Does Formal Education for Older Workers Increase Earnings?
–
Analyzing Annual Data Stretching over 25 Years[1]

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Abstract: Governments in the US, Canada and Europe have expressed an ambition to stimulate education of older. In this paper, we analyze if there are effects on annual earnings of formal education for participants aged 42-55 at the time of enrolment in 1994-1995. The analysis explores longitudinal population register data stretching from 1982 to 2007. The method used is difference-in-differences propensity score matching based on a rich set of covariates, including indicators of health and labor market marginalization. Results differ from earlier studies, implying no significant average earnings effects for males, positive effects for females, although insufficient to cover total costs.

Keywords: Adult education, Earnings, Government Expenditures, Human capital

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1 Introduction

Most OECD countries face increasing shares of the population in retirement rather than productive work. At the same time, there is a widespread belief that large groups in the mid-aged and older labor force have low or obsolete formal education. Consequently, education of older workers appears a viable approach to increase the tax base and total life time earnings in order to counteract the effects of an aging population. Policies which stimulate retraining of older workers have been advocated by intergovernmental political bodies such as the OECD and the European Union (OECD 1998, 2006, EU 2001, 2008). More recently, in the US, the Obama administration has issued a six-fold increase in federal government assistance to community colleges, to smooth the effects of structural changes and assist "casualties of a changing economy" (Kellogg and Tomsho 2009). Governments in Canada, the UK and continental Europe have also showed increased interest (Drummond et al. 2010, UK Cabinet Office 2008, Schwerdt et al.2011). The motivations for these policies chiefly stem from expectations of positive effects on employment and production, although education may be linked to positive non-pecuniary effects on e.g. democracy, health and social cohesion. Empirically, several authors have argued that most of the positive trend in US labor force participation rates among older is explained by an increased educational level (Blau and Goodstein 2010) and/or by a decrease in the educational gap between young and old workers (Goldin and Katz 2007, Maestas and Zizzimoupolos 2010). However, this primarily concerns education obtained at a young age. Studies addressing education for adults have broadly speaking involved three different measures (which sometimes partly overlap); on-the-job-training (OJT), training programs for unemployed and formal education for adults at e.g. secondary or tertiary level. Many studies have found OJT to be associated with high earnings returns, but these are widely believed to be marred by selection issues as employers monitor participation (e.g. Pischke 2007). With improved identification strategies, estimates have instead been close to zero (Leuven and Oosterbeck 2008, Fahr and Simons 2010). Evaluations of training programs for unemployed have overall indicated modest effects (Heckman *et al.* 1999, Kluve 2010, Card *et al.* 2010). While Heckman et al. (1999) reported a tendency of more positive effects for fe-

males; these were not corroborated in Card et al. (2010). Formal education for adults constitutes a much smaller literature and it is the measure we focus on in this study. In view of the emerging interest among policy makers, this topic appears as a highly relevant field of research.

The present paper provides an evaluation of the earnings effects of formal education for older, defined as aged 42-55 at the time of enrolment in 1994-1995. We explore exceptional data from Swedish population registers which include information on individuals' labor earnings 1982-2007 and course registrations since 1979. Sweden is a suitable country for a study of this kind as it is an open economy under pressure from international competition, with changes in skill composition of labor demand, and several institutional factors stimulating both the demand for and supply of formal education for adults at compulsory, upper secondary and tertiary level (henceforth AE). The richness of our data allows us to take into account important pre-program dynamics when modeling program participation (Heckman and Smith 1999). Theoretically, all individuals consider every year, at least implicitly, whether to enroll in AE or not by considering the net present value of their expected utility and opportunity costs of AE. The decisions are therefore likely to be influenced by dynamic factors such as new information, skills depreciation, changes in relative wages, changes in preferences, changes in the expected future attachment to the labor market, changes in health and/or changes in borrowing constraints.[2] Using difference-in-differences propensity score matching methods, we control for ten years of pre-AE earnings and check for changes regarding indicators of health and labor market marginalization occurring in the year(s) prior to enrolment. Even though identification of causal effects can always be questioned, our main results are stable to several different assumptions regarding unobservables.[3]

[2] See Altonji (1993), Ben-Porath (1967), Ikenaga and Kawaguchi (2010), Iwahashi (2007), Killingsworth, (1982), McCall (1990), Monks (1998), Neal (1999), Wallace and Ihnen (1975), Weiss (1971). Note that the suggested dynamics imply that the returns to education do not necessarily fall monotonically with age, and that the optimal timing of educational investments may occur at a fairly late stage in life.

[3] A general finding in the discussion on bias in the program evaluation literature is that non-experimental evidence cannot easily be discarded when based on high-quality data (Heckman et al. 1999 (ch 10), Heckman and Smith 1999, Glazerman 2003, Smith and Todd 2005, Diaz and Handa 2006, Card et al. 2010).

Earlier studies of formal education for older adults have reported positive earnings effects, although based on highly selected samples. In a series of papers, Jacobson, Lalonde and Sullivan (JLS, 2003, 2005a, 2005b) analyzed the proportional returns to community college schooling among laid-off workers in Washington State. JLS (2005a) reported an earnings return of roughly 10 percent to a year of studies, and no significant differences between samples aged 22-35 and 36-59 at the time of job loss. The private net benefits for the sample aged above 35 were lower as their expected time until retirement was substantially shorter, calculations indicating an internal rate of return to society of around 4 percent. The sample studied was conditioned to be attached to the Washington State unemployment insurance covered workforce during 14 years, reducing the sample from 167,000 to 65,000 (JLS 2005a, p412).[4]

European studies include Schwerdt et al. (2011) who explored data from Switzerland on individuals aged 20-60, subject to a randomly distributed voucher system that could be redeemed for AE during the first half of 2006. Some 18 percent of the individuals with vouchers also used them and completed on average 42 hours of courses, but there was no effect on labor market outcomes one year after education was completed. Stenberg and Westerlund (2008) analyzed the effects of AE in Sweden on a sample aged 25-55 at the time of enrolment in 1997. The study was limited to long-term unemployed who had zero annual earnings in 1996 and 1997. Six years later, in 2003, the estimated effect of AE (binary variable) on earnings was sizeable, covering the costs to society within five to seven years. They found no differences in the returns between the older and the younger half of their sample. Stenberg (2011) studied a more broadly defined and younger sample, aged 24-43 in 1994. Using family fixed effects, and with access to completed course credits, the reported earnings return to a year of AE was about 4.5 percent by 2004 when aged 34-53.[5] Again, there were no age-

[4] Jepsen et al. (2009) present estimates on earnings of student *intentions* to complete community college in Kentucky. Sample size is 46,500 (no non-participants), with an average age of 28.8 (std. dev. 11). Estimates are overall reasonably similar across age-groups and about 40 percent higher for females compared with males. On Canadian survey data collected over six years, Zhang and Palameta (2006) studied a smaller sample of community college participants (1,462 individuals). A positive earnings impact was only found for individuals aged 17-34 who had completed a certificate. No significant effects were found for individuals aged 35-59.

[5] The working paper version, Stenberg (2009), reconciles mixed results from Albrecht *et al*. (2004) and Ekström (2003).

related differences in the earnings returns of the treated but calculations indicated that the benefits only barely covered the total costs incurred by society. To motivate the expenses, social returns were required to exceed the private returns of AE by at least one third. Finally, Stenberg et al. (2011) analyzed if participation in AE 1986-1989 affected the timing of retirement. For a sample born 1931-1944, followed from 1982 until 2004 when aged 60-71, they did not discern any effects of AE on the timing of retirement.

In summary, the two articles on long term earnings effects of AE for older workers concern samples which are narrowly defined due to restrictions prior to AE (Stenberg and Westerlund 2008) or post enrolment (JLS 2005a). Even though both studies reported net positive effects for society, the results in other studies are more ambiguous in terms of the private effects of AE (Schwerdt et al. 2011, Stenberg et al. 2011) and regarding society's costs (Stenberg 2011). Thus, given the enormous interest among policymakers, we know surprisingly little about the costs and benefits associated with education for older.

The contribution of this paper is to present an evaluation of formal education for a broadly defined sample aged 42-55 at the time of enrolment, 1994-1995. We explore information on annual earnings until 2007, when the age span of our sample is 55-68, thereby stretching beyond the official retirement age at 65. The results indicate positive effects of AE on average earnings for females, but not for males. An interesting finding is that the beneficial effects of AE largely pertain to females who at an earlier stage appear to have been constrained from AE participation by household responsibilities such as child rearing. In an international perspective, this may suggest that by targeting e.g. information campaigns to this relatively well defined group, efficiency in educational investments could be improved. Taken as a whole, and contrary to earlier studies, we find that the benefits are not sufficient to cover total costs. Consequently, for the sample we study, the generous AE policy in Sweden does not seem to have eased the burden of support of an aging population.

The following section provides a brief outline of the institutional framework regarding adult education in Sweden and a description of the data. The empirical strategy is described in Section 3 and the results are presented in Section 4. We assess society's costs and benefits in Section 5 before a concluding discussion in Section 6.

2 Institutional setting and data

2.A Education for adults in Sweden

The institutional and legislative set-up in Sweden is relatively favorable for adults who wish to engage in formal education. AE is therefore a common feature of an individual career path and it attracts about 2 per cent of the labor force each year (including individuals only registered in one course). The demand for AE is enhanced by the fact that employees have a legally protected right to take a leave of absence for studies and thereafter return to the same job. Full-time students are also entitled to financial support which covers modest living expenses (a mix of loans and allowances of approximately €800 per month). On the supply side, municipalities are mandated to offer adult schooling covering subjects at the "compulsory" (elementary and lower secondary) and upper secondary (high school) level. This is provided by municipal education centers *Komvux*, where access to courses is relatively unrestricted. The compulsory level courses mainly cover Swedish and mathematics, whereas upper secondary level courses encompass a wide range of theoretical subjects, as well as courses related to health care, nursing and computer skills, but only to a lesser degree other vocational courses. Komvux also offer a small supply of "supplementary courses" (*påbyggnadsutbildning*) which are vocational and officially classified as tertiary education.

Compulsory school in Sweden today is nine years, followed by upper secondary school which, depending on the type of schooling, is one, two or three years. One year upper secondary education is dominated by health care and nursing, whereas two year upper secondary programs are considerably more heterogeneous. Three broadly defined groups will be distinguished: commerce, manufac-

turing (including craftsmanship and technology) and transport (including vehicle engineering and media communication). Three year upper secondary schooling is mainly theoretical and preparatory for higher (tertiary) education. The general requirement for tertiary education, which is provided in about 30 cities (in a total population of 9 million), is completion of a three year upper secondary school.

Adults who enroll at Komvux choose their own combination of courses. The aim may be to complete courses in basic skills such as numeracy and/or literacy (compulsory level), to complete a one- or two year upper secondary diploma, to become eligible for tertiary education via a three year diploma or to complete a (few) specific course(s) as part of a leisure activity or for labor market related reasons. The level of detail in our data, presented in the next section, will allow us to distinguish reasonably well among these activities.

