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Can adult education delay retirement from the labour market?

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WORKING PAPER 2010:2

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ISSN 1651-1166

Can adult education delay retirement from the labour market?*

by

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January 29, 2010

Abstract

In this paper, we examine whether adult education delays retirement and increases labour force participation among the elderly, a mechanism suggested in the OECD strategy for "active ageing" and the "Lisbon strategy" of the EU. Using register data from Sweden, we analyse transcripts from adult education for the period 1979–2004 and annual earnings 1982–2004. We match samples of treated individuals, in adult education 1986–1989, and untreated on the propensity score. The timing of exit from the workforce is assessed by non-parametric estimation of survival rates in the labour force. The results indicate no effects of adult education on the timing of retirement.

Keywords: Adult education, Retirement, Human capital, Labour supply, Pensions
JEL-codes: H52, H55, H75, I28, J14, J26

* The authors are grateful to Anders Björklund, Peter Fredriksson, and Anna Sjögren for helpful comments on an earlier version of the paper. Seminar participants at the European Society for Population Economics are also acknowledged. This work was supported by the Swedish Research Council (through the Ageing and Living Condition, ALC, program and the Swedish Initiative for Research on Microdata in the Social and Medical Sciences, SIMSAM, at Umeå University), and The Bank of Sweden Tercentenary Foundation.

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1 Introduction

In developed economies, increasing shares of the population are entering retirement rather than engaging in productive work. As a result, to ensure future individual and societal welfare, it may be crucial to delay the average age of transition from the labour force into retirement. To improve incentives related to delaying retirement age, governments in many countries have already issued changes in pension systems. In addition to such policies, the OECD proposes measures related to the concept of "active aging", e.g. measures to encourage the retraining of older workers (OECD, 1998, 2001). Similar reasoning is present in the European Union's "Lisbon strategy" for growth and jobs (EU 2007, 2008). The underlying view is that inadequate education, particularly among low-skilled individuals, is an important cause for early retirement because it increases the risk of becoming unemployed and decreases an individual's chances of receiving job offers.

The purpose of this paper is to study whether adult education at the compulsory or upper secondary level (henceforth AE) that takes place at the age of 42 or later affects the timing of the transition from productive work to a life financed by pensions. To our knowledge, this topic has not been previously addressed, presumably because it requires very long time-series data on earnings and training. The analysis is based on Swedish register data of AE transcripts from 1979 to 2004 and labour earnings from 1982 to 2004. Sweden is a suitable country for our study because, since 1969, the Swedish government has mandated that municipalities offer education for adults at the compulsory and upper secondary levels. Participation is free of charge, and full-time students are eligible to apply for financial support amounting to about €780 per month (2004 prices); one-third of this amount is a grant, and the rest is a loan with favourable conditions.[1] In the 1980s, around 100,000 individuals were enrolled annually. Although these numbers include those who were only enrolled in just a single course, it can be compared to the roughly 300,000 adolescents enrolled in regular upper secondary school for youths. In an international comparison, participation in AE is high in Sweden.

[1] For instance, the interest rate is below the market rate and required repayments never exceed 4 per cent of earnings.

According to human capital theory, individuals invest in education at any point during the life cycle if the present value of benefits from education exceeds the costs (Becker 1962). Education is assumed to increase productivity and therefore to increase an individual's wages. On the cost side, time in education invokes foregone earnings besides the direct costs for education. A general implication of restrictive versions of human capital theory is that the returns to additional investments in education decrease with an individual's age. This is because the individual has fewer remaining years in the workforce and because of increased foregone earnings (Becker, 1993). These circumstances may question the rationale of policies promoting AE later in life. However, even if returns decrease with age, they may still be positive for older workers. In addition, extending the earlier versions of the human capital model to include factors like changes in information, relative wages, preferences, health and/or borrowing constraints (Altonji, 1993, Iwahashi, 2004, Killingsworth, 1982, Monks, 1998, Sjögren and Sällström, 2004, Wallace and Ihnen, 1975, Weiss, 1971), it is possible that the returns to education do not fall monotonically with age and that the optimal timing of substantial investments in education will occur at a fairly late stage in the life cycle. Several empirical studies have found that the earnings returns of education among individuals above age 40 are comparable to those of younger age groups (Jacobson et al., 2005a, 2005b, Stenberg and Westerlund, 2008 and Stenberg, 2009).[2] However, increased earnings have theoretically ambiguous implications for the timing of retirement. In a static model of labour supply, higher income may increase the demand for leisure, if leisure is a normal good, and decrease the labour supply. In a dynamic setting, assuming that an individual maximises life-time utility from income streams from work and income as a retired person, the sign of the effect of increased wages on the optimal timing of retirement cannot be determined a priori (Stock and Wise 1990). Wage increases affect income both from work and from pensions, which may lead to earlier or later retirement depending on, for example, the shape of the utility function, subjective time preferences, and the link between income from work and pension entitlement.[3] Whether an earnings increase reflects increased

[2] Mixed results presented in Albrecht *et al*. (2004) and Ekström (2003) are reconciled in Stenberg (2009).
[3] In each period, an individual is assumed to compare the expected utility from immediate retirement with the utility from retiring at a future date. Workers remain in the labour force as long as the option value of work (alternately referred to as the option value of delayed retirement) is positive.

productivity or signaling effects (Spence 1973, Weiss 1995) makes no difference in this respect.

