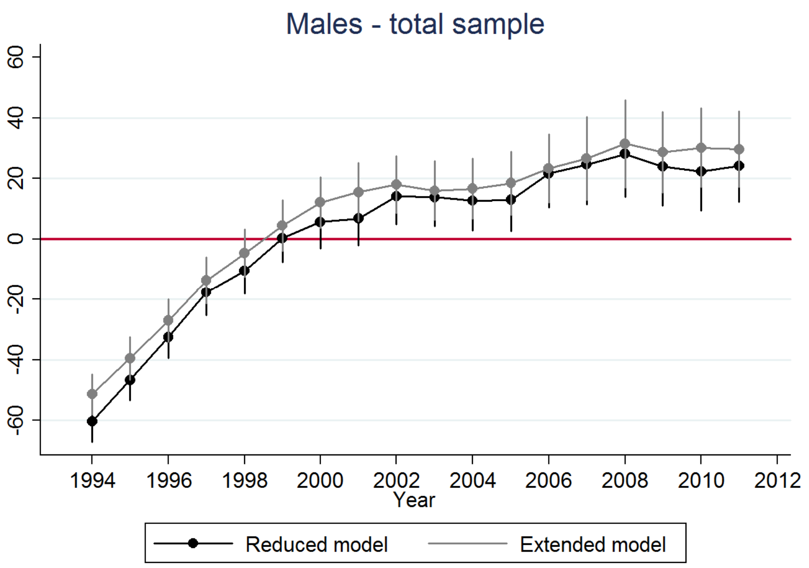


Note: $N^{TREATED}$ = 1,611 and $N^{UNTREATED}$ = 6,121 (weighted).

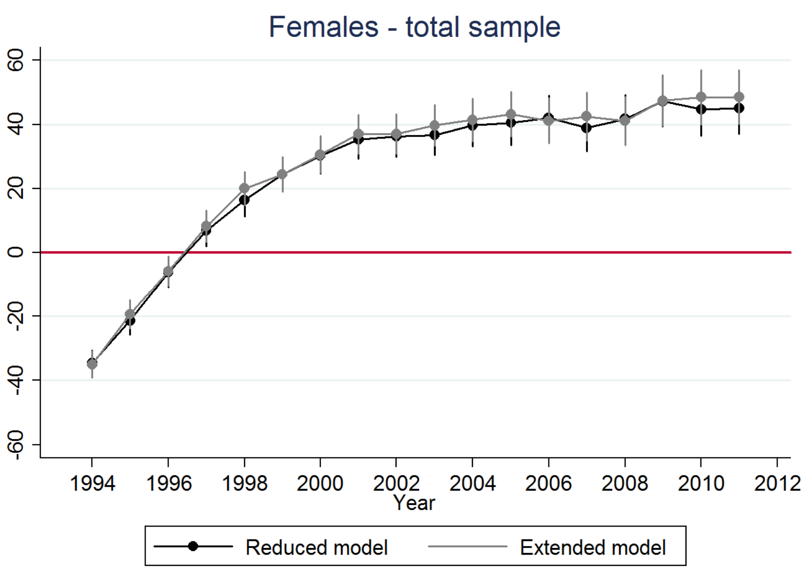


Note: $N^{TREATED}$ = 2,315 and $N^{UNTREATED}$ = 8,555 (weighted).

Figure 4. Difference-in-difference propensity score matching estimates, SEK in 1000s (2011 values).

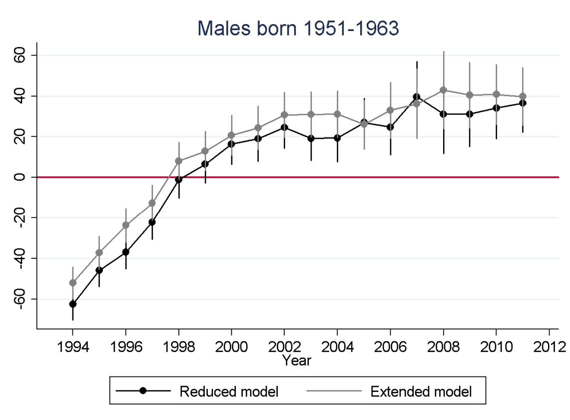


Average point estimate 2002-2011:
Reduced model SEK 19,857 (5.6 %). Extended model: SEK 23,886 (6.8 %)
Note: $N^{TREATED}$ = 1,611 and $N^{UNTREATED}$ = 6,121 (weighted).

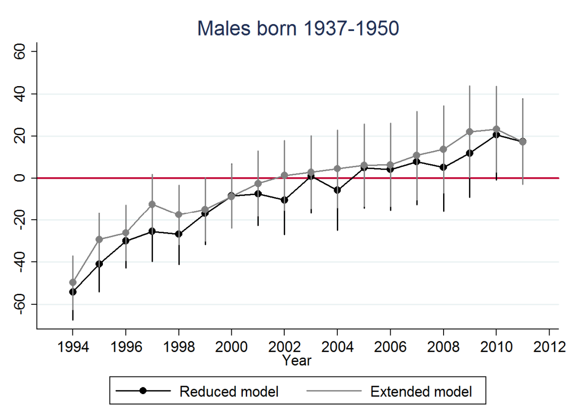


Average point estimate 2002-2011:
Reduced model SEK 41,303 (10.3 %). Extended model: SEK 43,073 (11.0 %)
Note: $N^{TREATED}$ = 2,315 and $N^{UNTREATED}$ = 8,555 (weighted).

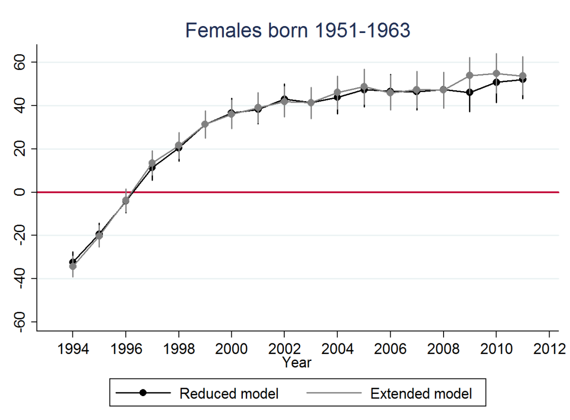
Figure 5. Difference-in-difference propensity score matching estimates by age groups.
Reduced model averages given below figures.