2.B Design of the study

This study is based on population register data administered by Statistics Sweden. Records of annual labor earnings are available 1982-2007 and Komvux transcripts of course registrations from 1979 to 2007. Our sample consists of all individuals born in Sweden between 1939 and 1952 and living in Sweden 1990-2007. Thus, the individuals are aged 42-55 in 1994 and 55-68 in 2007, allowing us to follow individuals up to and beyond the years when retirement typically occurs. The choice of sample is motivated by the fact that retirement becomes increasingly feasible from the age of 55, and by the fact that records of completed AE course credits are only reported from 1994 onwards. The courses offered at Komvux are mainly at the upper secondary level (80 percent) and compulsory level (15 percent). We therefore restrict our analysis to those whose highest attained level of education prior to AE enrolment is two years of upper secondary schooling or below, thereby excluding about one third of the population born 1939-1952. Individuals are also excluded if they were registered in AE at

some point 1979-1993.[6] This restriction means that until 1993, all individuals in the sample have year after year rejected (or not considered) the idea of participating in AE. Our "treatment group" consequently consists of first-time enrollees in AE in 1994 or 1995. The comparison group either did not register in AE at all 1979-2007, or registered for the first time in 1996 or later. This leaves us with a total sample of 636,314 individuals, of which about one percent (6,311) is treated. As one would expect, the youngest cohort, born in 1952, has the highest proportion of treated. The lowest proportion is found for the cohort born in 1939. For males, the fractions range from .26 percent to 1.06 percent and for females from .47 to 3.13 percent. The overrepresentation of females among participants in AE (68 percent) plausibly reflects gender differences in life-cycle patterns of labor market careers.

Note that we allow for enrolment in AE to occur also after 1995 since we do not want to condition on events which occur after 1994-1995. Re-enrolment is of foremost importance for the years 1997-1999, when the Swedish government launched the Adult Education Initiative. It enabled individuals to conduct full-time studies at Komvux with financial support equal to the individual's level of unemployment insurance benefits (e.g. Stenberg 2007a, Stenberg and Westerlund 2008). The proportion of treated registered in AE was 12 percent in 1998 and gradually fell to 1 percent in 2004. In the comparison group, the fraction registered at Komvux peaked in 1998 at 2.0 percent.

Table 1 presents descriptive characteristics by gender for treated and untreated. Most of the average treated-untreated differences are statistically significant, diverging in terms of education, age and the number of children at home.[7] There are also differences in the proportions receiving various social insurance payments such as unemployment insurance, early retirement pensions and social

[6] This excludes 22.6 percent of the sample, including 29.4 percent of those with two year upper secondary education.
[7] Our sample was born from 1939 until 1952. The compulsory school was gradually extended into the present nine-year system during the 1960s, affecting birth cohorts from 1949 and onwards. This means that more than half of our sample attended the old schooling system, where school length was seven or eight years, varying between municipalities. Pupils with better marks were selected to attend their seventh year at junior secondary school (*realskolan*) which was at least three years and a prerequisite for upper secondary school. Among those not selected, there was a second chance to enroll in junior secondary school in the year(s) that followed.

welfare. The last ten rows of Table 1 also indicate that, in the years preceding AE enrolment, social welfare benefits and sick-leave benefits increased more for the treated.[8]

Earnings trajectories 1982-2007 of treated and non-treated are presented in Figure 1 (males) and Figure 2 (females). The treated-untreated averages are remarkably similar from 1982 until 1992. For males, the differences are below 1 percent in ten of the twelve years before treated experienced a drop in earnings of 9.1 percent in 1993. Treated females experienced a slight positive trend relative to untreated. Their average in 1982 was 2.4 percent below untreated but in 1992 their earnings were 3.1 percent higher. As in the case of males, treated females also experienced a drop in average earnings in 1993, by 5.8 percent.[9] For both males and females, the earnings drop of the treated observed in 1993 was further deepened as they enrolled in AE 1994 and 1995. This had been recovered by the year 2000 to exceed the non-treated averages and the treated-untreated earnings gap then gradually increased until 2007. The overall decline in earnings at the end of the period reflects an increase in the proportions retiring from the workforce.

2.C The content of adult education

Table 2 describes the content of AE for our treated sample. We have access to detailed information on course registrations as well as the grades obtained (but not attained diplomas). Each course is associated with a number of credits and we summarize each individual's registrations expressed in years of full time studies. A course is defined as "completed" only if the grade is "pass" (or higher). If the grade is lower and/or if an interruption is reported, the course is considered as not completed. With this definition, only about half of the registered courses were completed, and only around one third of the compulsory level courses, indicating that initial skill level is associated with completion. The total average of completed studies is .36 years for treated males and .60 years for females.

[8] Averages amounts are overall low as about 75 per cent of the individuals received zero benefits.

[9] The male earnings trajectories of both treated and untreated already had dropped before 1993. This reflects the severe recession between 1990 and 1993 when unemployment rates in Sweden soared from 2.1 to 11.3 percent (ILO definition) before the situation stabilized, 1994-1996. An economic recovery saw unemployment rates decrease to 5 percent in 2000, and then hover between 5 and 7 percent for the remainder of the period.

About one third of the treated were at some stage registered at compulsory level; about 75 percent at upper secondary level and about 7 percent completed some education at tertiary level. Of those registered at the lowest (compulsory) level, about 58 percent were at some point also registered at upper secondary level. The supplementary courses at Komvux are officially classified as tertiary level courses and are accordingly included in the reported amount of completed tertiary level education. Otherwise, tertiary education is conducted outside Komvux where our records do not cover registrations, only completed courses.

Figure 3 shows a histogram of completed AE among treated. Many participants only completed a few course credits. About 27 percent completed no courses at all while 38 percent completed more than .25 years of AE, corresponding to roughly ten weeks of full-time studies, and 18 percent of the treated completed more than one year of AE.[10] Interestingly, among those with zero completed courses, the average amount of registered course credits (.225 years of AE) is almost identical to the group completing credits > 0 but < .25 years of studies (.217). This is important from a cost-benefit perspective as the potential benefits emerging from completed courses must be compared with the cost of the total supply of courses.

3 Empirical strategy

To estimate the average effect of AE on earnings, our empirical strategy is to use difference-in-differences outcomes where treated individuals are compared with comparable untreated individuals by nearest-neighbor matching on the propensity score (Rosenbaum and Rubin, 1983, Smith and Todd 2005). The approach is described in more detail in Section 3.A. In Section 3.B, we discuss two alternative specifications which we use for the matching procedure.

[10] This is considerably less than what Stenberg (2011) reported for AE enrolees 1994-1995 aged 24-43, where about 50 percent completed at least one year of full time studies.

3.A Difference-in-differences propensity score matching

Our outcome of interest is defined as $\Delta Y_{it} = (Y_{it} - (Y_{i1992}+Y_{i1991}+Y_{i1990})/3)$ where Y_{it} denotes annual earnings for an individual *i* in a given year *t* after treatment. Comparing these differences between treated and untreated, we control for unobserved time invariant heterogeneity across individuals (fixed effects). Except for such heterogeneity, the empirical approach assumes a selection mechanism into AE based on observables. The propensity score is obtained for each individual by fitting a probit model to estimate the probability of enrolment, given pre-AE observed covariates. We select four untreated individuals with propensity scores closest to those of each given participant in AE, and a weighted average outcome of these represents the counterfactual. An advantage of the matching procedure is that it allows us to explicitly control how weights are attached to members in the comparison group. One may thereby obtain estimates of the average treatment effect of the treated (*ATT*) even if the effects of AE are heterogeneous across individuals.[11] To identify causal effects of AE on earnings it is necessary to assume that, conditional on the covariates, any remaining mechanisms affecting the decision to enroll in AE must be independent of future outcomes (further discussed in Section 3.B). This makes it inappropriate to condition on events occurring after 1994-1995 and we therefore allow that individuals in the comparison group enroll in AE from 1996 or later. [12] Further assumptions include that the probability to enroll in AE must be strictly positive and, for the estimates to reflect ATT, an individual's enrolment in AE must not influence outcomes for the other individuals in the study (the stable unit treatment value assumption, SUTVA, or no interference assumption). This condition is further discussed in Section 5.

[11] In contrast, the implicit weights of the OLS estimators are proportional to how often a value of an explanatory variable occurs and to the variation in treatment for this value. Assuming heterogeneous treatment effects, it generates a weighted average which is not necessarily of interest to a researcher (Angrist 1998, Cobb-Clarke and Crossley 2003).

[12] The matched comparisons completed on average .07 years of AE, corresponding to less than three weeks of studies. The results presented in Section 4 remain very similar if one conditions the comparisons to have zero registrations in AE.

3.B Two model specifications

Due to potential transitory shocks prior to AE (Ashenfelter's dip), the pre-AE earnings in our difference-in-differences outcome is defined as the average earnings 1990-1992, i.e. the last observation is at least two years prior to AE enrolment. In addition, we present estimation results from two model specifications; our "benchmark model" disregards changes occurring 1992-1993 and 1993-1994, whereas our "extended model" explicitly accounts for these in the matching procedure. The rationale for using the two models is that temporary and/or permanent changes prior to treatment may be systematic, potentially causing biased estimates.

With the benchmark model specification, the covariates on which we balance are employment sector (7 categories), age (14 categories), prior education (7), number of children at home (6), age of children (6), rural or metropolitan area (3), regional employment level, pre-treatment annual earnings and earnings trajectories (levels 1982 – 1992), as well as five different kinds of social insurance benefits related to unemployment insurance (UI), parental leave, sick-leave, early retirement pensions and social welfare, applying both dummy variables (zero earnings, incidence of the various benefits) and continuous measures of amounts in SEK in 1992. By construction, annual earnings is a composite variable encompassing both productivity and probability of employment as it reflects hourly wage levels multiplied by the number of hours worked. In all, balancing treated and untreated on these covariates should provide a credible control for systematic differences in labor market productivity as measured in 1992, and trends prior to 1992.

Table 3 presents probit estimates of AE enrolment for males and females, using both the benchmark model and the extended model specifications.[13] The matching procedures turn out to generate samples of matched comparisons which are well balanced with the treated individuals. Tests for the

[13] We excluded variables from our probit estimations if they were associated with *p*-values above .2 unless they were essential for our balancing tests. Including irrelevant covariates in the set-up of the matching procedure has been shown to increase the bias and/or variance of matching estimators (Caliendo and Kopeinig 2008, Waernbaum 2008).

null hypothesis of equal means, based on the benchmark model, are presented in Table 1 and generally indicate *p*-values at .30 or higher (to save space, not all variables are included, see note *b)* of Table 1). This pattern holds throughout as we vary the definition of treated in the next section. The exception is, of course, that when using the benchmark model, changes in earnings 1992-1993 and social insurance benefits 1992-1993 and 1993-1994 remain unbalanced, in particular for males.

If there are permanent effects of changes ("shocks") prior to AE enrolment, the benchmark model captures beneficial effects of AE minus negative effects of changes which are unbalanced between treated and matched comparisons. Considering e.g. the drop in earnings among treated, observed in Table 1, and that layoffs following employment fluctuations have been reported to have persistent effects on average earnings (Eliason and Storrie 2006, Schmieder and von Wachter 2010), our benchmark estimates will tend to be conservative.

In our extended model specification, covariates are added to explicitly account for changes prior to AE enrolment. Theoretically, participation in AE should be related to some form of change in conditions (examples given in the introduction). Therefore, we now control for changes in social insurance benefits 1992-1993, and 1993-1994, regarding sick-leave, social welfare, early retirement pensions, as well as changes 1992-1993 in UI payments and annual earnings.[14] To see how one would expect this to affect our results, it is useful to make an admittedly simplified distinction between demand side changes – one can think of shocks outside the control of the individual, such as plant closures due to changes in world market prices – and supply side changes – related to individual behavior e.g. deteriorating health and/or motivation. Both types of changes imply decreased earnings

[14] In accordance with Heckman and Smith (1999), our extended models also balance treated and untreated on transitions in labor force status 1992-1993. In total this concerns nine different transitions between outside the labor force, employment and unemployment. For reasons of space, these variables are not included in Table 1, but some of them significantly explain participation in AE (see Table 3). Including benefit payments received in 1994 among covariates is potentially incorrect if AE causes increased sick-leave, early retirement or social welfare in the first year of enrolment for those enrolled in 1994. Excluding these variables has only a minor impact on our estimates in Section 4.

and lower opportunity costs of AE, but may differ in their long-run consequences on earnings.[15] In the typical case of an Ashenfelter's dip, temporary demand side changes are more common among participants in a program (Ashenfelter 1978 and e.g. Heckman et al. 1999, Heckman and Smith 1999). The estimates are then upward biased as negative long-run consequences would dominate among the matched comparisons. Such an assumption is untestable and we therefore find it justified to compare the estimates of the extended model with the plausibly conservative benchmark estimates.[16]

4 Results

The results presented in this section concern the benchmark model and the extended model specifications presented in Section 3. Section 4.A contains the main results. It is followed in Section 4.B by estimates pertaining to groups with different amounts of completed AE. Gender differences are addressed in Section 4.C.