Empirical studies tend to suggest that low-skilled individuals are more likely to retire early. Among the mechanisms that may explain this pattern, technological changes have been claimed to work to the relative disadvantage of older workers (Aubert et al., 2006, Borghans and Weel, 2002).[4] AE is potential insurance against such a scenario because broadened and updated skills may improve the employability of individuals and, from a more long-term perspective, make him/her less sensitive to structural changes in labour demand. Indeed, Peracchi and Welch (1994) suggest that high-skilled older workers are less likely to leave full-time employment, and Blau (1994) finds high-skilled more closely attached to the labour market and less sensitive to labour market shocks. These findings may partly reflect the apparent complementarity between on-the-job-training and formal education observed among older workers by Schils and Fouarge (2007).[5] Another possible explanation is that education is correlated with health. Kalwij and Vermeulen (2005) argue that health is an important factor in explaining labour force participation among the elderly, while Lleras-Muney (2005) shows evidence of education's causal effect on decreasing mortality. Thus, empirical studies point to some potential and intuitively reasonable mechanisms lending support to the policy recommendations of the EU and the OECD, that is, investment in adult education might postpone the timing of retirement. The aim of our analysis is to study whether this view finds support in the data.

The contribution of the present study is that it directly addresses the issue of whether AE prolongs working careers. Unlike earlier studies of the effect of AE on earnings, this paper analyses the potential effects on labour supply up to and beyond the official retirement age. Irrespective of earnings effects, extending the working life for the elderly may mitigate the negative economic and social impacts of ageing populations. Moreover, given labour market imperfections, increased labour force participation has beneficial effects on the economy at large because it increases competition for jobs and moderates labour supply shortages during economic booms. However, as pointed out before, the theoretical effects of AE on the transition from the

[4] However, Bartel and Sicherman (1993) find that workers in industries with high rates of technological change retire later to recoup the returns on late investments in on-the-job-training; nevertheless, a technological shock would (ceteris paribus) induce workers to retire sooner because of increased costs for re-training.

work force into retirement are indeterminate. Our main finding in this study is that AE has no effect on the timing of retirement from the labour market. This result is stable across a number of robustness checks.

The paper consists of five sections. Section 2 presents the institutional setting, the data and the design of the study. The empirical method is specified in Section 3; the results are presented and discussed in Section 4, before some concluding remarks in Section 5.

2 Institutional setting, data and definitions

In Sweden, eligibility for higher education at the university level requires 12 years of schooling. It includes compulsory schooling, which has lasted nine years since the beginning of the 1960s, and three years of upper secondary school. Until the beginning of the 1960s, the schooling systems varied somewhat across municipalities, but in essence, pupils attended seven or eight years of compulsory school. Pupils with better marks were selected to attend junior secondary school (i.e. realskolan) beginning in their seventh year. This stage lasted at least three years and was a prerequisite for upper secondary school. Those not selected had a second chance to enrol in junior secondary school at a later point in time. In our data, each individual's highest level of education attained is described by category, and "9 years of schooling" includes those who attended junior secondary school. As for upper secondary school, those with one year of upper secondary school are almost exclusively in nursing. Among those with two years of upper secondary school, professional trajectories are much more diversified, and we distinguish three broad groups, namely, 1) commerce, including services and secretarial and office training; 2) manufacturing, which includes craftsmanship and technology; and 3) transport, including vehicle engineering and media communication.

Adult schooling at the compulsory and upper secondary levels (AE) is offered by all Swedish municipalities and is free of charge. It is primarily of interest to those who spent time in school less than or equal to a completed two-year program at the upper secondary level. Participants in AE are eligible for study allowances sufficient to cover modest living expenses, and employees are entitled by law to be on unpaid leave for

[5] On-the-job-training typically involves high-skilled individuals (Brunello 2001, Arulampalam *et al.* 2004).

the purpose of attending any kind of training and be reinstated with the same working conditions and pay.

Our data originate from various registers of the Swedish population administered by Statistics Sweden. AE course transcripts cover the period 1979-2004, and labour earnings 1982-2004. The treatment group is defined as first-time AE enrolees between 1986 and 1989. During this period, they must have been registered in courses amounting to at least 125 course credits, the equivalent of a half-semester of full-time study. This definition allows us to consider earnings trajectories prior to 1986 and still leaves a 15-year follow-up period. The comparison group consists of individuals who were not registered in AE at all or who were registered in courses that amounted to less than 125 course credits. The sample of the study is further restricted to individuals who were born from 1931 to 1944, i.e. aged 42 to 55 in 1986 and 60 to 73 years old in 2004. Given that AE is most relevant for low-skilled, our sample of individuals is conditioned to have had eleven years of schooling or less in 1985, thus excluding about one-third of the population and making all sample members ineligible for tertiary education. To further "clean" our analysis, we exclude individuals enrolled in AE 1979-1985 or individuals with zero earnings in 1984 and 1985 (i.e., those not active on the labour market). The descriptive statistics of treated and comparison group members are presented in Table 1. The large number of individuals defined as untreated is an advantage because it should ease the process of finding appropriate matches, i.e. individuals comparable to those in the treatment group.

Table 1 Sample means of treated (AE enrolees) and untreated, males and females, in 1985, unless otherwise stated.