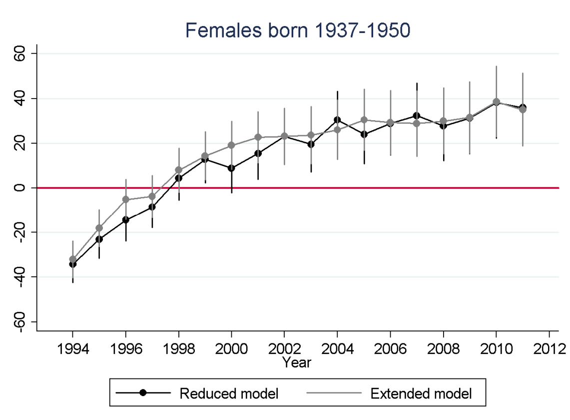
Average 2002-2011: SEK 28,735 (6.2 %)
N^{TREATED} = 1,050 and N^{UNTREATED} = 3,931 (weighted).



Average 2002-2011: SEK 5,671 (4.3 %)
N^{TREATED} = 561 and N^{UNTREATED} = 2,200 (weighted).

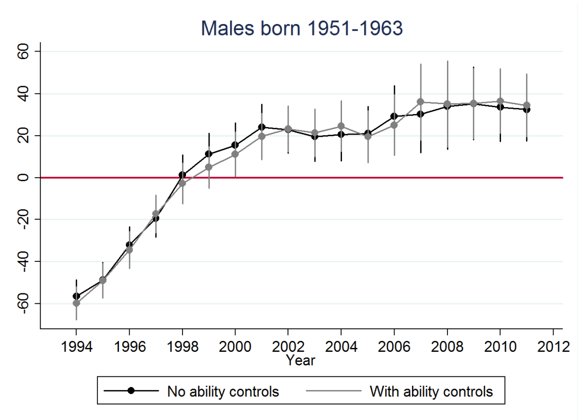


Average 2002-2011: SEK 46,402 (9.6 %)
N^{TREATED} = 1,693 and N^{UNTREATED} = 6,108 (weighted).

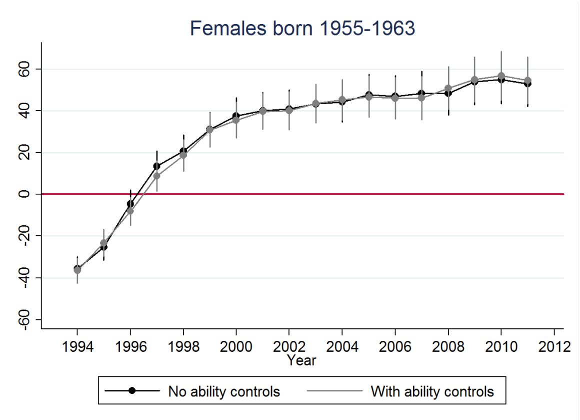


Average 2002-2011: SEK 29,130 (15.2 %)
N^{TREATED} = 621 and N^{UNTREATED} = 2,373 (weighted).

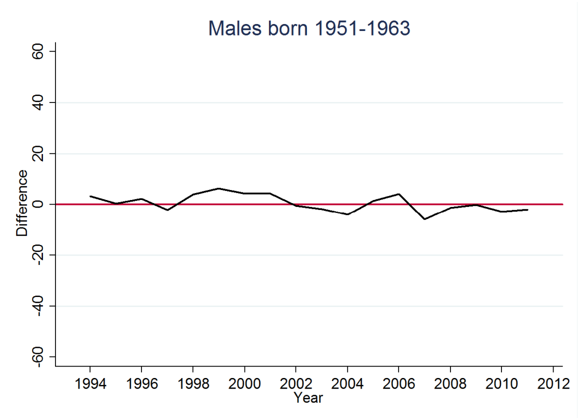
Figure 6. Estimates with and without ability controls and the deviation in point estimates.



Average 2002-2011: SEK 27,860 (6.0 %)

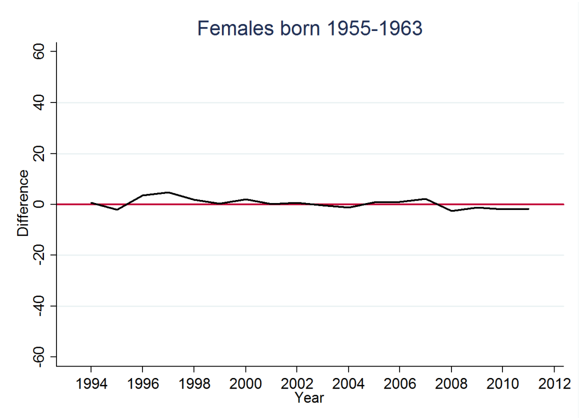


Average 2002-2011: SEK 48,099 (9.3 %)



Average 2002-2011: SEK 1,286 (.3 %)

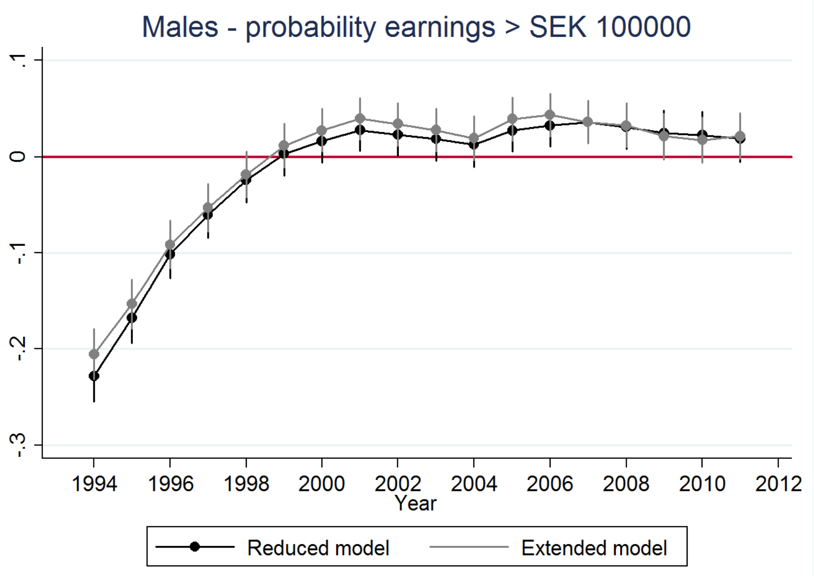
$N^{TREATED}$ = 951 and $N^{UNTREATED}$ = 3,493(weighted).