4.A Main results

Figure 3 and Figure 4 present trajectories of average annual earnings for matched samples of males and females, both of the benchmark model and the extended model. Figure 3 concerns the full sample of treated, whereas the definition of treatment in Figure 4 is restricted to those who completed at least one year of AE. Treated-untreated trajectories are virtually identical from 1982 until 1992 before the averages of the treated drop in 1993, and as AE enrolment occurs 1994-1995. The drop in earnings from 1994 and onwards gives an indication of the individuals' opportunity costs of AE and also, to the extent that it reflects foregone production value, an indication of the indirect costs of AE for the society. These costs appear substantial as the trajectories of treated males never catch up with those of the matched comparisons, and treated females only exceed the level of their matched

[15] For example, the balance on changes in sick-leave transfers is not adequate if it reflects dripping noses among treated but hip, back or shoulder problems with long term implications among untreated (or vice versa).
[16] To be precise, if treated and matched comparisons differ in the background causes of the changes, and our covariates fail to capture this, the extended model may entail a risk for upward *or* downward biased estimates.

comparisons from 2001 onwards. The gaps between these trajectories are reflected by our difference-in-differences propensity score matching estimates. Consequently, these estimates are generally below zero until in 2001 or later years. The discussion in this section primarily concerns estimates from the latter part of the period, while the initial negative effects are taken into account in Section 5 as we consider the costs of AE.

Table 4 present difference-in-differences estimates from 1996 to 2007, separately for males and females. For the full sample of treated, results from the benchmark model are presented in columns (1) and the extended model in columns (2), whereas columns (3) show the extended model estimates applied on treated who completed at least one year of full-time AE. For each column, a new matched comparison group is created (as described in Section 3). Graphical expositions are presented in Figures 5 and 6, where the grey lines represent the extended model estimates. For males, the point estimates remain negative throughout, although not significantly different from zero from 2001 (extended model). This may reflect an absence of effects of AE or that any positive effects are offset by negative long-run consequences of pre-program changes and/or locking-in effects of AE if it detaches individuals from the labor market. As expected, the extended model yields more positive results than the benchmark model, but the discrepancies are much larger for males. This is because the benchmark model to a greater degree fails to balance male treated and matched comparisons on changes 1992-1993 and 1993-1994, in particular on increases in sick-leave benefits (see Table 1). The pattern is very similar to Heckman and Smith (1999) who compared an experimental control group from the U.S. Job Training Partnership Act (JTPA) with an untreated comparison group derived from non-experimental data. They found that taking into account labor force transitions prior to enrolment substantially reduced downward bias in estimates for males (from -$5,228 to -$450).[17]

[17] As with our results, their estimates pertaining to females were relatively insensitive to model specification. With a small upward bias remaining (not statistically significant from zero), they hypothesized it could reflect family characteristics which were not measured well in their study. When we exclude controls for the number of children at home in 1992 and the age of the children (six categories each), we find estimates 2002-2007 to increase by on average 8 percent.

For the sample of females, the estimates for treated females are positive and significantly different from zero from 2001 and onwards. For females completing at least one year of AE, the extended model estimates for 2002-2005 correspond to between 8 and 11 percent of the average earnings of the untreated comparisons, increasing to about 13 percent in 2006 and 2007. Rudimentary estimates of the earnings returns to a year of AE may be given by dividing these percentages with the average amount of completed studies, 2.04 years.[18] These are between 3.8 and 5.3 percent in 2002-2005 before increasing to above 6 percent in 2006 and in 2007.

4.B Different exposures to treatment

As discussed in Section 2, the majority of the treated completed less than .25 years of AE. Figure 7a through 7m give difference-in-differences estimates for samples of treated with different amounts of completed credits , from zero accomplished credits (Figures 7a and 7b) to treated who completed some tertiary level courses (*k* and *m*).

One would not expect zero completed credits to have any causal effects on earnings. For males, the results of the extended model are insignificant with point estimates close to zero, whereas the benchmark model yields significantly negative estimates through the whole period. The estimates for females hover around zero, regardless of model specification.

For the group with credits > 0 but < .25 years of AE, the average completed AE is below .10, corresponding to less than four weeks of full time studies. Again, one would not necessarily expect such low intensity treatment to have noticeable causal effects. However, we find positive point estimates, statistically insignificant for males but significantly above zero for females. It is possible that there are short courses with a significant payoff on earnings, but the proportion registered in vocational courses, including subjects related to health care, is only at 8 percent, compared with 62 percent

[18] For a year *t*, the formula could be written [(*ATT* / average earnings of untreated comparisons)$_t$ / average AE completed].

among treated completing > .25 years. The group is instead overrepresented in English courses, perhaps indicating "consumption" of AE, i.e. that courses are used to enhance the utility of leisure rather than of work.[19] The sample of treated (0 < AE < .25 years) is particular on two accounts; the earnings level 1982-1992 is 10-20 percent higher than other treated and their earnings drop in 1994 and 1995 is only 3 percent and completely recovered by 1996. It may be that many of these individuals received a job offer shortly after enrolling and that the positive estimates pertaining to females with >.25 but <.50 years of AE probably capture a weaker but similar mechanism (Figure 7 *f*). We also note that our ATT estimates for females display the expected increasing pattern for treated females with >.50 but < 1 year of AE (Figure 7 *h*), females completing at least one year of AE (no tertiary Figure 7*j*) and females with some tertiary level education (Figure 7 *m*).

A plausible explanation for this U-shaped association between credits and the earnings return is that job-search decisions are correlated with decisions to enroll AE (see also Heckman and Smith 1999). Although there are untreated who search for jobs as well, it seems likely that AE enrollees who job search may partly be matched with comparisons who do not search for a new job. If there is an effect of job search in itself (e.g. a mobility premium), it may bias our estimates upward if the decision to search for a new job influences the decision to enrol in AE, e.g. following a lay-off. [20] On the other hand, a payoff to job search can also be seen as an indirect effect of treatment, as access to AE may influence the propensity to search, and since stimulating job mobility is one of the main purposes of the public provision of AE. This should occur if AE is expected to ultimately increase the probability of finding a job, and perhaps also if AE prevents depreciation of human capital or social marginalization as unemployed. In Section 5, we discuss what our assumptions in this regard imply for the cost-benefit analysis.

[19] The "core subjects" at upper secondary level (English, Swedish and mathematics) overall attract similar fractions of the treated, 50, 42 and 43 percent. In the group with > 0 but < .25 years of AE, however, the proportions are 31, 12 and 12 percent.

[20] Note that this problem would remain with quasi-experimental data. In the case of randomly distributed AE vouchers, job-searchers could still be more likely to use the vouchers. Only by randomly rationing seats in AE among individuals already interested in attending AE is the proportion of job-searchers equal between treated and untreated.

Whether one or the other of the two interpretations is valid, an earnings effect of job search in itself could make the relation between our estimates and the amount of completed AE ambiguous. Basically, this depends on how the expected marginal returns to job search and further investment in AE evolve during participation. If the most productive enrollees also tend to be the best job searchers, it is possible that they will drop out of AE at an early stage as they receive and accept job offers. While the remaining participants complete more course credits, good job searchers continue to leave AE to accept job offers, but less frequently because the remaining participants are on average less productive job searchers than the very early drop outs. Moreover, reservation wages may increase as the investments in AE approach levels at which expected wage offers improve.[21] When the amount of AE reaches this level, the participants consist of individuals whose future earnings are less likely to be affected by their job search skills and/or intensity, i.e. only the direct effect of AE remains.

Now, we fully acknowledge that the above gives a stylized and somewhat pessimistic view of participants completing a large number of credits, but it is at least partly consistent with a positive payoff to "just showing up" and the non-monotonic point estimates reported in Jacobson et al. (2005a, 2005b), Stenberg and Westerlund (2008) and Stenberg (2011).

4.C Gender differences

Larger estimated effects for females are standard in the literature on active labor market program evaluations, but the underlying reasons behind such results are rarely addressed. By scrutinizing the data presented in Section 2, one finds that treated males to a greater degree than treated females are associated with increases in sick-leave benefits in the years preceding enrolment (see Tables 1 and 3). One hypothesis is that males who enroll in AE tend to be closer to some form of labor market marginalization, which potentially offsets any positive earnings effects of AE. Treated females, on the

[21] The hazard function for exit from education to employment then has a negative slope for short spells in AE. Again, note that even if a social experiment generates perfect random assignment to AE, estimates of *ATT* pertaining to early dropouts would be positive to the extent that there is a random element in the assignment to jobs with good earnings prospects.

other hand, may have had a higher latent demand for AE if they in earlier years were subject to restrictions due to household responsibilities. Children at home in 1992 might be an (albeit blunt) indication of restrictions which have prevented earlier AE enrolment. [22] Results displayed in Figure 8 pertain to females with less than two children (a) and two children or more (b), completing at least .5 years of AE. For the sample of females having less than two children at home in 1992, the estimation results are barely significantly different from zero (only 2006 and 2007), whereas strong positive effects are found for females with at least two children.[23] The average difference in the point estimates for 2002-2006 corresponds to about 5 percent in terms of their annual earnings. We also find that females with zero or one child in 1992 were more associated with increases in sick-leave benefits prior to AE enrolment in 1993-1994 (also 1992-1993 for the subsample with no children). Hypothetically, it is possible that the positive estimates reflect labor supply effects following child-rearing. [24] To prevent such a mechanism, the matched comparison groups are balanced on the number of children at home (six categories), the age of children (six categories), earnings each year 1982-1993, and changes in benefits 1992-1993 and 1993-1994.[25] A time trend in the labor supply therefore seems unlikely to be a confounding factor. However, one should here note that it is difficult to entirely separate the hypothesized effect from heterogeneous effects related to prior earnings. Stenberg and Westerlund (2008) and Stenberg (2011) have previously reported high returns to AE for individuals with low earnings. [26] Nevertheless, the results hint at an important policy implication as child rearing females could be a suitable target group for educational investments aiming to enhance the production potential. In countries with lower female labor force participation than in Sweden (where it is similar to males), the corresponding effect could be smaller if females tend to be unwilling to enter

[22] A similar mechanism, but related to young mothers with small children, has been discussed in earlier work (Stenberg 2007b, p15, Bergemann and van den Berg 2008).

[23] The average amount of completed credits is similar between the treated groups, corresponding to 1.62 vs 1.53 years. When we abstain from the restriction of at least .5 years of completed AE, to include full samples of treated, the gap in point estimates is reduced by almost one third. This is consistent with the hypothesis of an inherent demand for AE as we then introduce effects of job search which presumably are unrelated to the actual human capital investment.

[24] "Labor supply" is a somewhat inaccurate term since hours of work may also reflect the probability of finding employment under labor market excess supply, or in job search equilibrium under imperfect information.

[25] As an additional control for family situation, the samples are balanced on disposable income, which gives males and females equal weights of household income.