	Males			**Females**		
Born 1931-1944	Treated	Untreated	*p*-value[a)]	Treated	Untreated	*p*-value[a)]
N	1348	354529		4982	334647	
Age in 1986	46.33	48.15	*.000*	46.92	48.42	*.000*
Child(ren) at home	1.272	1.080	*.000*	1.244	.933	*.000*
Less than 9 years of schooling	.373	.588	*.000*	.280	.470	*.000*
9 years of schooling [b)]	.142	.083	*.000*	.115	.118	*.514*
1 year of upper secondary school	.022	.009	*.000*	.302	.127	*.000*
2 years of upper secondary school	.464	.320	*.000*	.302	.285	*.005*
- Commerce	.065	.047	*.002*	.123	.121	*.593*
- Manufacturing	.280	.188	*.000*	.027	.027	*.929*
- Transport	.035	.030	*.250*	.013	.025	*.000*
Earnings 1982 (SEK in 1000s)	75.7	79.5	*.594*	45.4	49.2	*.016*
Earnings 1985 (SEK in 1000s)	93.9	101.3	*.017*	61.4	63.5	*.000*
Earnings change 1982 – 1985	21.8	18.3	*.648*	16.0	14.3	*.275*
Foreign-born	.162	.110	*.000*	.107	.113	*.000*
Unemployment benefits > 0	.116	.065	*.000*	.098	.063	*.000*
Stockholm county [c)]	.177	.150	*.005*	.174	.159	*.004*
Inland of Norrland [c)]	.060	.059	*.902*	.064	.053	*.001*
Regional employment (M/F)	.835	.834	*.007*	.767	.765	*.000*
Born in 1944						
N	215	31362		647	26992	
Age in 1986	42	42		42	42	
Child(ren) at home	1.519	1.449	*.370*	1.772	1.529	*.000*
Less than 9 years of schooling	.265	.468	*.000*	.219	.347	*.000*
9 years of schooling [b)]	.181	.128	*.002*	.122	.151	*.044*
1 year of upper secondary school	.019	.013	*.426*	.309	.138	*.000*
2 years of upper secondary school	.535	.391	*.000*	.349	.365	*.412*
- Commerce	.060	.049	*.450*	.147	.171	*.101*
- Manufacturing	.340	.246	*.002*	..034	.034	*.946*
- Transport	.037	.034	*.770*	.015	.023	*.181*
Earnings (SEK in 1000s)	90.7	103.5	*.001*	57.6	64.0	*.000*
Earnings diff 1982 – 1985	17.9	26.4	*.001*	18.9	18.5	*.673*
Foreign-born	.163	.100	*.003*	.097	.099	*.882*
Unemployment benefits > 0	.126	.070	*.002*	.114	.069	*.000*
Stockholm county [c)]	.195	.158	*.129*	.170	.169	*.932*
Inland of Norrland [c)]	.037	.055	*.256*	.060	.048	*.169*
Regional employment (M/F)	.837	.834	*.029*	.767	.766	*.403*

[a)] *t*-test for equality of means between treated and untreated groups

[b)] Including individuals who attended junior secondary school (*realskolan*), which encompassed up to 10 years of schooling.

[c)] 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 per cent of the population, and the overall employment level is higher than in any other region of Sweden.

The descriptives in Table 1 reveal that education late in life is predominantly a female phenomenon. We do not explore the potential mechanisms behind this pattern, but greater family commitment is likely to be correlated with a greater need and/or desire

to go back to school after age 40. Note also that those in AE are on average slightly younger by 1.8 years (males) and by 1.5 years (females). We also observe that the treated have a higher level of education. This is an expected pattern because earlier studies have shown that initial skill level is positively correlated with the willingness to participate in adult education (Oosterbeek 1998, OECD 2003, 2006). The opportunity costs of participating in AE should also influence the decision to enrol. Consequently, treated individuals had lower average earnings in both 1982 and 1985 and were also more likely to be receiving unemployment benefits during 1985, although the fractions doing so are only around 10 per cent.[6] Regional labour market conditions potentially influence the effects of AE. Helpfully, the means of the regional employment rates are almost identical between treated and comparison groups. We do not take migration after AE into account when assessing the potential effects of AE since mobility may be an endogenous outcome of treatment.[7] The lower half of Table 1 contains the corresponding descriptive characteristics of individuals born in 1944, i.e. controlling for age. The differences in averages for the treated and comparison groups display patterns similar to those in the upper half of the table.

Table 2 presents descriptive data on the actual AE undertaken by the treated individuals. We observe the number of credits and the level of each course (i.e., compulsory or upper secondary); we also categorise each course based on its name. We do not observe whether the courses are completed, and there is no information on diplomas awarded. Course registrations on average amount to 390 credits for males and 458 credits for females, where 500 course credits correspond to a school year (Figure A.1 in the Appendix presents a histogram of the number of registered course credits among the treated).[8] We note that choices made by females and males are fairly different. For instance, the proportion of individuals who attended courses at the compulsory level was 28 per cent among women and 50 per cent among men. Almost 60 per cent of the males attended at least one course labelled vocational. For females, the corresponding proportion is 36 per cent, while if one includes health-related courses in the category of "vocational", the share is 83 per cent. Moreover, in addition

[6] The policy until the mid-1990s was that in order not to generate perverse incentives, formal education was not offered as an active labour market program.
[7] Migration to another regional labour market is a very uncommon event in these age cohorts (Eliasson and Westerlund 2009).

to AE, on-the-job-training may be a factor for both treated and untreated individuals. We do not have information on this issue, but such training typically has relatively short duration, with low-skilled individuals underrepresented among the participants (Brunello 2001, Arulampala et al. 2004). It is possible that some individuals merely "consume" AE and attend a course in carpentry or a foreign language without the purpose of enhancing their labour market productivity. This is difficult to address because we do not know why individuals enrol; however, a rudimentary check is made in the empirical section insofar as we increase the lower bound of registered course credits in our definition of the treatment group.

Table 2 Content of adult education (treated individuals only).

	Males	Females
N	1348	4982
Total course credits	390.6	458.3
Standard deviation	(332.7)	(458.0)
Compulsory level		
Proportion registered	.503	.280
Registered compulsory credits	175.7	94.3
Upper secondary level		
Proportion registered	.774	.871
Registered upper secondary credits	208.5	350.4
Frequency of upper secondary type of course registration		
English	.455	.278
Swedish	.372	.206
Mathematics	.389	.178
Social sciences	.398	.307
Natural sciences	.217	.120
Human sciences (e.g. foreign languages)	.079	.057
Computer sciences	.056	.054
Health-related subjects (e.g. nursing)	.044	.474
Vocational course	.584	.360

The outcome of interest in this study is the timing of the transition from productive work to dependence on transfers related to retirement pensions of different forms. The economics literature does not seem to have developed a consensus view on how to measure this transition (Denton and Spencer 2008). Our approach is to define a ratio, r_{it}, for an individual i in year t between income from pensions p_{it} and annual labour earnings e_{it} (in 100s of SEK). The ratio can be written explicitly as