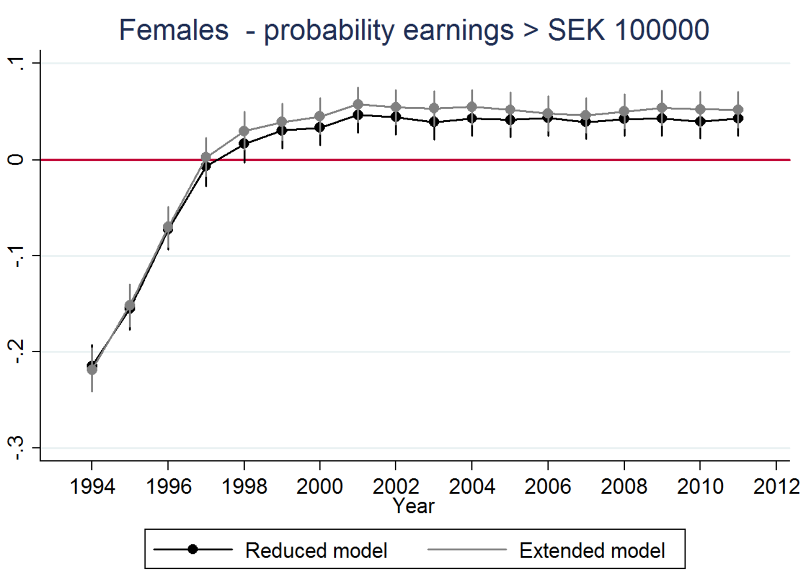
Average 2002-2011: SEK 4,184 (-.1 %)

$N^{TREATED}$ = 1,148 and $N^{UNTREATED}$ = 4,106 (weighted).

Figure 7. Propensity score matching estimates of the probability of earnings exceeding SEK 100,000 (approximately € 10,300), reduced model in black, extended model in grey.

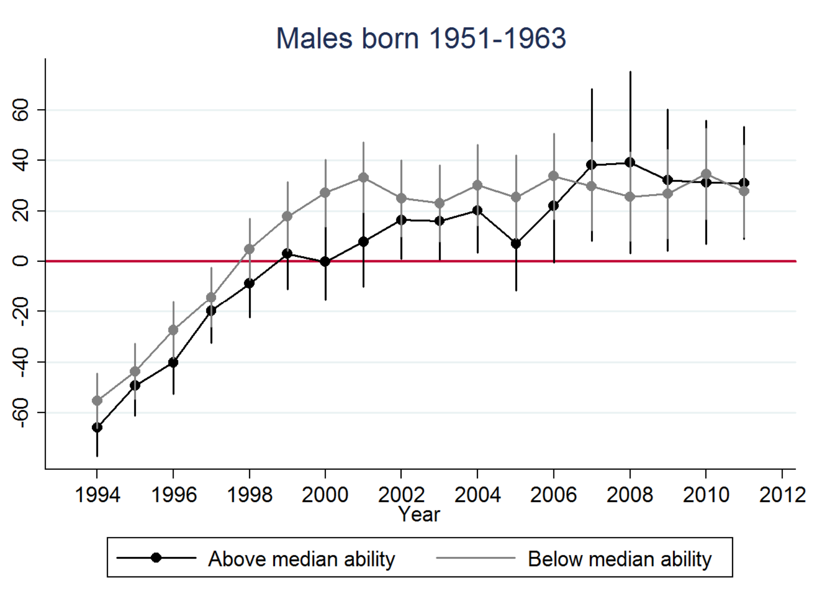


Average point estimate 2002-2011:
Reduced model 2.4 %. Extended model: 3.7 %.
Note: $N^{TREATED} = 1{,}611$ and $N^{UNTREATED} = 6{,}121$ (weighted).

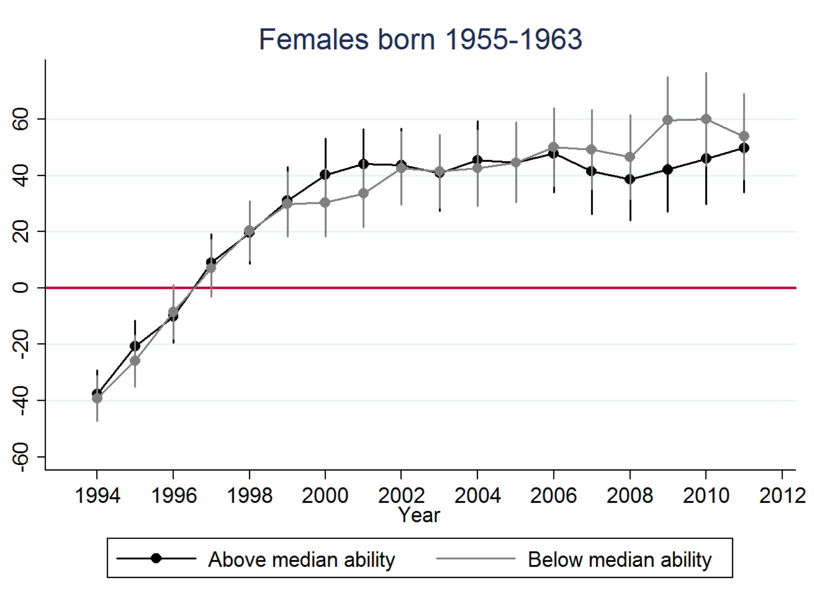


Average point estimate 2002-2011:
Reduced model 4.4 %. Extended model: 4.8 %.
$N^{TREATED} = 2{,}315$ and $N^{UNTREATED} = 8{,}555$ (weighted).

Figure 8. Heterogeneous effects of above and below median cognitive skills (males) or GPA (females).



Average point estimate 2002-2011:
Above median SEK 25,403 (4.8 %). Below median: SEK 28,242 (7.1 %)
Above median: $N^{TREATED}$ = 501 and $N^{UNTREATED}$ = 1,871 (weighted).
Below median: $N^{TREATED}$ = 450 and $N^{UNTREATED}$ = 1,702 (weighted).



Average point estimate 2002-2011:
Above median SEK 44,074 (8.0 %). Below median: SEK 49,091 (10.1 %)
Above median: $N^{TREATED}$ = 577 and $N^{UNTREATED}$ = 1,989 (weighted).
Below median: $N^{TREATED}$ = 571 and $N^{UNTREATED}$ = 2,096 (weighted).

Table 2. Average point estimates 2002-2011 by amount of completed studies, expressed in SEK 1000s (2011 values) and in percent.

	Males			**Females**		
	1000s SEK	Percent[a)]	N	1000s SEK	Percent [a)]	N
< 1 year	1.5	--	587	19.0	--	588
1<2 years	21.8	8.5%	379	36.6	17.4%	406
2<3 years	29.6	6.0%	272	42.4	10.2%	412
≥3 years	35.4	3.9%	376	61.1	8.4%	924
Total	19.7	5.6%	1611	41.6	10.3%	2315

[a)] Percent in each year is given by [(ATT/Average earnings of comparison group)/Completed AE]. The completed AE is based on recorded highest attained education each year 1994-2011.

Table 3. Internal rate of returns under varying assumptions for indirect costs and spill-over effects. All calculations are based on reduced model results.

	Probability that non-treated individuals fill work hours made vacant by AE participation (zero implies foregone earnings = foregone productivity).		
	0 ("lower bound")	.35	.70 ("upper bound")
Assumption on spill-over effects			
A. Social multiplier 1.0* private returns	6.9%	9.3%	12.9%
B. Social multiplier 1.3* private returns	10.4%	13.4%	17.8%
C. Social multiplier 1.5* private returns	12.6%	16.0%	21.1%

References

Acemoglu, A. and Autor, D.H. (2012). What Does Human Capital Do? A Review of Goldin and Katz's The Race Between Education and Technology. *Journal of Economic Literature* 50(2), 426-463.