[26] Based on total earnings measured 1982-1990 among treated females, the share with above median earnings among treated females with two children or more was 26 percent, while 58 percent if with zero or one child.

the labor market, but it may also be larger if AE increases labor supply. Importantly, we emphasize that our results are only suggestive on this point and further research is needed before strong policy recommendations can be made.

5 Costs and benefits to society

To assess what our results imply for policy, we compare the total benefits of AE with approximations of the direct and indirect costs incurred by society. As always, this kind of exercise is by necessity based on several untestable assumptions and should be interpreted with caution.

The direct costs are calculated as the product of the total amount of course registrations among treated, multiplied by the average costs as stated by the responsible public agencies. The benefits are assumed equal to the ATT estimates for participants completing at least one year of AE. We extrapolate the earnings gains recorded in 2007 until individuals are 64 years old, meaning that the stream of benefits gradually shrinks as the number remaining in the workforce decreases, and eventually becomes zero in 2017 when the youngest cohort turns 65. Both benefits and indirect costs (forgone earnings) are multiplied by 1.4 to include payroll taxes and better represent a production value. Further, we assume no crowding out effects of AE (see Dahlberg and Forslund 2005) and a discount rate of 3 percent. We then find that the present value of the benefits covers the direct costs by 2014. However, when adding indirect costs in the shape of a deadweight loss of 20 per cent, and society's loss in terms of foregone production, only 30 percent of the total costs are covered by the time the youngest cohort retires (35 percent if we include treated with at least .50 years of AE). [27]

With one reservation, our conjecture is that the costs of AE for older workers were not covered. Our reservation concerns the possibility that we underestimate the returns to AE when we disregard

[27] We assume the average probability of a non-employed replacement is .35, which is half the employment rate (.70) in the older segment of the low and medium skilled population (considered as the upper bound). The stable unit treatment value assumption (SUTVA) assumes a program has no influence on non-treated's number of hours worked. In this case, earnings losses are an appropriate measure of foregone earnings. The other extreme is to assume *all* vacancies are replaced by non-employed, reducing foregone production to zero (Johnson and Layard 1986).

the positive earnings payoff for individuals with short stints in AE (notably below .50 years). If one interprets these as indirect effects of AE, the average returns in Table 4 are slightly reduced but concern a much larger sample of treated. It increases the total returns to society such that 90 percent of the costs are covered by the time the youngest cohort turns 65. For AE to exceed its costs in a non-trivial manner, which is necessary if one wishes to claim that it is the most efficient use of public funds, one also needs to assume non-pecuniary welfare effects of AE on the society as a whole. In the present case, these are required to increases the returns to society by a factor of about 1.5, or by about 2 if one assumes that half of the job-search effects are related to AE. Such effects are typically difficult to measure, but some studies have suggested substantial external effects of education. Calculations in Albrecht et al. (2009) indicate general equilibrium effects of AE in Sweden which make social returns exceed private returns by a factor between 1.5 and 2, following job-composition effects and externalities on productivity. External effects of education have also been reported on productivity (e.g. Moretti 2004, Kirby and Riley 2008) and health (Cutler and Lleras-Muney 2008). Welfare effects could also encompass aspects such as equity, democracy and social cohesion.[28] In sum, to argue that benefits exceed the costs, we believe one must infer that there are substantial benefits of AE from its effect on stimulating job search and from its external effects on society.

6 Summary and discussion

Increasing public investment in formal education of older workers has many proponents among policy makers in North America and Europe, and there are seemingly good economic reasons for the strong interest in this measure. In addition, public policies targeting low skilled may be necessary for upgrading their qualifications as on-the-job-training mostly falls to high skilled. Low skilled may lack access to on-the-job training because of employers' preferences and/or because low skilled might be unwilling to participate due to a perception of low returns and/or financial constraints (Oosterbeek 1998, OECD 2003, 2006, Schwerdt et al. 2011). However, our results do not support that AE of older

[28] A negative side-effect is that the possibility of AE enrolment may cause adolescents to drop out of school prematurely. While this does not regard the sample of our study, it is a cost to society as it may distort incentives.

workers would alleviate the economic effects of an aging population, neither in terms of an increased tax base, nor in terms of net effects on total real resources. In order to justify the investments, the unmeasured indirect effects of AE through stimulating job search and positive externalities would have to be fairly large, arguably beyond what one may find plausible. In combination with earlier evidence suggesting that AE has no effect on the timing of retirement (Stenberg *et al*. 2011), the impression is strengthened that policymakers may be overly optimistic concerning the average effects on productivity and labor supply of education for older workers.

Our results indicate positive effects of AE on earnings for females, but insignificant effects for males. The gender gap in our estimates may stem from different underlying reasons for enrolment. Male participation is associated with increased levels of sick-leave benefits shortly before enrolment, while females appear to some extent to have been driven by a latent demand for AE which has earlier been suppressed by household responsibilities. This suggests that females who were previously restricted by e.g. child rearing might constitute a suitable target group in countries where one seeks to expand and/or introduce formal education for adults. Generally, the results in this paper have to be interpreted with care, and in the light of the two following comments. First, the study concerns AE which targets low and medium skilled. Human capital investments at the tertiary level may have different implications than those reported here. Second, most countries in the OECD are currently reviewing and reforming pension systems, which may have implications for the effects of adult education on earnings. The individuals in our sample, born 1939-1952, are part of two overlapping pension schemes. The new scheme makes earnings later in life more important for the level of pension entitlements. If it has important effects on work incentives, it could also influence the incentives to exploit earnings enhancing potential of formal education, even if completed at the age of 42 or higher. Unfortunately, education for older workers is a hitherto neglected area of research and we know little about the importance of the surrounding institutional set up.

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Table 1; Descriptive averages as measured in 1992 unless otherwise stated, of treated and untreated, including benchmark model matched comparison groups. Amounts given in SEK 2004 prices, in thousands, where € 100 is approximately SEK 970.

	Males					**Females**				
	AE	No AE	*p*-value[a]	Matched comparisons	*p*-value[a]	AE	No AE	*p*-value[a]	Matched comparisons	*p*-value[a]
Total completed AE (years)	.363	.019	*.000*	.028	*.000*	.600	.055	*.000*	.091	*.000*
Age in 1994 [b]	46.88	48.41	*.000*	46.91	*.792*	46.65	48.74	*.000*	46.64	*.871*
Born 1952	.121	.067	*.000*	.117	*.697*	.127	.058	*.000*	.124	*.619*
Born 1939	.027	.061	*.000*	.026	*.826*	.022	.071	*.000*	.021	*.726*
No. of children at home [b]	1.27	1.09	*.000*	1.27	*.911*	1.40	.98	*.000*	1.40	*.806*
Years of schooling [c]	9.90	9.43	*.000*	9.94	*.400*	9.99	9.58	*.000*	10.00	*.701*
Less than 9 years of schooling	.229	.403	*.000*	.215	*.302*	.150	.303	*.000*	.144	*.469*
9 years of schooling	.197	.174	*.006*	.202	*.715*	.177	.172	*.366*	.180	*.714*
- Junior secondary school	.158	.138	*.009*	.167	*.423*	.121	.119	*.595*	.058	*.863*
1 year of upper secondary school	.016	.009	*.000*	.014	*.497*	.207	.164	*.000*	.209	*.879*
2 years of upper secondary school	.558	.414	*.000*	.569	*.460*	.466	.361	*.000*	.467	*.914*
- Commerce	.062	.053	*.076*	.062	*.974*	.221	.163	*.000*	.224	*.775*
- Manufacturing	.364	.256	*.000*	.367	*.819*	.041	.028	*.000*	.039	*.680*
- Transport	.047	.034	*.002*	.053	*.375*	.025	.025	*.897*	.026	*.811*
No sector reported [d]	.033	.043	*.030*	.041	*.162*	.034	.070	*.000*	.034	*.882*
Farming and mining [d]	.036	.065	*.000*	.033	*.619*	.016	.021	*.012*	.016	*.914*
Construction [d]	.135	.157	*.006*	.138	*.810*	.024	.016	*.000*	.023	*.709*
Manufacturing [d]	.216	.272	*.000*	.217	*.962*	.091	.129	*.000*	.091	*.978*
Finance, insurance [d]	.083	.077	*.291*	.084	*.898*	.097	.098	*.848*	.097	*.964*
Public sector [d]	.116	.075	*.000*	.118	*.883*	.487	.414	*.000*	.485	*.867*
Other sector [d]	.381	.311	*.000*	.370	*.459*	.250	.252	*.814*	.254	*.714*
Earnings 1982 [b]	161.9	163.0	*.924*	159.6	*.385*	87.9	90.0	*.480*	86.8	*.445*
Earnings 1988 [b]	189.1	188.5	*.816*	188.0	*.737*	115.6	114.7	*.397*	115.4	*.897*
Earnings 1992 [b]	177.9	178.8	*.709*	175.4	*.497*	131.0	127.0	*.001*	131.5	*.753*
Zero earnings 1992	.131	.141	*.210*	.145	*.186*	.091	.130	*.000*	.086	*.394*
Unemp. insurance (UI) > 0	.112	.079	*.000*	.110	*.851*	.114	.066	*.000*	.111	*.682*
– average amount if >0	55.2	54.5	*.764*	54.8	*.893*	32.4	34.3	*.145*	32.1	*.862*
ALMP benefits > 0	.082	.036	*.000*	.080	*.829*	.054	.026	*.000*	.052	*.691*
– average amount if >0	33.8	29.2	*.038*	33.8	*.999*	28.8	28.3	*.788*	30.9	*.309*

Table 1; Continued.

Parental leave benefits > 0	.026	.015	*.000*	.027	*.864*	.030	.013	*.000*	.028	*.723*
– average amount if >0	11.2	13.4	*.443*	15.5	*.214*	24.4	23.3	*.712*	22.6	*.560*
Sick-leave benefits > 0	.260	.221	*.000*	.263	*.844*	.279	.273	*.342*	.272	*.454*
– average amount if >0	30.4	28.5	*.371*	32.0	*.503*	20.3	22.7	*.024*	19.9	*.722*
Early retirement > 0	.025	.052	*.000*	.031	*.264*	.034	.094	*.000*	.033	*.869*
– average amount if >0	68.0	77.5	*.032*	69.3	*.778*	56.2	64.9	*.000*	54.7	*.521*
Social welfare benefits > 0	.098	.049	*.000*	.096	*.832*	.075	.050	*.000*	.072	*.562*
– average amount if >0	14.3	10.5	*.002*	14.4	*.956*	8.3	7.7	*.408*	7.4	*.292*
Regional employment	.724	.722	*.000*	.724	*.630*	.723	.722	*.063*	.723	*.654*
Inland of Norrland [e]	.049	.062	*.016*	.049	*.927*	.053	.054	*.727*	.050	*.550*
Stockholm county [e]	.160	.132	*.000*	.159	*.880*	.140	.136	*.391*	.142	*.786*
Earnings change 1992-1990	- 12.7	- 8.3	*.005*	- 12.3	*.874*	.1	.2	*.915*	.3	*.810*
Changes in amounts 1993-1992										
Earnings [f]	- 16.3	- 9.6	*.000*	- 8.7	*.001*	- 7.5	- 5.2	*.000*	- 4.4	*.000*
UI benefits [f]	4.6	2.5	*.000*	2.9	*.019*	4.0	1.4	*.000*	1.8	*.000*
Early retirement [f]	1.8	3.9	*.000*	2.7	*.057*	2.6	5.6	*.000*	2.8	*.604*
Sick-leave benefits [f]	2.3	- .4	*.000*	- 1.1	*.000*	- .3	- 1.2	*.001*	- .6	*.399*
Social welfare benefits [f]	.5	.1	*.000*	.1	*.040*	.3	.1	*.000*	.1	*.032*
Changes in amounts 1994-1993										
Early retirement [f]	- .3	- 1.9	*.000*	- .5	*.633*	- 1.5	- 3.2	*.000*	- .9	*.023*
Sick-leave benefits [f]	2.0	.1	*.000*	- .1	*.004*	.9	.1	*.003*	.3	*.172*
Social welfare benefits [f]	.1	-.0	*.109*	- .2	*.148*	.1	-.0	*.034*	- .0	*.148*
N	2015	339654		7868 (weighted)		4296	290349		16415 (weighted)	

[a] *t*-test for equality between average of treated and untreated matched comparisons.