[8] The average among untreated individuals is 8.5 credits. In the empirical section, the mean among matched

$$r_{it} = \frac{p_{it}}{(e_{it} + 1)}$$

where (+1) is included in the denominator to avoid division by zero. For comparability with future studies, the empirical section contains several alternative threshold values of r_{it}, but our benchmark is that individuals are classified as active in the labour market as long as $r_{it} \leq 1$, meaning that the duration outcome is defined as the number of years, beginning in 1990, during which an individual has enjoyed labour earnings that exceed (or are equal to) the sum of pension transfers.[9] Duration may be censored due to events such as death, migration abroad, or enrolment in AE.[10] The latter is only possible for individuals in the untreated group. For our purposes, it is important that an observation satisfying our definition of retirement ($r_{it} > 1$) is not followed by re-entry into the labour market ($r_{it} \leq 1$) in subsequent years. However, such events are rare and concern less than 1 per cent of the treated and untreated samples. Another issue is that of part-time retirees, who in our benchmark definition are assumed to remain in the labour force as long as their earnings exceed their pension transfers. This matter is addressed in the empirical section by allowing the threshold value of r_{it} to vary.

Figures 1 and 2 focus on treated and untreated individuals and separately describe their duration in the labour force using Kaplan-Meier survival functions, i.e. the proportion of individuals surviving ($r_{it} \leq 1$) at least up to year t. For both males and females, survival rates are significantly higher for the treated. This descriptive data is not expected to reflect a causal effect of AE. In particular, the average age of the treated is slightly lower, something that may drive the differences in survival rates. An alternative view of the same data is given in Figures A.2 and A.3 in the Appendix, where the differences in survival functions between the treated and untreated are displayed. This is the format of illustration that we will use in the empirical section.

To investigate how age may influence survival rates, Table 3 presents a snapshot of cohort-specific proportions of individuals still active on the labour market ($r_{it} < 1$) in 1997 among treated and untreated (proportions in italics).[11] As expected, the

comparisons never exceeds 20 credits (i.e. about ten days)

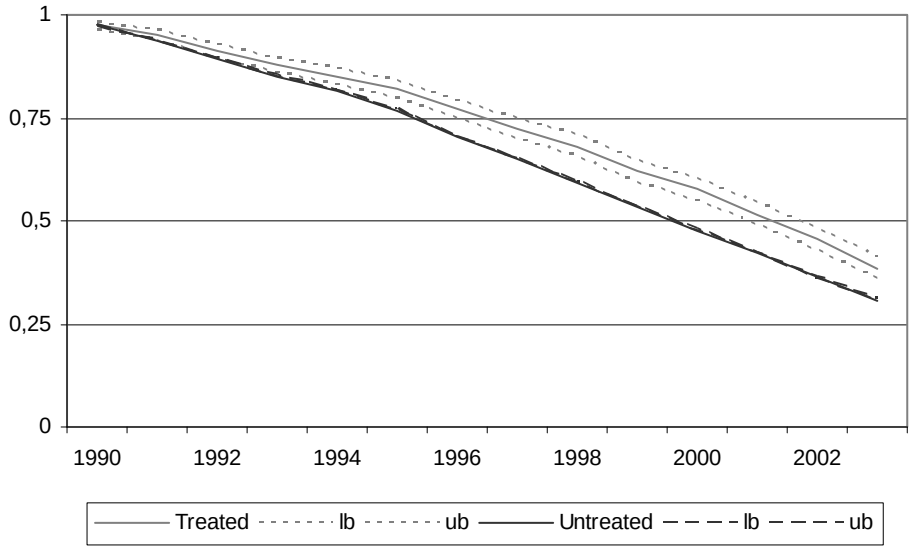
[9] p_{it} is the sum of part-time pensions, retirement (i.e. old-age) pensions, national supplementary pensions (ATP), and early-retirement pensions, including sickness pensions and various occupational pensions.

[10] Until 2004, the last year of observation, the proportion of censored individuals was below 4.5 per cent.

[11] The macroeconomic background made the probability of retirement higher during the economic downturn at the beginning of the 1990s, but note that the regime under which individuals retire is the same for the treated and the untreated at different points in time.

proportions defined as "active" tend to decrease with age, particularly as the age of 65 approaches. In most occupations, the officially stated age of retirement is 65, but early retirement is generally allowed beginning at the age of 60.[12] More than 40 per cent of our sample is still classified as active on the labour market during the year in which they turn 64; thereafter, the percentages decrease to about 15 per cent at the age of 65 and below 2 per cent at the age of 66. Among treated females, the proportion of individuals active on the labour market is higher than among untreated females. For males, the number of treated becomes somewhat low when separated into different cohorts, but a relatively stable pattern emerges as the treated demonstrate a lower proportion of active individuals than untreated (except for 62- and 64-year-olds). It indicates that the higher survival rate for treated males displayed in Figure 1 was driven at least partially by their lower average age.

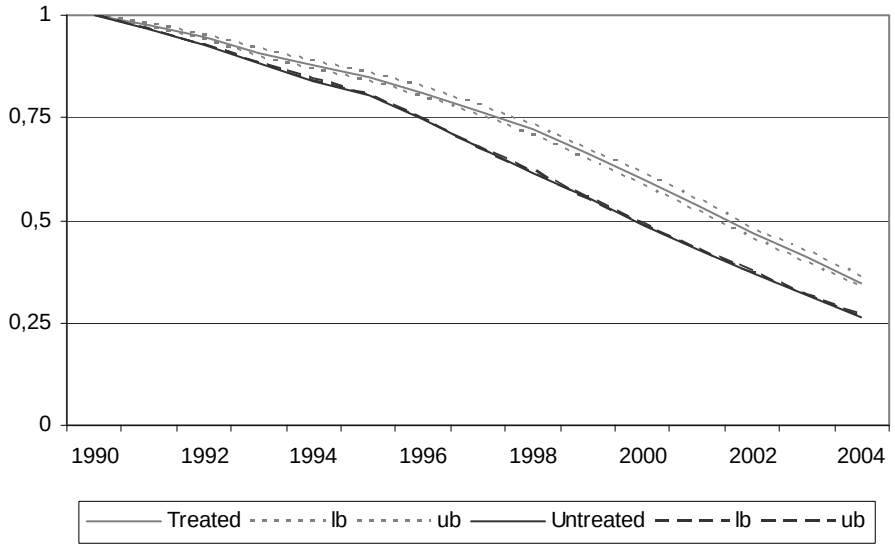
Figure 1 Kaplan-Meier estimates of survival rates in the labour force 1990–2004. Unmatched samples of males.