Albrecht, J.W., Van den Bergh, G. and Vroman, S.B. (2009). The aggregate labour market effects of the Swedish Knowledge Lift program. *Review of Economic Dynamics*, 12(1), 129-146.

Altonji, J. (1993). The Demand for and Return to Education When Education Outcomes are Uncertain. *Journal of Labor Economics* 11(1) 48-83.

Antelius, J. and Björklund, A. (2000). How Reliable are Register Data for Studies of the Return on Schooling? An examination of Swedish data. *Scandinavian Journal of Educational Research* 44(4), 341-355.

Ashenfelter, O. (1978). Estimating the Effect of Training Programs on Earnings. *Review of Economics and Statistics* 60(1), 47-57.

Autor, D.H., Katz, L.F. and Kearney, M. (2008). Trends in U.S. Wage Inequality: Revising the Revisionists. *Review of Economics and Statistics* 90(2), 300-323.

Autor, D.H., Levy, F. and Murnane, R. (2003). The Skill Content of Recent Technological Change: An Empirical Exploration. *Quarterly Journal of Economics* CXVIII, 1279-1333.

Becker G.S. (1962). Investment in Human Capital: A Theoretical Analysis. *Journal of Political Economy* 70(5) Part 2: Investment in Human Beings (Oct., 1962), pp. 9-49.

Becker, G. S. (1985). Human capital, effort, and the sexual division of labor. *Journal of Labor Economics*, *3*, 33-58.

Becker, G. S. (1991). *A Treatise on the Family*. Cambridge, Mass: Harvard University Press.

Ben-Porath, Y. (1967). The Production of Human Capital and the Life Cycle of Earnings. *Journal of Political Economy* 75(1), 352-365.

Biewen, M., Fitzenberger, B., Osikominu, A. and Paul, M. (2014). The Effectiveness of Public-Sponsored Training Revisited: The Importance of Data and Methodological Choices. *Journal of Labor Economics* 32(4), 837-897.

Cameron, S. and Heckman, J. (2001). The Dynamics of Educational Attainment for Black, Hispanic and White Males. *Journal of Political Economy* 109(3), 455-499.

Card, D. (1999). The causal effect of education on earnings. In O.A. Ashenfelter and D. Card (eds), *Handbook of Labor Economics*, Vol 3. Amsterdam: North-Holland.

Comay, Y., Melnik, A. and Pollatschek, M. A. (1973. The Option Value of Education and the Optimal Path for Investment in Human Capital. *International Economic Review* 14(2), 421-435.

Dahlberg, M. and Forslund, A. (2005). Direct Displacement Effects of Active Labour Market Programs. *Scandinavian Journal of Economics* 107(3), 475-494.

Davis, S.J. and von Wachter, T.M. (2011). Recessions and the Costs of Job Loss. *Brooking Papers on Economic Activity*, Fall (1), 1-72.

de Luna, X., Waernbaum, I. and Richardson, T. (2011). Covariate Selection for the Non-Parametric Estimation of an Average Treatment Effect. *Biometrika* 98(4), 861-875.

Duflo, E. (2001). Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment. *American Economic Review* 91(4), 795-813.

Dustmann, C., Ludsteck, J. and Schoenberg, U. (2009). Revisiting the German Wage Structure. *Quarterly Journal of Economics* 124(2), 843-881.

Eliason, M. and Storrie, D. (2006). Lasting or Latent Scars? Swedish Evidence on the Long-Term Effects of Job Displacement. *Journal of Labor Economics* 24(4), 831-856.

Englund, P. (1999). The Swedish Banking Crisis – Roots and Consequences. *Oxford Review of Economic Policy* 15(3), 80–97.

EU (2000). *Lifelong Learning: the contribution of education systems in the Member States of the European Union*. Eurydice, Brussels.

EU (2001). *National Actions to Implement Lifelong Learning in Europe*. Eurydice, Brussels.

Goldin, C. and Katz,L. (2008). *The Race between Education and Technology*. Belknap Press for Harvard University Press; Cambridge, MA.

Goos, M. and Manning, A. (2007) Lousy and Lovely Jobs: The Rising Polarization of Work in Britain. *Review of Economics and Statistics* 89(1), 118-33.

Goos, M., Manning, A. and Salomons, A. (2009). Job polarization in Europe. *American Economic Review* 99(2), 58-63.

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.

Harjes, T. (2007). Globalization and Income Inequality: A European Perspective. IMF Working Paper, 07/169.

Heckman, J., LaLonde, R. and Smith, J. (1999). The Economics and Econometrics of Active Labor Market Programs. In Ashenfelter, O. and Card, D. (eds) *Handbook of Labor Economics*, Volume 3A, Ch. 31.

Heckman, J. and Smith, J. (1999). The Pre-Programme Earnings Dip and the Determinants of Participation in a Social Programme. Implications for Simple Programme Evaluation Strategies. *Economic Journal* 109(457), 313-348.

Heckman, J., Smith, J. and Tabler, C. (1998). Accounting for Dropouts in Evaluations of Social Programs. *Review of Economics and Statistics* 80(1), 1-14.

Heckman, J. and Urzua, S. (2008). The Option Value of Educational Choices And the Rate of Return to Educational Choices. Mimeo, University of Chicago.

IALS (2000). *Literacy in the Information Age – Final Report of the International Adult Literacy Survey*. Statistics Canada and OECD, Paris.

Ikenaga, T. and Kawaguchi, D. (2013). Labor-Market Attachment and Training Participation. *The Japanese Economic Review* 64(1), 73-97.

Jacobson, L.S., LaLonde, R.J. and Sullivan, D.G. (2003). Should We Teach Old Dogs New Tricks? The Impact of Community College Retraining on Older Displaced Workers. Federal Reserve Bank of Chicago WP 2003-25.

Jacobson, L.S., LaLonde, R.J. and Sullivan, D.G. (2005a). The Returns to Community College Schooling for Displaced Workers. *Journal of Econometrics* 125(1-2), 271-304.

Jacobson, L.S., LaLonde, R.J. and Sullivan, D.G. (2005b). The Impact of Community College Retraining on Older Displaced Workers: Should We Teach Old Dogs New Tricks? *Industrial & Labor Relations Review* 58(3), 397-415.

Jepsen, C., Troske, K. and Coomes, P. (2014). The Labor Market Returns to Community College Degrees, Diplomas and Certificates. *Journal of Labor Economics* 32(1), 95-121.