[b] Balancing tests also include age dummies (14 categories), dummies for number of children (six categories), age of children (six categories) and annual earnings each year 1982-1992. To save space, these are not displayed. Please note that all variables are balanced when the extended model is applied.

[c] To obtain years of schooling, "less than the 9 years of schooling" is assumed to be 8 years, and one and two years of upper secondary school 10 and 11 years.

[d] If sector is not reported in 1993, we use the latest reported sector from previous years, back to 1990.

[e] 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.

[f] Variable is balanced between treated and matched comparisons when the extended model is applied (not displayed).

Table 2. Content of adult education (treated individuals only). Credits are expressed in years of full-time studies.

	Males	Females
N	2015	4296
Total registered course credits at Komvux (years)	.568	.890
Total completed course credits at Komvux (years)	.295	.525
Proportion registered in compulsory level courses	38.0 %	34.2 %
Registered compulsory credits, average	.144	.133
Completed compulsory credits, average	.042	.046
Completed compulsory credits, if registered at level	.110	.134
Proportion registered in upper secondary level courses	68.2 %	78.7 %
Registered upper secondary credits, average	.392	.712
Completed upper secondary credits, average	.243	.468
Completed upper secondary credits, if registered at level	.357	.595
Proportions in type of upper secondary course registration		
- English	38.2 %	44.5 %
- Swedish	28.2 %	30.9 %
- Mathematics	33.5 %	29.5 %
- Social sciences	48.8 %	66.0 %
- Natural sciences	14.0 %	14.5 %
- Human sciences (e.g. foreign languages)	13.2 %	16.7 %
- Computer sciences	64.4 %	66.6 %
- Health-related subjects (e.g. nursing)	12.3 %	35.9 %
- Vocational courses	12.7 %	6.7 %
Proportion registered in supplementary level courses	5.0 %	4.6 %
Registered supplementary level credits, average	.017	.020
Completed supplementary level credits, average	.010	.011
Completed supplementary level credits, if registered at level	.204	.251
Proportion completing some tertiary level education	7.3 %	7.2 %
Completed tertiary education, average	.078	.086
Completed tertiary education, if registered at level	.925	1.029
Total adult education completed (years)	**.363**	**.600**

Table 3: Maximum likelihood estimation of a probit model for the propensity score. [a)]

Dependent variable: AE course registration in AE 1994-1995.

	Males				**Females**			
	Benchmark		Extended model		Benchmark		Extended model	
	Coefficient	Std.Err.	Coeff.	Std.Err.	Coefficient	Std.Err.	Coeff.	Std.Err.
Less than 9 years of schooling	-.188***	(.027)	-.193***	(.027)	-.194***	(.019)	-.200***	(.019)
9 years of schooling	-.067**	(.029)	-.072**	(.029)	-.103***	(.021)	-.110***	(.021)
- Junior secondary school					.137***	(.032)	.139***	(.032)
1 year of upper secondary school								
2 years of upper secondary school								
- Commerce	- .049	(.039)	- .049	(.039)	.062***	(.017)	.061***	(.018)
- Manufacturing	.052**	(.026)	.050***	(.026)	.107***	(.034)	.104***	(.034)
- Transport								
Farming and mining	-.210***	(.066)	-.182***	(.066)	-.002	(.058)	.013	(.059)
Construction	-.110*	(.059)	-.106*	(.057)	.238***	(.055)	.250***	(.056)
Manufacturing	-.108*	(.058)	-.082	(.056)	-.021	(.041)	-.000	(.043)
Finance, insurance	-.031	(.061)	-.009	(.060)	.065	(.042)	.090**	(.043)
Public sector	.074	(.060)	.103	(.058)	.107***	(.037)	.147***	(.039)
Other sector	.035	(.056)	.061	(.054)	.091**	(.038)	.116***	(.040)
Earnings 1992 [b)]	.075	(.143)			.175	(.111)	.175	(.111)
Δ earnings 1992-1990 [b)]					- .024	(.016)	- .039**	(.016)
Unemp. insurance (UI) > 0	.046	(.030)	- .057	(.037)	.215***	(.032)	- .021	(.036)
– amount of UI [b)]					-.099	(.073)	.028	(.081)
Parental leave benefits > 0	.052	(.054)	.051	(.054)				
ALMP benefits > 0	.131***	(.048)	.118**	(.049)	.167***	(.033)	.147***	(.033)
– amount [b)]	.216**	(.102)	.279*	(.104)				
Sick-leave benefits > 0	.038	(.021)	.018	(.216)				
– amount if above zero [b)]	.057*	(.032)	.187***	(.036)				
Early retirement pension > 0								
– amount if above zero [b)]	- .277***	(.071)	- .174**	(.085)	- .367***	(.054)	- .410***	(.063)
Social welfare benefits > 0	.152***	(.035)	.118***	(.036)	.103***	(.029)	.086***	(.030)
– amount if above zero [b)]	.352**	(.141)	.695***	(.154)	.338*	(.176)	.610***	(.188)
Regional employment	.939***	(.352)	.939***	(.354)	.988**	(.397)	1.035***	(.399)
Inland of Norrland 1992	- .069*	(.038)	- .072*	(.038)				
Stockholm county 1992					- .049*	(.027)	- .053*	(.027)
Transition 1992-93 OLF – unemp. [c)]			.433***	(.118)			.573***	(.109)
Transition 1992-93 emp. – unemp. [c)]			.159***	(.036)			.289***	(.028)
Transition 1992-93 emp. – emp. [c)]							- .090***	(.027)
Δ earnings 1992-1993			-.029**	(.015)			-.046**	(.020)
Δ UI benefits 92-93							.099*	(.057)
Δ early retirement benefits 92-93			-.067	(.072)			-.193***	(.068)
Δ sick-leave benefits 92-93			.231***	(.035)				
Δ social welfare benefits 92-93			.743***	(.149)			.943***	(.183)
Δ early retirement benefits 93-94							-.301***	(.088)
Δ sick-leave benefits 93-94			.147***	(.027)			.053	(.036)
Δ social welfare benefits 93-94			.620***	(.145)			.704***	(.174)

Notes: *** significant at the 1 % level. ** at the 5 % level. * at the 10 % level.

[a)] All regressions include a constant term, age-dummies and when relevant dummies for number of children (six categories) and age of children (six categories). These are not displayed for reasons of space.

[b)] SEK 2004 prices divided by 100,000, € 100 is approximately SEK 970.

[c)] OLF= outside labor force, a binary variable which takes the value one if annual earnings are below SEK 20,000 (app. $3,000) and individual received no transfers related to unemployment insurance or active labor market programs.

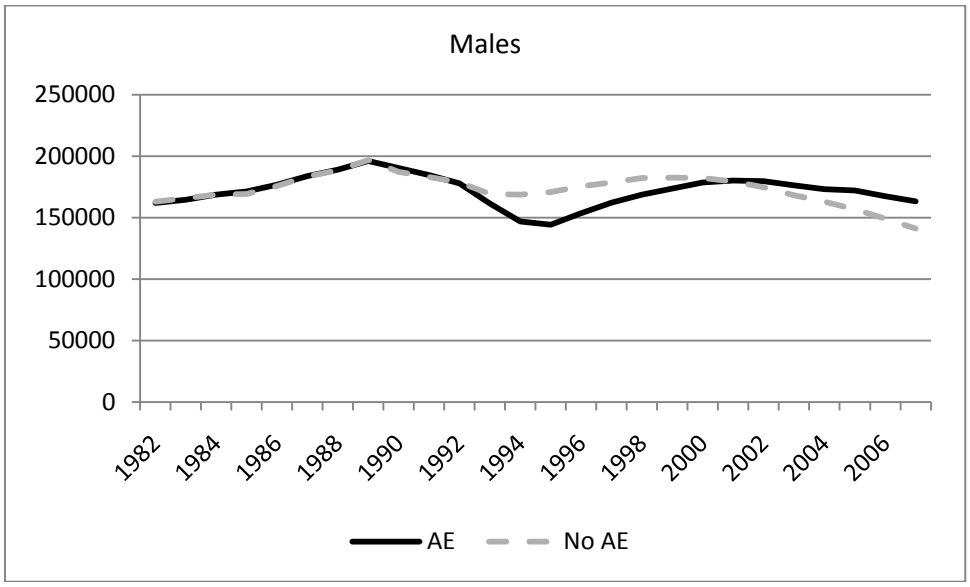
Table 4. Difference-in-differences propensity score matching estimates. Bootstrap standard errors based on 500 replications in parentheses.

Dependent variable: Earnings difference $\Delta Y_{jt} = (Y_{it} - (Y_{i1992}+Y_{i1991}+Y_{i1990})/3)$

	Males				**Females**			
	Benchmark model	Extended model	Extended model		Benchmark model	Extended model	Extended model	
	(1)	(2)	(3)		(1)	(2)	(3)	
Final year (*t*)	All AE	All AE	> 1 year	% [a)]	All AE	All AE	> 1 year	% [a)]
2007	-8302	-3684	-11531	-3.2%	12154	14087	17901	6.3%
	(3868)	(3769)	(10374)		(1702)	(1816)	(3426)	
2006	-7401	-3159	-218	-.1%	11270	12412	18577	6.5%
	(3534)	(3562)	(9714)		(1824)	(1901)	(3420)	
2005	-7502	-3923	-4900	-1.4%	9574	10203	15192	5.3%
	(3452)	(3490)	(9814)		(1768)	(1807)	(3319)	
2004	-8915	-4457	-3493	-1.0%	8209	9600	11772	4.1%
	(3225)	(3367)	(9809)		(1741)	(1794)	(3285)	
2003	-8177	-3075	-7388	-2.1%	8067	9726	14685	5.1%
	(3165)	(3174)	(9495)		(1633)	(1649)	(3322)	
2002	-8826	-3871	-15570	-4.3%	6656	8675	11069	3.8%
	(3114)	(3242)	(9218)		(1606)	(1577)	(3453)	
2001	-11304	-6883	-28259	-7.8%	4573	6995	4828	1.7%
	(3033)	(3101)	(9394)		(1578)	(1558)	(3393)	
2000	-13776	-9465	-50049	-13.9%	1059	3114	-2754	-1.0%
	(2939)	(2929)	(8864)		(1502)	(1549)	(3366)	
1999	-16545	-11502	-61043	-17.6%	-2882	-879	-11577	-4.2%
	(2850)	(2778)	(8306)		(1430)	(1440)	(3222)	
1998	-19458	-13647	-67035	-20.1%	-7217	-5729	-20423	-7.6%
	(2736)	(2731)	(8285)		(1324)	(1412)	(3007)	
1997	-21339	-16154	-67499	-21.5%	-13784	-11695	-32416	-12.5%
	(2628)	(2673)	(8132)		(1279)	(1306)	(2881)	
1996	-25343	-20316	-71134	-23.6%	-21438	-18911	-48340	-19.2%
	(2481)	(2636)	(7793)		(1157)	(1189)	(2604)	
$N^{TREATED}$	2015	2015	234		4296	4296	943	
$N^{UNTREATED}$ (weighted)	7669	7886	932		16265	16241	3687	

[a)] Percentage returns on earnings per year of completed AE, derived as [(estimate/average earnings of untreated comparisons)/average AE completed].