Note: lb = lower bound of 95 per cent confidence interval; ub = upper bound.

[12] Blöndahl and Scarpetta (1999) estimate the average age of transition to inactivity in 1995. For Sweden, this was found to be 63 for males and 62 for females. The corresponding figures for the US were 64 and 62 years of age, and for Germany, they were 61 for males and 58 for females. Labour force participation and employment rates among 55-64 year olds are higher in Sweden than the OECD average. See also Eklöf and Hallberg (2004, 2006) and Karlström *et al.* (2004).

Figure 2 Kaplan-Meier estimates of survival rates in the labour force 1990–2004. Unmatched samples of females.



Note: lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Table 3 Proportions still active on the labour market in 1997.

		Males			**Females**		
			Proportions with $r_{it} < 1$			Proportions with $r_{it} < 1$	
Born	Age in 1997	N_T / N_U	Treated	Untreated	N_T / N_U	Treated	Untreated
1940	57	116 / 25151	*.767*	*.798*	425 / 22536	*.798*	*.765*
1939	58	112 / 25796	*.741*	*.765*	387 / 23508	*.796*	*.729*
1938	59	92 / 24685	*.696*	*.737*	356 / 23436	*.778*	*.706*
1937	60	66 / 24160	*.591*	*.674*	335 / 23184	*.719*	*.656*
1936	61	66 / 23697	*.545*	*.585*	302 / 23195	*.702*	*.603*
1935	62	70 / 23395	*.629*	*.536*	220 / 22780	*.659*	*.554*
1934	63	53 / 22983	*.321*	*.462*	241 / 23096	*.668*	*.506*
1933	64	43 / 22710	*.442*	*.407*	209 / 23073	*.545*	*.454*
1932	65	36 / 23491	*.111*	*.136*	180 / 24502	*.150*	*.120*
1931	66	26 / 23700	*.000*	*.013*	173 / 24685	*.017*	*.011*

Note: N_T / N_U yields the number of individuals in the treated and the untreated group, respectively.

3 Empirical method

We aim to estimate the causal effect of AE on the timing of exit from the labour force, where AE and exit ($r_{it} > 1$) are defined as in Section 2. For this purpose, we use the Rubin causal model (Rubin, 1974, Holland, 1986) and the matching estimators adapted for duration outcomes in de Luna and Johansson (2007) and Fredriksson and Johansson (2008).

Our empirical strategy assumes a selection mechanism (into AE) based on observables. The presentation below includes a detailed discussion of why this assumption is reasonable in our context.

For a given individual, let T(1) and T(0) denote the timing of his/her exit from the labour force if treated (AE) or not treated, respectively. One of these two potential outcomes will always be missing because we will observe the outcome either with or without AE. Treated individuals are matched with a comparison group, which we obtain by nearest-neighbour matching on the propensity score and by selecting four untreated individuals with propensity scores closest to those of each given participant in AE.[13] The propensity scores are estimated probabilities of being among the treated, using probit models (the choice of covariates is presented below). We define the causal effect of interest as the difference in the Kaplan-Meier estimates of the survival functions of the AE group and the matched comparison group. This provides an unbiased estimate of the causal effect under the following identifying assumptions (see de Luna and Johansson 2007 for a formal account):

(A) An individual's choice of enrolling in AE or not does not affect the outcomes of other individuals.

(B) Conditional on the covariates, the probability of treatment is strictly positive and strictly smaller than one.

(C) Conditional on the covariates, the mechanism of enrolment into AE is independent of the potential outcomes.

(D) Conditional on the covariates, the different censoring mechanisms are independent of the outcomes.[14]

In Table A.1 in the Appendix, the probit estimates of the propensity score are presented for females and males, while the results of the balancing tests are given in Table A.2 in the Appendix. The balancing tests involve all covariates that theoretically may explain AE enrolment, and they show that the AE and matched groups are comparable in the sense that, for any of the covariates, the equality of their respective

[13] We match on the propensity score, i.e. the probability of entering AE conditional on the covariates; see Rosenbaum and Rubin (1983) for a theoretical justification. Matching all covariates may be difficult due to "the curse of dimensionality." That is, finding close matches on many dimensions is not possible.

[14] As mentioned in Section 2, censoring is associated with death, migration abroad and, for the comparison group, enrolment in AE.

mean values cannot be rejected. The balancing tests concern 14 year-of-birth-dummies also interacted with earnings in 1985 as well as earnings squared in 1985. Other variables are the earnings difference between 1985 and 1982, this same earnings difference squared, the level of regional employment, a dummy indicating if unemployment benefits were above zero in 1985 and a variable giving the amount of unemployment benefits actually received. Dummies are included to indicate educational levels, whether an individual is foreign-born, Nordic/non-Nordic country of origin, regional residence (Stockholm or inland of Norrland) and the number of children living at home (five or more is the upper limit). The explanatory variables in the probit models are a subset of these variables because over-parameterisation has been shown to increase the mean squared error and exacerbate the support problem (Caliendo and Kopeinig, 2005, Waernbaum 2008). Variables were thus excluded if associated with p-values of .2 or above. This rule-of-thumb was circumvented if the inclusion of a covariate was necessary to balance the AE and matched comparison groups.