Johnson, G.E. and Layard, R. (1986). The Natural Rate of Unemployment: Explanation and Policy. In O.A. Ashenfelter and Layard, R. (eds), *Handbook of Labor Economics*, Vol 2. Amsterdam: North-Holland.

Kahn, L. B. (2010). The Long-Term Labor Market Consequences of Graduating from College in a Bad Economy. *Labour Economics* 17(2), 303-316.

Kane, T.J. and Rouse, C.E. (1995). Labor Market Returns to Two- and Four-Year College. *American Economic Review* 85(3), 600-614.

Killingsworth, M. (1982). Learning by Doing and “Investment in Training: A Synthesis of Two Rival Models of the Life Cycle. *Review of Economic Studies* XLIX, 263-271.

Lechner, M. and Wiehler, S. (2011). Kids or Courses? Gender Differences in the Effects of Active Labor Market Policies. *Journal of Population Economics* 24(3), 783-812.

Leigh, D.E. and Gill, A.M. (1997). Labor Market Returns to Community Colleges: Evidence for Returning Adults. *Journal of Human Resources* 32(2), 334-353.

Light, A. (1995). The Effects of Interrupted Schooling on Wages. *Journal of Human Resources* 30(3) 472-502.

Lindqvist, E. and Vestman, R. (2011). The Labor Market Returns to Cognitive and Noncognitive Abiity: Evidence from the Swedish Enlistment. *American Economic Journal: Applied Economics* 3(1), 101-128.

Manski, C. and Pepper, J. (2000). Monotone Instrumental Variables: With an Application to the Returns to Schooling. *Econometrica* 68(4), 997-1010.

Mincer, J. & Polachek, S. (1974). Family investments in human capital: Earnings of women. *Journal of Political Economy, 82(2)* part 2, 76-S108.

Monks, J. (1997). The Impact of College Timing on Earnings. *Economics of Education Review* 16(4), 419-423.

Monks, J. (1998) The Effect of Uncertain Returns on Human Capital Investment Patterns. *Atlantic Economic Journal* 26 (4), 413-419.

Neumark, D., Johnson, H., Li, Q. and Schiff, E. (2011). An Assessment of Labor Force Projections Through 2018: Will Workers have the Education Needed for the Available Jobs? Report prepared for the AARP foundation by the Public Policy Institute of California.

Nickell, S. (2004). Poverty and Worklessness in Britian. *Economic Journal* 114(494), C1-C25.

Nybom, M. (2014). The Distribution of Lifetime Earnings Returns to College. SOFI Working Paper 2/2014, Stockholm university.

OECD (1998). *Maintaining Prosperity in an Aging Society*. Paris: OECD.

OECD (2001). *Ageing and income: Financial resources and retirement in 9 OECD countries*. Paris.

OECD (2012). *Education at a Glance*. Paris: OECD.

Öckert, B. (2012), On the Margin of Success? Effects of Expanding Higher Education for Marginal Students, *Nordic Economic Policy Review* 1/2012: 111-157.

Oreopoulos, P., von Wachter, T. and Heisz, A. (2012). The Short-and Long-Term Career Effects of Graduating in a Recession. *American Economic Journal: Applied Economics* 4(1), 1-29.

Pissarides, C. (2011). Regular Education as a Tool of Counter-Cyclical Employment Policy. *Nordic Economic Policy Review* 1, 207-232.

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

Schwerdt, G., Messer, D.Woessman, L. and Wolter, S. (2012). The Impact of An Adult Education Voucher Program: Evidence from a Randomized Field Experiment. *Journal of Public Economics* 96(7-8), 569-583.

Smith, J. and P. Todd (2005). Does matching overcome LaLonde's critique of non-experimental estimators? *Journal of Econometrics* 125(1-2), 305-353.

Spitz-Oener, A. (2006). Technical change, job tasks and rising educational demands: Looking outside the wage structure. *Journal of Labor Economics* 24(2), 235-270.

Stange, K. (2012). An Empirical Investigation of the Option Value of College Enrolment. *American Economic Journal: Applied Economics* 4(1), 49-84.

Stenberg, A. (2007). Does Adult Education at Upper Secondary Level Influence Annual Wage Earnings? SOFI Working Paper 6/2007.

Stenberg, A. (2011). Using Longitudinal Data to Evaluate Publicly Provided Formal Education for Low-skilled. *Economics of Education Review* 30(6), 1262-1280.

Stenberg, A., de Luna, X. and 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., de Luna, X. and Westerlund, O. (2012). Can Adult Education Delay Retirement from the Labor Market? *Journal of Population Economics*, 25(2), 677-696.

Stenberg, A. and Westerlund, O. (2008). Does Comprehensive Education Work for the Unemployed? *Labour Economics* 15(1), 54-67.

Stenberg, A. and Westerlund, O. (2013): Higher Education and the Timing of Retirement. *The IZA Journal of European Labor Studies* 2:16.

Wallace, T.D. and Ihnen, L.A. (1975). Full-Time Schooling in Life-Cycle Models of Human Capital Accumulation. *Journal of Political Economy* 83(1), 137-155.

Weiss, Y. (1971). Learning by Doing and Occupational Specialization. *Journal of Economic Theory* 3(2), 189-198.

APPENDIX

Table A.1: Probit model maximum likelihood estimates of the propensity score. Selected variables, measured in 1990 unless stated otherwise. [a)]

Dependent variable: binomial indicator variable of registration in higher education 1992-1993.

	Males		**Females**	
	Reduced model	Extended model	Reduced model	Extended model
Regional emp	-2.614***	-2.483***	-2.273***	-2.486***
	(0.515)	(0.520)	(0.532)	(0.400)
Stockholm	-0.087*	-0.079*	-0.033	
	(0.034)	(0.034)	(0.033)	
Uppsala			-0.144*	-0.120*
			(0.059)	(0.059)
Göteborg			-0.086*	
			(0.037)	
Malmö	-0.138***	-0.135***		
	(0.037)	(0.038)		
Kronoberg			0.238***	0.259***
			(0.061)	(0.059)
Inland of Norrland	-0.087	-0.090	0.125**	0.116**
	(0.046)	(0.046)	(0.041)	(0.041)
Humanities	0.107**	0.105**	0.295***	0.299***
	(0.034)	(0.034)	(0.026)	(0.026)
Business			0.132***	0.138***
			(0.025)	(0.026)
Technology			0.363***	0.364***
			(0.053)	(0.054)
Science	0.204***	0.201***	0.350***	0.364***
	(0.033)	(0.034)	(0.040)	(0.040)
Vocational	-0.201***	-0.204***		
	(0.026)	(0.027)		
Married			-0.022	
			(0.022)	
One child	-0.072*	-0.059*	-0.003	-0.003
	(0.031)	(0.028)	(0.033)	(0.032)
Two children	-0.057	-0.039	0.114**	0.106**
	(0.033)	(0.026)	(0.036)	(0.032)
Three children	-0.027		0.235***	0.230***
	(0.046)		(0.048)	(0.044)
Four children			0.375***	0.375***
			(0.073)	(0.069)
More than four			0.386**	0.410**
			(0.129)	(0.127)
Child aged 0-3	-0.089**	-0.158***	-0.276***	-0.402***
	(0.034)	(0.037)	(0.032)	(0.037)
Child aged 4-6	0.039	0.035	-0.002	
	(0.032)	(0.030)	(0.028)	
Child aged 7-10			-0.063*	-0.066*
			(0.027)	(0.027)
Child aged 11-15	-0.015			
	(0.030)			
Child aged 16-17	0.057	0.058		
	(0.035)	(0.034)		
Child aged 18-	0.088**	0.081**	0.044	0.043
	(0.031)	(0.029)	(0.031)	(0.030)
Age at immigr			0.010	0.008
			(0.010)	(0.010)
Public sector	0.323***	0.331***	0.209***	0.217***
	(0.030)	(0.030)	(0.023)	(0.023)