Figure 1. Annual earnings of males and females, 1982-2007; treated and non-treated.



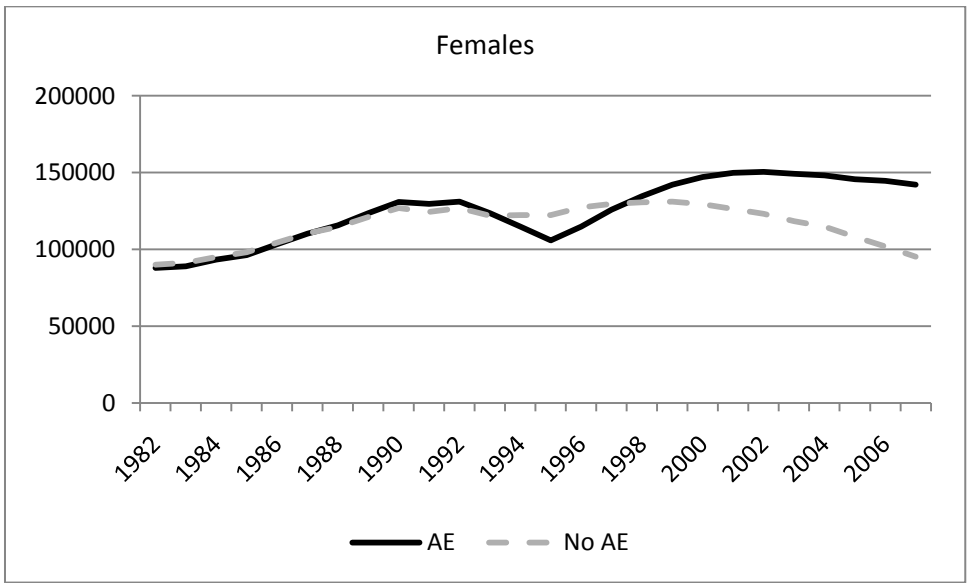
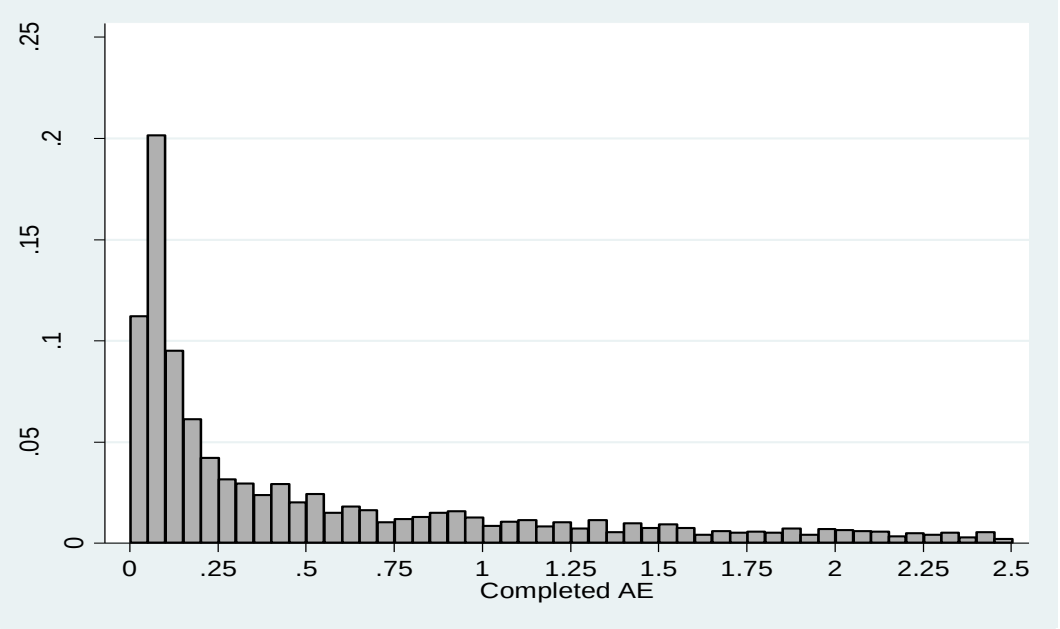
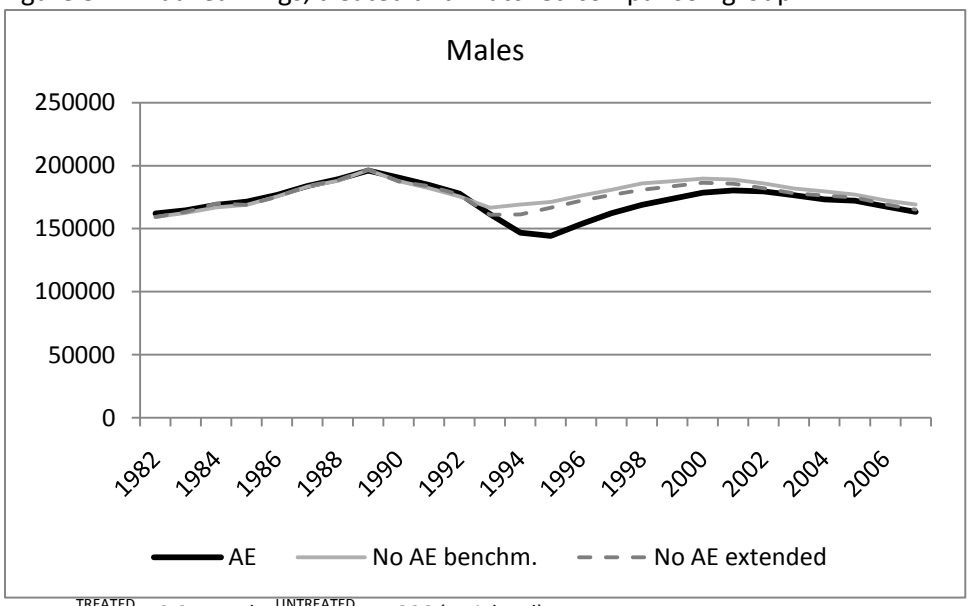


Figure 2. Completed AE among treated, expressed in years of full time studies.

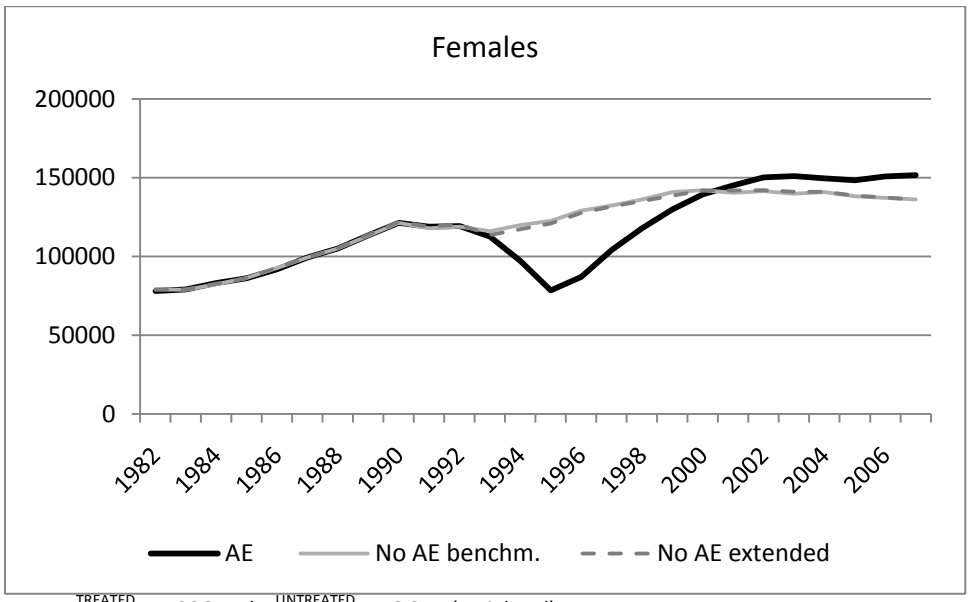


Note: For expositional reasons, 4.6 percent of the individuals with more than 2.5 years of AE are excluded.

Figure 3. Annual earnings, treated and matched comparison group.

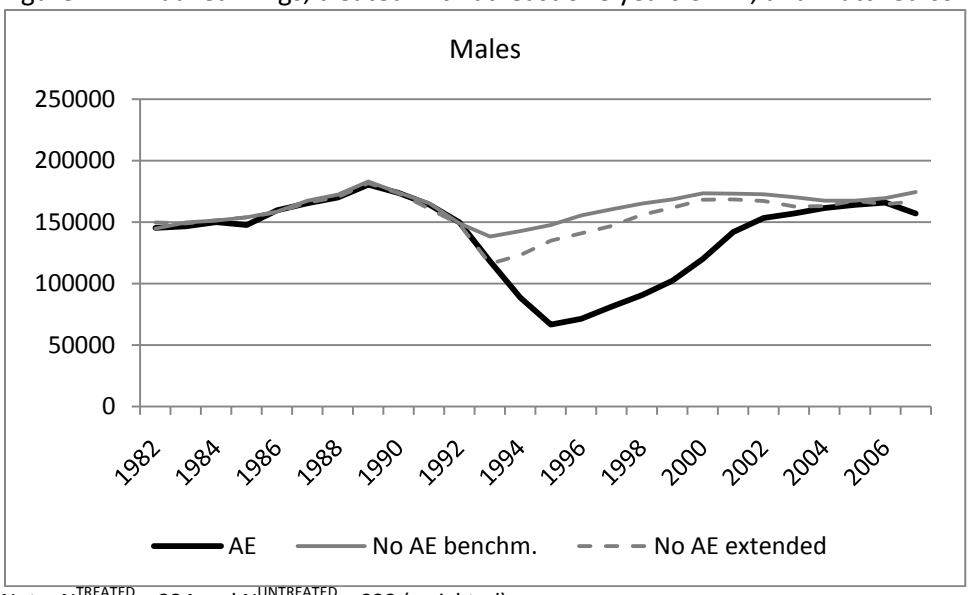


Note: $N^{TREATED}$ = 2,015 and $N^{UNTREATED}$ = 7,886 (weighted).

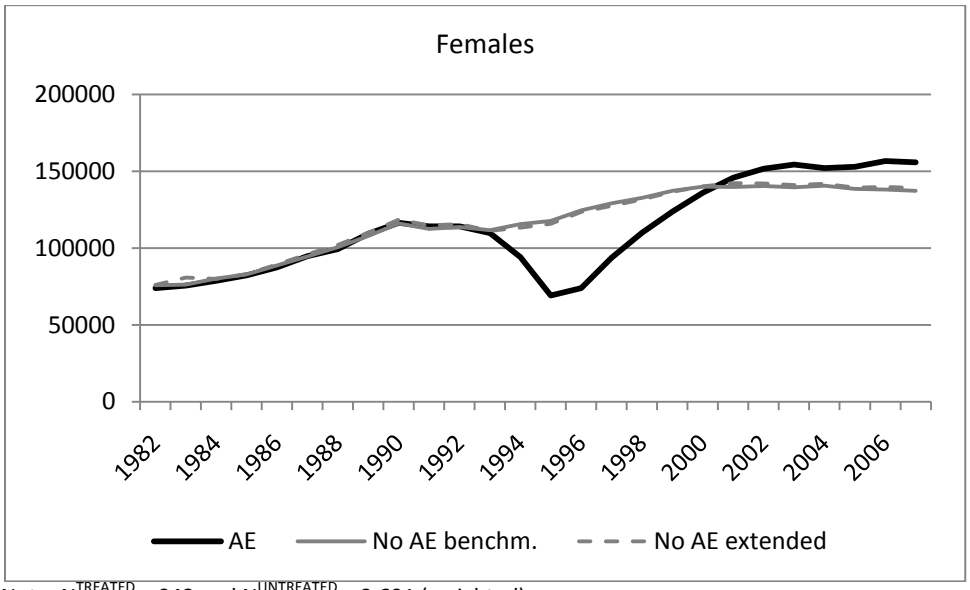


Note: $N^{TREATED}$ = 4,296 and $N^{UNTREATED}$ = 16,241 (weighted).