To identify causal effects of AE, it is crucial that assumption (C) above is not violated by unobservable factors that affect both AE participation and the timing of the exit. We see three main factors that can confound the effect of AE participation on exit from the labour force. First, individuals with a higher level of ability and/or motivation may systematically enrol in AE and, at some future point in time, decide to retire later as a result of the same characteristics that made them enrol. Conversely, if AE is considered a leisure activity, then participants might value leisure (and/or household work) more highly and thus tend to retire earlier. Third, geographical variations in enrolment may stem from regional heterogeneity in labour market conditions and/or social interaction effects, leading to a spurious correlation between the decision to enrol in AE and the timing of an exit from the labour force. To control for unobserved ability and/or motivation, we use indicators of individual's unemployment, as well as 1985 earnings and change in earnings during the period 1982–1985. An advantage with earnings is that it is a continuous variable with a fair amount of variation, reflecting both productivity and willingness to work (as it is composed of the level of wages and number of hours worked). Note also that zero earners were excluded in the construction of our samples (see Section 2). Via interaction variables, the coefficients associated with earnings and earnings squared in 1985 are allowed to vary between

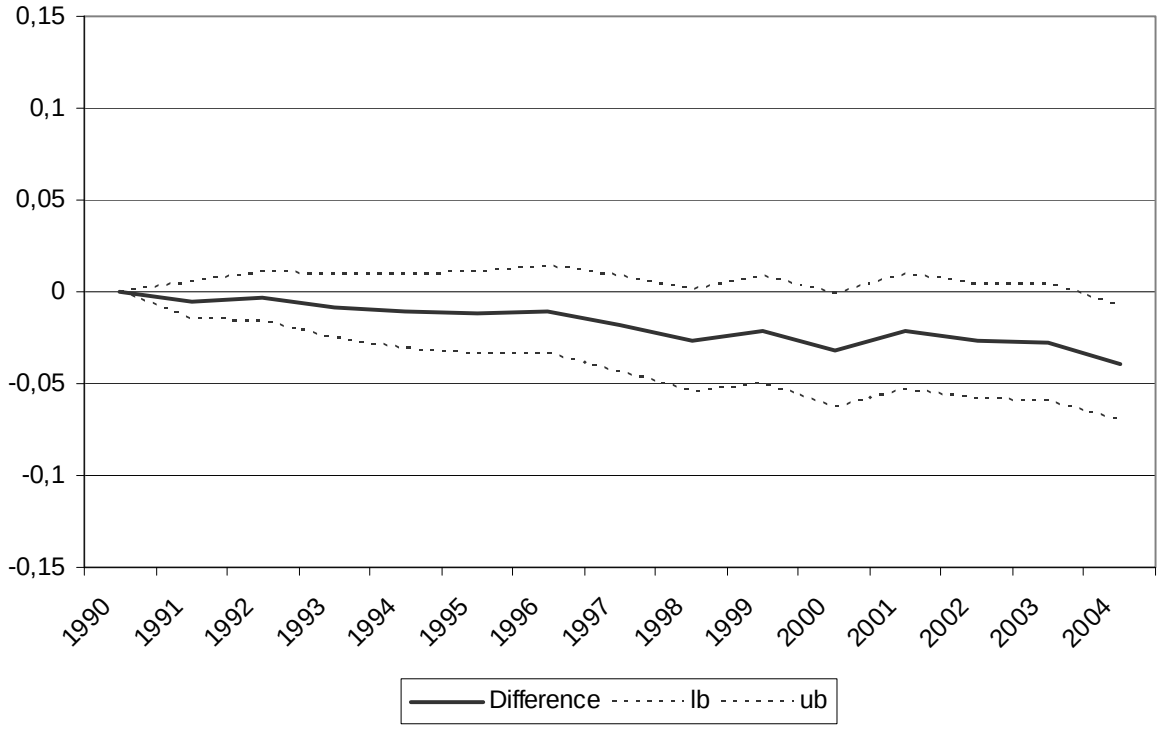
cohorts born in different years. This approach appears reasonable, especially since several influential studies have found earnings before treatment to be a powerful control for self-selection into training programs (Heckman et al. 1999, Glazerman 2003, Smith and Todd 2005). With respect to leisure preferences, we presume that those interested in AE as a leisure activity would only sporadically register for courses. This is one reason why we set the minimum threshold value at half a semester of full-time study to define AE and include robustness checks with even higher thresholds. Also, as earnings levels should contain information on individual preferences for leisure, as they partly reflect the number of hours worked, as well as an individual's costs of leisure, as they partly reflect wage levels. Finally, to specifically control for geographical variations in labour market conditions or social interaction effects, our controls include regional employment. In addition, in a robustness check, we focus on residents in Stockholm to restrict the sample to one specific labour market.

4 Results

Using samples of treated and matched comparison groups, Figures 3 and 4 display the differences in Kaplan-Meier estimates of survival in the labour force for males and females respectively.[15] The differences in survival rates are small over the entire follow-up period, and the point-wise 95 per cent confidence intervals of the differences do not diverge from zero. Table A.3 in the Appendix lists the underlying levels of survival rates and confidence intervals.