Farming			0.024	
			(0.064)	
Constr.	0.035	0.017	-0.095	-0.096
	(0.036)	(0.036)	(0.077)	(0.078)
Manuf.	0.095***	0.087***	0.054	0.054
	(0.026)	(0.026)	(0.032)	(0.032)
Finance	0.038	0.046	-0.033	-0.025
	(0.033)	(0.033)	(0.030)	(0.031)
Earn. 1990	-0.001***	-0.001***	-0.001***	
	(0.000)	(0.000)	(0.000)	
Max rank 1980s			0.110***	0.111***
			(0.022)	(0.022)
Disp. inc 1990			0.000***	0.000***
			(0.000)	(0.000)
Zero earn.	-0.237***		-0.095*	-0.080
	(0.048)		(0.042)	(0.044)
Unemp. insur.		-0.182***	0.092*	-0.035
		(0.053)	(0.037)	(0.041)
ALMP	0.182*		0.122*	0.099
	(0.072)		(0.049)	(0.050)
- amount	0.003*	0.004***		
	(0.001)	(0.001)		
Parental leave	0.075	0.111*		
	(0.044)	(0.045)		
Sick leave	0.033		0.043*	0.036
	(0.022)		(0.022)	(0.022)
- amount	0.002***	0.003***		
	(0.000)	(0.000)		
Social welfare	0.132	0.054	0.234***	0.205***
	(0.068)	(0.069)	(0.048)	(0.048)
- amount	-0.013	-0.010		
	(0.007)	(0.007)		
Early retirem.	-0.339**	-0.205	-0.285**	-0.223*
	(0.114)	(0.119)	(0.099)	(0.102)
Extended model variables				
Earn. 1991		-0.000**		-0.001***
		(0.000)		(0.000)
Earn. 1991-1990				0.000*
				(0.000)
Sick 1991-1990		0.002***		
		(0.001)		
Sick 1992-1991		0.002**		
		(0.001)		
Soc welf 1991-90		0.005		
		(0.003)		
Early ret. 1991-90		-0.006		-0.009*
		(0.004)		(0.004)
Early ret. 1992-91		-0.011***		
		(0.003)		
Newly married				-0.123*
				(0.062)
Newly divorced				0.109
				(0.058)
Parental leave 1992-91				-0.002**
				(0.001)
No parent 1990, but 1991		0.132*		
		(0.058)		
Child 0-3 1991, but not 92		0.192***		0.199***
		(0.055)		(0.051)
No parent 1990, but 1991				-0.236***
				(0.059)
Parental leave 1990, not 91				0.125**
				(0.048)
Parental leave 1991, not 92				0.173***
				(0.043)

Outside – unemp [b)]				0.429***
				(0.083)
Emp - unemp		0.439***		0.299***
		(0.037)		(0.035)
Outside - outside		-0.279***		
		(0.053)		
Emp - outside				-0.093
				(0.057)
Unemp - outside				-0.305
				(0.184)
N	175613	175487	95978	95680

* p<0.05, ** p<0.01, *** p<0.001

[a)] See text for choice of explanatory variables. All regressions include a constant term and age-dummies and when relevant annual earnings of selected years 1982-1990. Complete results are available on request.

[b)] When relevant for balancing treated and untreated, extended model regressions include transitions in labor force status 1990-1991 between employment, unemployment and outside the labor force (OLF), in all nine possible transitions. OLF is defined as annual earnings below SEK 20,000 (app. €2,200) and no transfers related to unemployment insurance.

Table A.2: Balancing tests for reduced and extended propensity score matching models, males

	Reduced model			Extended model	
	Treated	Matched	*p*-value	Matched	*p*-value
AE years	1.372	0.135	*0.000*	0.135	*0.000*
Age	38.629	38.590	*0.880*	38.630	*0.977*
Born 1937	0.011	0.011	*0.899*	0.012	*0.869*
Born 1963	0.105	0.106	*0.943*	0.112	*0.553*
Regional employm.	0.822	0.822	*0.727*	0.822	*1.000*
Stockholm	0.150	0.155	*0.695*	0.151	*0.931*
Uppsala county	0.023	0.026	*0.588*	0.030	*0.190*
Södermanland	0.034	0.031	*0.653*	0.030	*0.581*
Östergötland	0.045	0.051	*0.432*	0.046	*0.933*
Jönköping	0.024	0.028	*0.391*	0.029	*0.298*
Kronoberg	0.017	0.014	*0.393*	0.014	*0.435*
Kalmar	0.022	0.028	*0.324*	0.024	*0.684*
Gotland	0.007	0.006	*0.710*	0.006	*0.669*
Blekinge	0.021	0.017	*0.403*	0.016	*0.331*
Skåne	0.032	0.029	*0.592*	0.033	*0.882*
Kristianstad	0.071	0.063	*0.371*	0.070	*0.864*
Halland	0.025	0.029	*0.517*	0.030	*0.393*
Göteborg	0.097	0.083	*0.166*	0.088	*0.395*
Älvsborg	0.051	0.046	*0.498*	0.046	*0.567*
Skaraborg	0.020	0.028	*0.127*	0.029	*0.098*
Värmland	0.051	0.043	*0.298*	0.042	*0.242*
Örebro	0.032	0.038	*0.303*	0.035	*0.640*
Västmanland	0.032	0.041	*0.139*	0.043	*0.082*
Dalarna	0.037	0.048	*0.143*	0.047	*0.167*
Gävleborg	0.049	0.039	*0.150*	0.038	*0.143*
Västernorrland	0.050	0.036	*0.065*	0.038	*0.090*
Jämtland	0.021	0.019	*0.683*	0.018	*0.462*
Västerbotten	0.038	0.036	*0.728*	0.036	*0.695*
Norrbotten	0.050	0.065	*0.073*	0.059	*0.286*
Inland of Norrland	0.060	0.065	*0.585*	0.062	*0.811*
Humanities	0.117	0.122	*0.664*	0.121	*0.786*
Business	0.204	0.209	*0.761*	0.210	*0.680*
Science	0.128	0.129	*0.937*	0.119	*0.447*
Engineering	0.340	0.332	*0.621*	0.345	*0.760*
Professional	0.184	0.182	*0.873*	0.179	*0.732*
Married	0.507	0.498	*0.603*	0.506	*0.923*
Divorced	0.063	0.065	*0.857*	0.064	*0.928*
Children at home	1.153	1.156	*0.943*	1.125	*0.473*
One child at home	0.204	0.197	*0.620*	0.197	*0.684*
2 children	0.269	0.268	*0.952*	0.263	*0.669*