Figure 4. Annual earnings, treated with at least one years of AE, and matched comparison group.

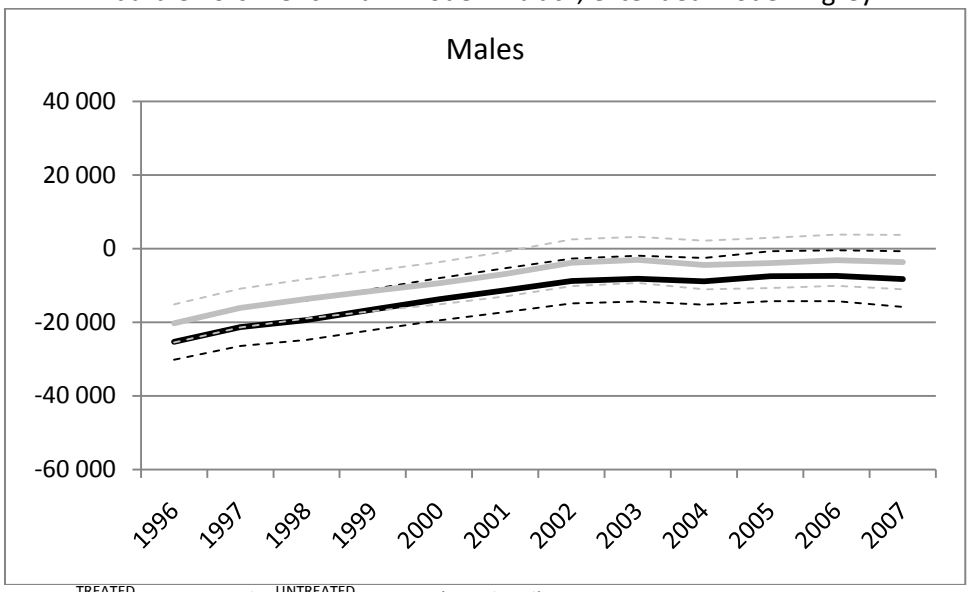


Note: $N^{TREATED}$ = 234 and $N^{UNTREATED}$ = 932 (weighted).

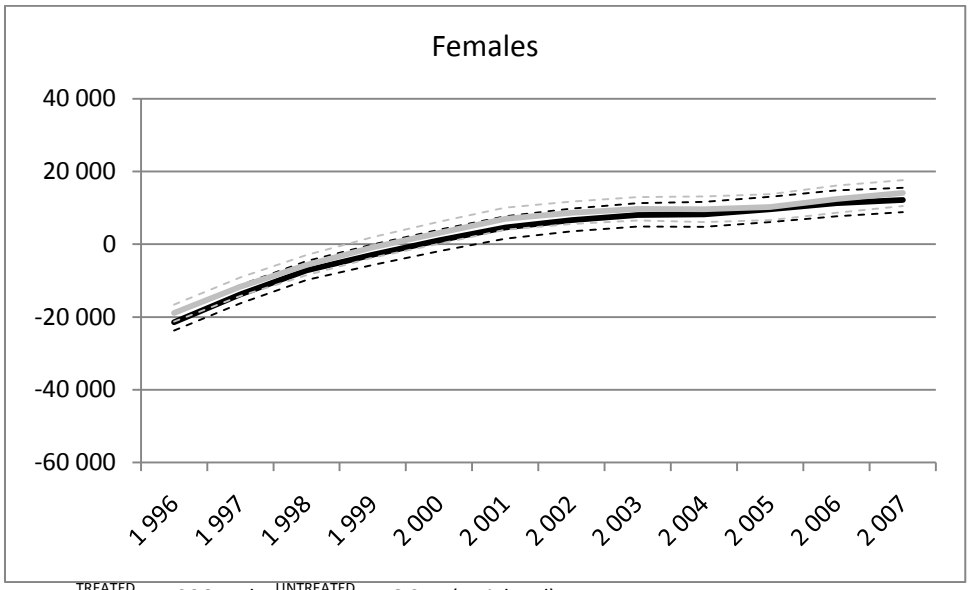


Note: $N^{TREATED}$ = 943 and $N^{UNTREATED}$ = 3,691 (weighted).

Figure 5. Full samples, difference-in-difference propensity score matching estimates, bootstrap standard errors. Benchmark model in black, extended model in grey

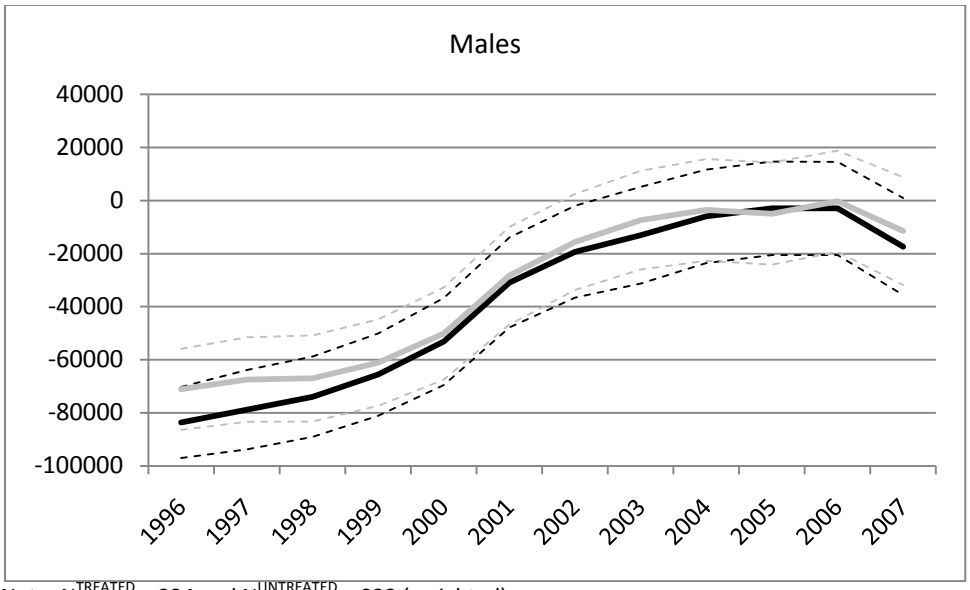


Note: $N^{TREATED}$ = 2,015 and $N^{UNTREATED}$ = 7,886 (weighted).

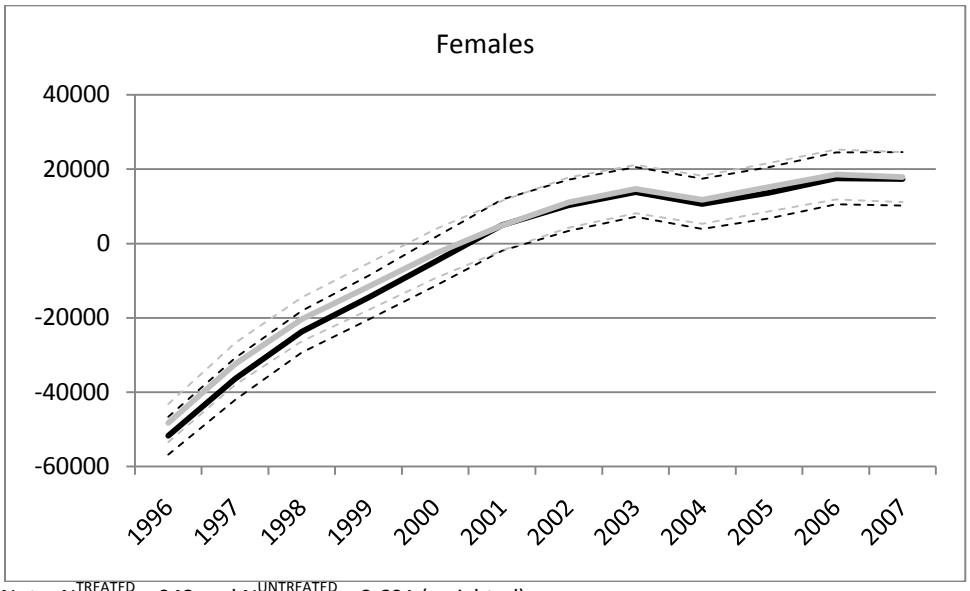


Note: $N^{TREATED}$ = 4,296 and $N^{UNTREATED}$ = 16,241 (weighted).

Figure 6. Treated with at least one year of AE, difference-in-difference propensity score matching estimates, bootstrap standard errors. Benchmark model in black, extended model in grey



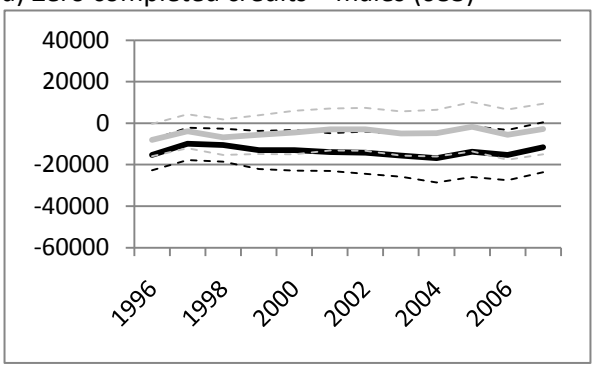
Note: $N^{TREATED}$ = 234 and $N^{UNTREATED}$ = 932 (weighted).



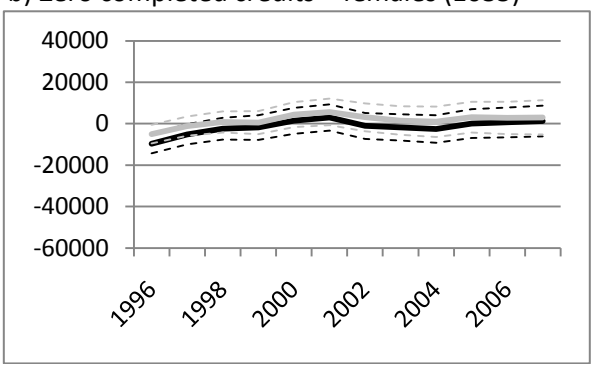
Note: $N^{TREATED}$ = 943 and $N^{UNTREATED}$ = 3,691 (weighted).

Figure 7. Difference-in-difference propensity score matching estimates, bootstrap standard errors. Benchmark model in black, extended model in grey. $N^{TREATED}$ within parenthesis.