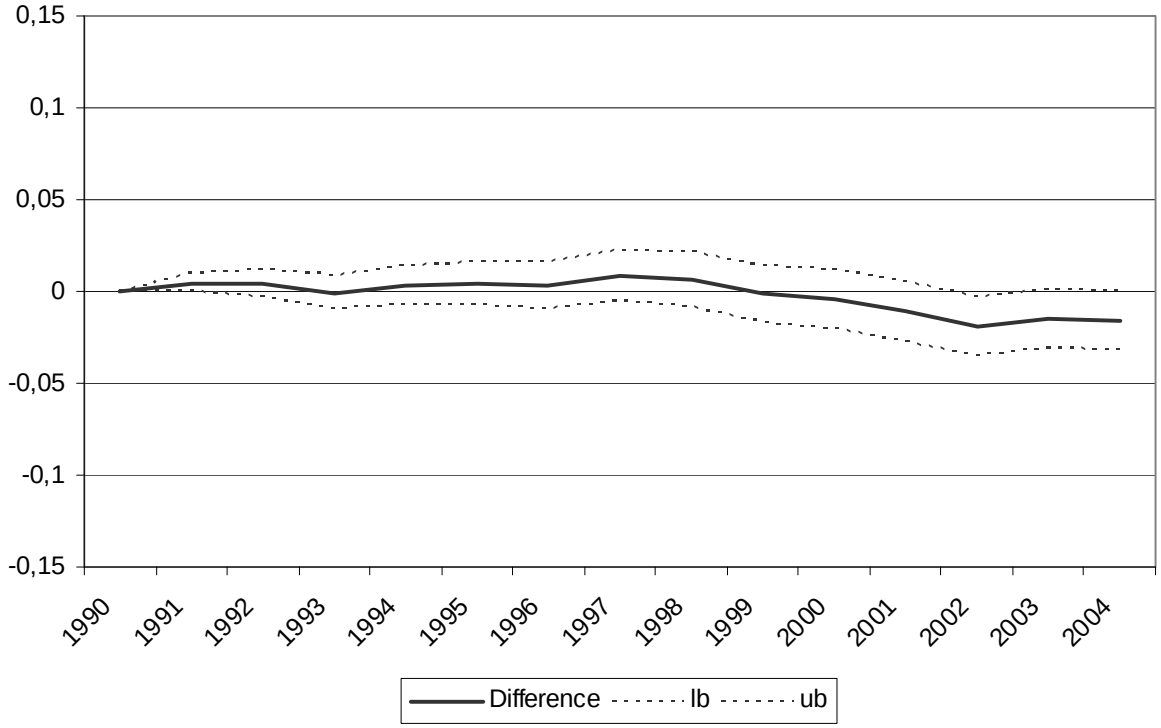
[15] An estimator for the variance of the estimator is given in de Luna and Johansson (2007, Sec. 4.2); using this tool, confidence intervals can be computed with a normal approximation.

Figure 3 Males, treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates in the labour force 1990–2004.



Note: lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure 4 Females, treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates in the labour force 1990–2004.



Note: lb = lower bound of 95 per cent confidence interval; ub = upper bound.

We can note that the substantial differences in survival rates indicated by the raw sample means presented earlier in Section 2 (see Figures 1 and 2 as well as Figures A.2 and A.3) do not apply when we control for heterogeneity in observables . To check the stability of these results, we employ three strategies; 1) alternative sub-samples, 2) alternative definitions of the treatment group and 3) alternative definitions of the outcome variable. In the Appendix, Figures A.4 through A.13 provide an account of some of these estimates.[16]

First, we repeat the analysis, separately for males and females, conditioning on the following characteristics as observed in 1985: a) no upper secondary schooling (Figures A.4 and A.5); b) some upper secondary schooling; c) year of birth 1935–1939; i.e. the youngest cohort turns 65 in 2004 (Figures A.6 and A.7); d) year of birth 1940–1944; and e) place of residence is Stockholm (Figures A.8 and A.9). As with the full sample results, the differences in the point estimates of the survival rates are small

[16] The full set of figures and confidence interval tables is available upon request from the authors.

and there are no significant differences in the timing of the transition into retirement between the samples of treated and matched comparisons.[17] A different sampling aspect is that some individuals in the comparison group were registered in AE courses, but registrations amount to, on average, less than ten days of AE (less than 20 credits). Omitting these observations does not change our results.

Second, the definition of treatment is altered. To begin with, we require a minimum of 250 registered credits (see Figures A.10 and A.11 in Appendix). However, the estimation results are not substantially different from what has been presented above. The same applies if the minimum number of course credit registrations is set to 500 (i.e. equal to a schooling year) as well as when we conditioned on the content of the AE courses, notably with respect to vocational courses for males and health-related studies for females.

Third, we turn to alternative definitions of retirement. We first address whether the results are sensitive to our treatment of part-time retirees by using different threshold values of $r_{it} > 1.2$, .8, and 0, with the last defining the timing of retirement as the first year when pension income exceeds zero (Figures A.12 and A.13). Regardless of the definition of the outcome, the estimations have the same implications as before; that is, there are no significant differences in the timing of retirement between matched groups of treated and comparison groups. Next, because we may underestimate the measure of labour market activity by using earnings, we add unemployment benefits to the denominator of r_{it}. However, this has little effect on the estimates. Nevertheless, one might argue that unemployment benefit transfers are received in a transition period from employment to retirement (Hallberg 2003). In such a case, our ratio r_{it} would instead exaggerate attachment to the labour force. If, for example, the untreated use this transition path more often, our estimated effects of AE on survival rates may be biased downwards. To check whether this is a problem, we compared the incidence and levels of unemployment benefits for 59- to 65-year-olds in each year in the period 1996-2004 for treated and untreated individuals. In the matched groups of males, there were no significant differences in incidence or levels. For females, equality in incidence could not be rejected for any year, while the benefit levels were significantly

[17] The point estimates sometimes tend to be below zero. Although estimates are very small and not significantly different from zero, a negative effect on the survival rate is of course theoretically feasible, as described in the introduction.

higher for untreated individuals in three of the nine years, namely, 1996, 2001 and 2002. We conclude that this particular aspect does not seem to be of crucial importance.

Although it may seem a reasonable hypothesis, and despite running a number of robustness checks, we do not find any indications that additional investments in education at later stages of working life influence the retirement age. One may speculate regarding possible explanations for our findings. It may be the case that we have captured many individuals who merely "consumed" education. In other words, the motives behind these individuals' enrolment decisions may not have been related to labour market outcomes. This argument, however, requires that the consumption of education entails large amounts of AE because the results are robust to definitions of treatment that correspond to a year of full-time study measured in terms of course registrations. The "consumption of AE" argument is also contradicted by the positive earnings effects of AE reported in Stenberg and Westerlund (2008) and Stenberg (2009) as well as those reported using US data in Jacobson (2005b). A different interpretation would be that a positive earnings effect generates an income effect that increases the demand for leisure, potentially counterbalancing a positive effect on labour supply. In addition, the timing of AE enrolment in our sample, 1986–1989, coincides with a strong economic expansion period with low levels of unemployment. We cannot necessarily expect our results to generalise to all phases of a business cycle because different macroeconomic conditions may attract different groups of participants.