3 children	0.101	0.104	*0.760*	0.101	*0.988*
4 children	0.020	0.019	*0.848*	0.019	*0.798*
More than 4 children	0.006	0.006	*0.775*	0.004	*0.664*
Child aged 0-3	0.196	0.200	*0.765*	0.185	*0.446*
Child aged 4-6	0.166	0.163	*0.776*	0.160	*0.593*
Child aged 7-10	0.172	0.173	*0.954*	0.176	*0.807*
Child aged 11-15	0.181	0.184	*0.828*	0.179	*0.936*
Child aged 16-17	0.092	0.089	*0.747*	0.090	*0.891*
Child aged 18 or above	0.181	0.177	*0.792*	0.177	*0.801*
Foreign born	0.026	0.026	*0.956*	0.026	*0.956*
Farming/Mining	0.034	0.034	*0.981*	0.028	*0.345*
Construction	0.093	0.095	*0.833*	0.088	*0.635*
Manufacturing	0.259	0.253	*0.701*	0.262	*0.802*
Finance. insurance	0.111	0.115	*0.749*	0.115	*0.697*
Public sector	0.190	0.192	*0.893*	0.186	*0.753*
Other sector	0.258	0.267	*0.569*	0.262	*0.818*
Earnings 1990	209.880	212.020	*0.606*	209.710	*0.970*
Earnings 1982	167.900	167.230	*0.865*	167.940	*0.994*
Earnings change 1990-1988	-3.703	-0.696	*0.316*	-1.952	*0.567*
Zero earnings 1990	0.059	0.054	*0.555*	0.065	*0.465*
Disposable inc 1990	178.660	182.380	*0.097*	182.930	*0.081*
Unemp. Insurance > 0	0.061	0.057	*0.681*	0.063	*0.812*
average amount	1.788	1.791	*0.993*	1.866	*0.807*
ALMP benefits > 0	0.056	0.053	*0.685*	0.055	*0.774*
average amount	2.055	1.780	*0.508*	1.984	*0.805*
Parental leave > 0	0.077	0.081	*0.636*	0.074	*0.740*
average amount	0.807	1.015	*0.292*	0.869	*0.760*
Sick leave > 0	0.684	0.689	*0.754*	0.683	*0.955*
average amount	9.969	9.545	*0.641*	10.087	*0.884*
Social welfare >0	0.037	0.038	*0.926*	0.037	*0.944*
average amount	0.187	0.162	*0.612*	0.196	*0.854*
Early retirement > 0	0.006	0.006	*0.956*	0.006	*0.955*
average amount	0.372	0.424	*0.788*	0.422	*0.798*
Earnings 1991	200.370			200.200	*0.969*
Earnings change 1991-1990	-9.513			-9.513	*0.998*
UI change 1991-1990	3.396			3.297	*0.859*
Sick leave change 1991-1990	0.246			0.480	*0.750*
Sick leave change 1992-1991	-1.948			-1.948	*0.999*
Social welfare change 1991-1990	0.264			0.293	*0.794*
Social welfare change 1992-1991	0.094			0.006	*0.466*
Early retirement change 1991-1990	0.064			0.141	*0.603*
Early retirement change 1992-1991	0.026			-0.020	*0.778*
Newly married	0.030			0.034	*0.550*
Newly divorced	0.024			0.025	*0.797*
Parental leave change 1991-1990	0.272			0.159	*0.630*
Parental leave change 1992-1991	0.093			-0.081	*0.484*

No parental 1990 - above zero 1991	0.041	0.038	*0.718*
No parental 1991 - above zero 1992	0.035	0.031	*0.524*
Parental leave > 0 1990 - none 1991	0.033	0.038	*0.446*
Parental leave > 0 1991 - none 1992	0.040	0.036	*0.613*
Child 0-3 1990 - none 1991	0.043	0.045	*0.731*
Child 0-3 1991 - none 1992	0.046	0.045	*0.866*
Emp - emp	0.855	0.848	*0.595*
OLF - OLF	0.045	0.049	*0.590*
Unemp - unempl	0.074	0.080	*0.575*
Emp - OLF	0.021	0.030	*0.117*
Emp - unempl	0.149	0.148	*0.970*
OLF - emp	0.024	0.020	*0.456*
OLF - unempl	0.006	0.005	*0.772*
Unemp - emp	0.063	0.068	*0.570*
Unemp - OLF	0.005	0.003	*0.362*

Note: Reported *p*-values are from *t*-tests of no equality between treated and untreated. The extended model includes nine possible transitions in labor force status 1990-1991 between employment, unemployment and outside the labor force (OLF). OLF is defined as annual earnings below SEK 20,000 (app. €2,200) and no transfers related to unemployment insurance. Further controls include changes in amounts of social insurance benefits 1990-1991 and 1991-1992 as well as changes in annual earnings and UI payments 1990-1991.

Table A.3: Balancing tests for reduced and extended propensity score matching models, females