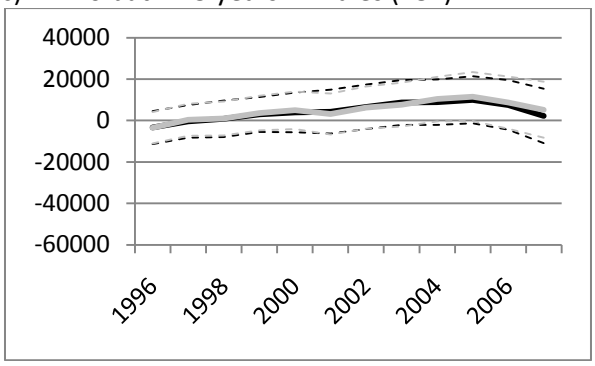
a) Zero completed credits – males (683)



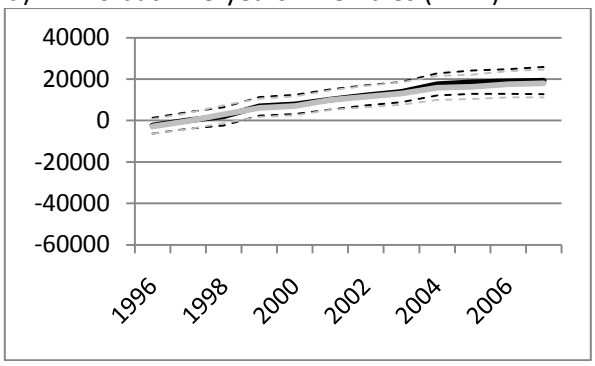
b) Zero completed credits – females (1035)



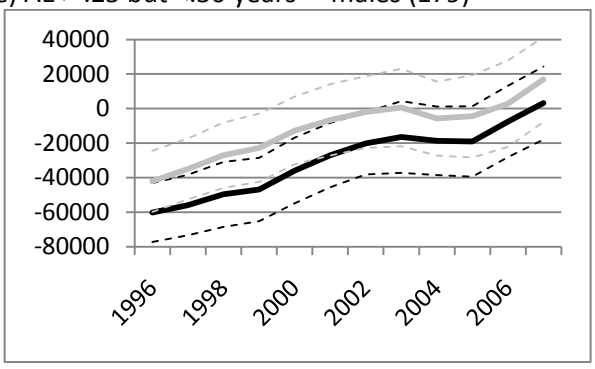
c) AE > 0 but <.25 years – males (781)



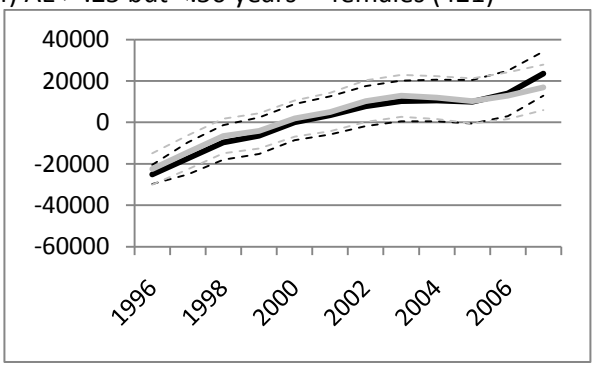
d) AE > 0 but <.25 years – females (1414)



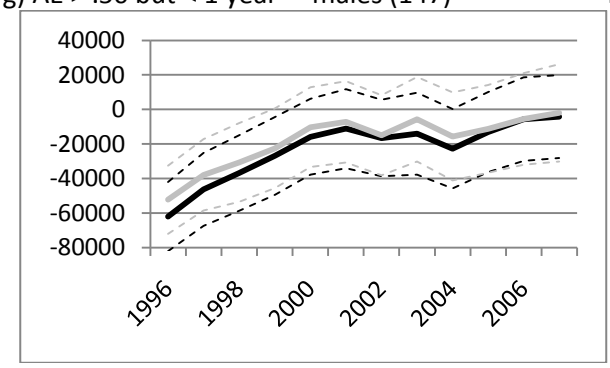
e) AE > .25 but <.50 years – males (179)



f) AE > .25 but <.50 years – females (421)



g) AE > .50 but < 1 year – males (147)



h) AE > .50 but < 1 year – females (518)

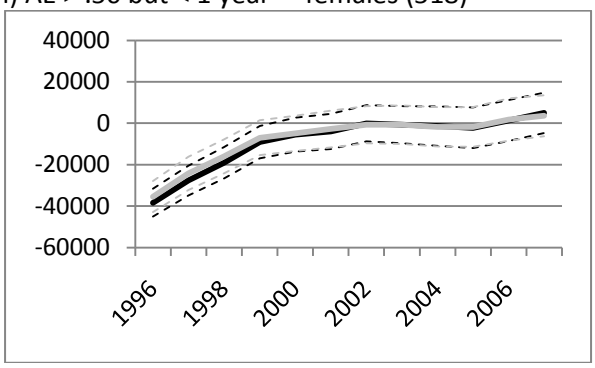
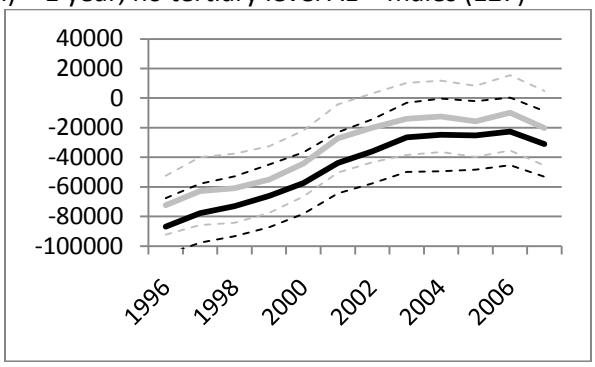
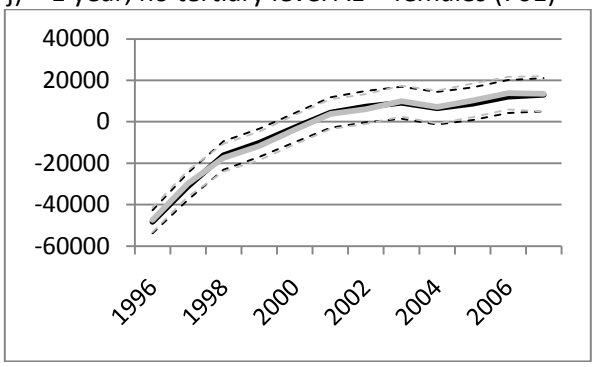


Figure 7 *cont*. Difference-in-difference propensity score matching estimates, bootstrap standard errors. Benchmark model in black, extended model in grey. $N^{TREATED}$ within parenthesis.

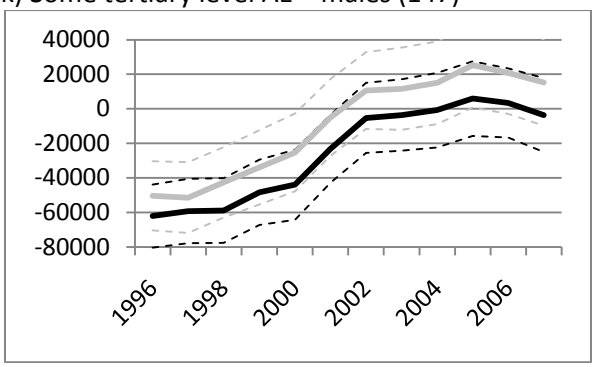
i) > 1 year, no tertiary level AE – males (127)



j) > 1 year, no tertiary level AE – females (701)



k) Some tertiary level AE – males (147)



m) Some tertiary level AE – females (311)

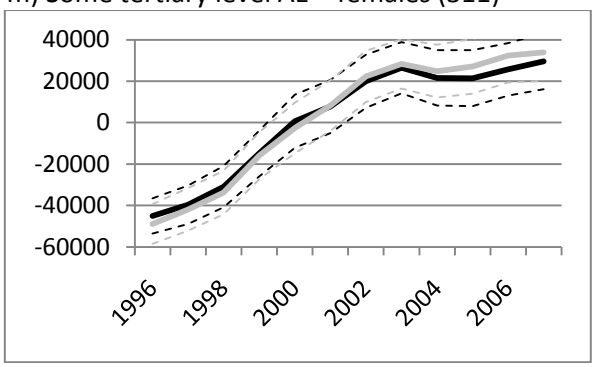
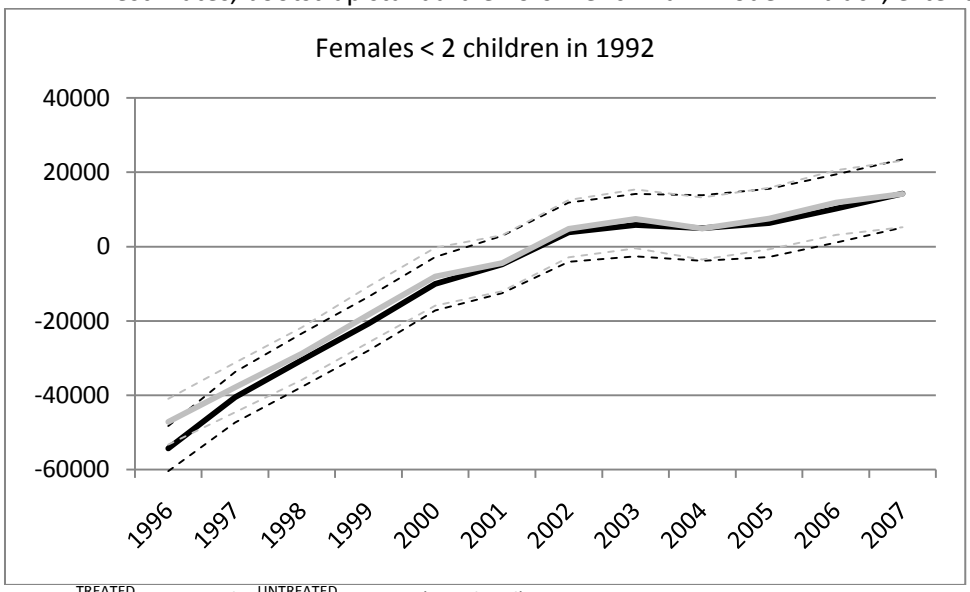
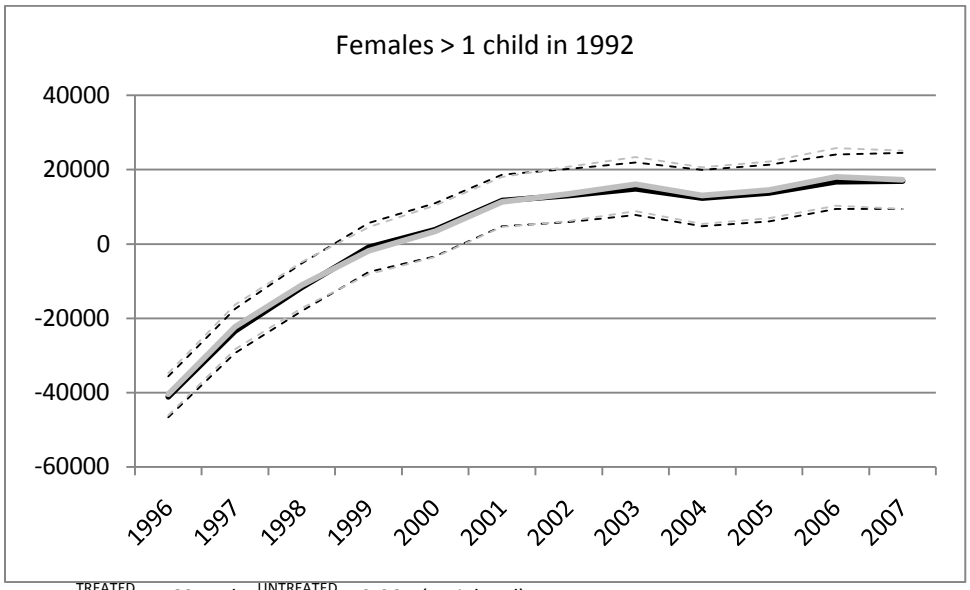


Figure 8. Treated with at least .50 years of AE, difference-in-difference propensity score matching estimates, bootstrap standard errors. Benchmark model in black, extended model in grey



Note: $N^{TREATED}$ = 678 and $N^{UNTREATED}$ = 2,676 (weighted).



Note: $N^{TREATED}$ = 769 and $N^{UNTREATED}$ = 2,984 (weighted).