	Reduced model			Extended model	
	Treated	Matched	*p*-value	Matched	*p*-value
AE years	1.869	0.288	*0.000*	0.288	*0.000*
Age	37.418	37.395	*0.903*	37.437	*0.950*
Born 1937	0.003	0.004	*0.749*	0.004	*0.749*
Born 1963	0.099	0.104	*0.635*	0.102	*0.769*
Regional employm.	0.824	0.824	*0.680*	0.824	*0.882*
Stockholm	0.184	0.184	*0.977*	0.179	*0.675*
Uppsala county	0.022	0.021	*0.724*	0.023	*0.883*
Södermanland	0.029	0.029	*0.983*	0.026	*0.517*
Östergötland	0.041	0.045	*0.528*	0.044	*0.689*
Jönköping	0.030	0.027	*0.509*	0.026	*0.425*
Kronoberg	0.028	0.027	*0.752*	0.029	*0.826*
Kalmar	0.030	0.026	*0.477*	0.024	*0.248*
Gotland	0.005	0.006	*0.517*	0.006	*0.548*
Blekinge	0.018	0.015	*0.488*	0.015	*0.398*
Skåne	0.031	0.037	*0.262*	0.033	*0.660*
Kristianstad	0.079	0.087	*0.305*	0.082	*0.645*
Halland	0.025	0.031	*0.249*	0.029	*0.484*
Göteborg	0.069	0.061	*0.318*	0.079	*0.186*
Älvsborg	0.041	0.051	*0.093*	0.047	*0.316*
Skaraborg	0.029	0.031	*0.649*	0.031	*0.619*
Värmland	0.031	0.035	*0.421*	0.035	*0.352*
Örebro	0.035	0.035	*0.905*	0.031	*0.413*
Västmanland	0.036	0.030	*0.295*	0.031	*0.369*
Dalarna	0.040	0.038	*0.762*	0.040	*0.985*
Gävleborg	0.044	0.038	*0.318*	0.039	*0.419*
Västernorrland	0.046	0.039	*0.259*	0.040	*0.337*
Jämtland	0.024	0.021	*0.552*	0.021	*0.470*
Västerbotten	0.031	0.035	*0.421*	0.033	*0.660*
Norrbotten	0.053	0.049	*0.605*	0.055	*0.708*
Inland of Norrland	0.076	0.071	*0.447*	0.074	*0.696*
Humanities	0.337	0.341	*0.774*	0.341	*0.786*
Business	0.356	0.351	*0.759*	0.359	*0.788*
Science	0.081	0.083	*0.799*	0.079	*0.755*
Engineering	0.035	0.035	*0.873*	0.033	*0.715*
Professional	0.153	0.166	*0.232*	0.165	*0.278*
Married	0.588	0.599	*0.428*	0.601	*0.385*
Divorced	0.081	0.078	*0.694*	0.076	*0.530*
Children at home	1.574	1.593	*0.574*	1.574	*0.992*
One child at home	0.197	0.193	*0.739*	0.195	*0.882*
2 children	0.378	0.374	*0.797*	0.380	*0.886*

3 children	0.150	0.156	*0.554*	0.149	*0.934*
4 children	0.034	0.034	*0.887*	0.035	*0.824*
More than 4 children	0.007	0.009	*0.621*	0.006	*0.655*
Child aged 0-3	0.217	0.218	*0.964*	0.221	*0.756*
Child aged 4-6	0.238	0.241	*0.783*	0.241	*0.756*
Child aged 7-10	0.271	0.278	*0.565*	0.269	*0.849*
Child aged 11-15	0.308	0.302	*0.655*	0.300	*0.512*
Child aged 16-17	0.123	0.130	*0.479*	0.125	*0.815*
Child aged 18 or above	0.167	0.167	*1.000*	0.169	*0.798*
Foreign born	0.026	0.026	*0.945*	0.027	*0.750*
Farming/Mining	0.021	0.021	*0.918*	0.021	*1.000*
Construction	0.013	0.012	*0.664*	0.013	*0.948*
Manufacturing	0.114	0.113	*0.954*	0.118	*0.687*
Finance. insurance	0.120	0.118	*0.874*	0.114	*0.507*
Public sector	0.416	0.420	*0.829*	0.421	*0.777*
Other sector	0.242	0.253	*0.372*	0.252	*0.433*
Earnings 1990	137.050	136.970	*0.978*	136.720	*0.866*
Highest earnings rank 82-90	0.713	0.707	*0.788*	0.714	*0.983*
Earnings 1982	104.560	104.630	*0.978*	104.700	*0.985*
Earnings change 1990-1988	7.768	6.052	*0.421*	5.960	*0.397*
Zero earnings 1990	0.068	0.066	*0.757*	0.066	*0.848*
Disposable inc 1990	149.020	147.640	*0.466*	149.010	*0.967*
Unemp. Insurance > 0	0.090	0.087	*0.688*	0.088	*0.796*
average amount	1.995	1.928	*0.788*	1.951	*0.854*
ALMP benefits > 0	0.050	0.049	*0.852*	0.052	*0.751*
average amount	1.858	1.636	*0.463*	1.834	*0.933*
Parental leave > 0	0.199	0.199	*0.956*	0.209	*0.396*
average amount	6.970	7.253	*0.624*	7.600	*0.265*
Sick leave > 0	0.740	0.743	*0.827*	0.743	*0.860*
average amount	7.085	7.148	*0.900*	6.802	*0.541*
Social welfare >0	0.048	0.047	*0.782*	0.051	*0.685*
average amount	0.275	0.266	*0.894*	0.312	*0.610*
Early retirement > 0	0.007	0.008	*0.700*	0.006	*0.821*
average amount	0.344	0.403	*0.670*	0.332	*0.927*
Earnings 1991	134.100			135.060	*0.734*
Earnings change 1991-1990	2.682			-1.656	*0.397*
UI change 1991-1990	1.870			1.938	*0.845*
Sick leave change 1991-1990	-0.686			-0.764	*0.290*
Sick leave change 1992-1991	-2.204			-2.139	*0.342*
Social welfare change 1991-1990	0.069			0.019	*0.451*
Social welfare change 1992-1991	0.038			0.027	*0.698*
Early retirement change 1991-1990	0.060			0.034	*0.602*
Early retirement change 1992-1991	0.146			0.200	*0.541*
Newly married	0.022			0.021	*0.919*
Newly divorced	0.029			0.030	*0.777*
Parental leave change 1991-1990	-0.074			0.265	*0.542*

Parental leave change 1992-1991	0.079	-2.633	*0.708*
No parental 1990 - above zero 1991	0.039	0.040	*0.895*
No parental 1991 - above zero 1992	0.026	0.028	*0.699*
Parental leave > 0 1990 - none 1991	0.049	0.052	*0.628*
Parentail leave > 0 1991 - none 1992	0.070	0.070	*0.943*
Child 0-3 1990 - none 1991	0.054	0.061	*0.303*
Child 0-3 1991 - none 1992	0.048	0.045	*0.623*
Emp - emp	0.802	0.814	*0.309*
OLF - OLF	0.062	0.071	*0.227*
Unemp - unempl	0.092	0.087	*0.536*
Emp - OLF	0.031	0.030	*0.814*
Emp - unempl	0.140	0.139	*0.924*
OLF - emp	0.040	0.038	*0.761*
OLF - unempl	0.018	0.018	*1.000*
Unemp - emp	0.086	0.091	*0.526*
Unemp - OLF	0.002	0.003	*0.709*

Note: Reported *p*-values are from *t*-tests of no equality between treated and untreated. The extended model includes nine possible transitions in labor force status 1990-1991 between employment, unemployment and outside the labor force (OLF). OLF is defined as annual earnings below SEK 20,000 (app. €2,200) and no transfers related to unemployment insurance. Further controls include changes in amounts of social insurance benefits 1990-1991 and 1991-1992 as well as changes in annual earnings and UI payments 1990-1991.