5 Summary and discussion

This paper has analysed whether adult education (AE) for prime-aged individuals at the compulsory or upper secondary level can be used as a policy measure to increase labour force participation during the later stages of working life. To answer this question, extended time-series data on AE and earnings are necessary. We use unique panel data on AE transcripts and annual earnings across 1979–2004 and 1982–2004, respectively. We further exploit the fact that, by international standards, AE has been exceptionally prevalent in Sweden.

The empirical analysis is based on a sample of individuals aged 42–55 (in 1986) with short educations. The groups we define as treated enrolled in AE during the

period 1986–1989. Matching on the propensity score is used to generate untreated comparison groups of non-participants. Potential effects on duration in the labour force are examined until 2004, i.e., up to and beyond the officially stated age of retirement. Our findings indicate that AE had no significant effect on the survival rate in the labour force. The results are robust with respect to alternative definitions of the timing of retirement and alternative definitions of treatment; they are also unchanged if we study sub-samples conditioned on year of birth, (prior-to-enrolment) educational levels or Stockholm residents.

Retraining the older workforce is often presumed to extend working life and is a cornerstone of both the European Union's so-called "Lisbon strategy" for growth and jobs and the OECD campaign for "active ageing". The results of this study indicate no effect of these efforts on the timing of the transition from labour force participation into retirement, thus weakening the economic rationale for AE in this case. According to our findings, proponents of AE for older workers would have to argue on the basis of its potentially positive effects on individual productivity while they remain in the labour force, and/or need to emphasize issues such as fairness, social equity, increased democracy and other similar considerations often included in the officially stated goals for publicly financed AE. As always, further studies that explore different economic environments will need to be conducted before stronger and more general conclusions can be drawn.

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Appendix

Figure A. 1 Number of registered credits among the treated, intervals of 50 AE credits.

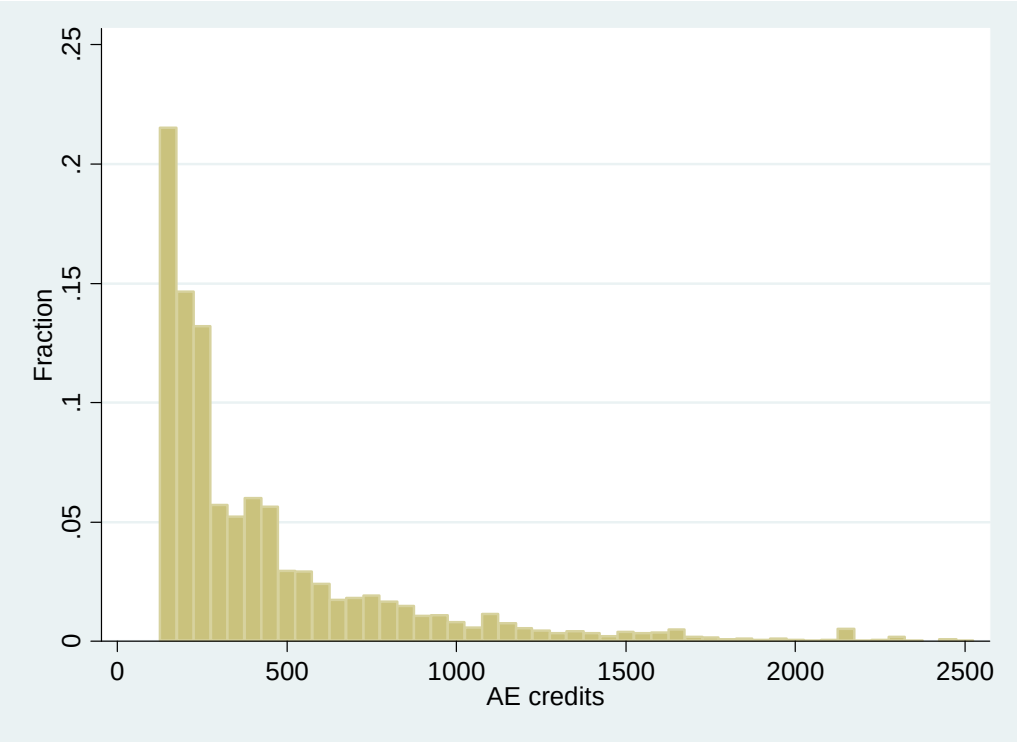


Table A. 1: Maximum likelihood estimation of a probit model for the propensity score.

Dependent variable: Course registrations in AE 1986-1989 exceeding 124 credits.

	Males		**Females**	
	Coefficient	S.E.	Coefficient	S.E.
Born 1931	- .743***	(.096)	- .450***	(.097)
Born 1932	- .582***	(.080)	- .532***	(.097)
Born 1933	- .483***	(.079)	- .484***	(.091)
Born 1934	- .460***	(.069)	- .427***	(.091)
Born 1935	- .334***	(.071)	- .514***	(.097)
Born 1936	- .359***	(.072)	- .295***	(.081)
Born 1937	- .335***	(.073)	- .198***	(.075)
Born 1938	- .254***	(.068)	- .412***	(.087)
Born 1939	- .206***	(.064)	- .166**	(.072)
Born 1940	- .142**	(.066)	- .077	(.068)
Born 1941	- .165***	(.063)	- .131*	(.067)
Born 1942	- .125**	(.058)	- .051	(.063)
Born 1943	- .072	(.061)	- .068	(.061)
Foreign-born	.112***	(.027)	.027	(.022)
Born outside Scandinavia			- .074*	(.038)
Unemployment benefits 1985 > 0 [a)]	.219***	(.048)	.242***	(.031)
Unemployment benefits 1985	-.000	(.000)	-.000	(.000)
9 years of schooling	.303***	(.030)	.164***	(.020)
1 year of upper secondary school	.393***	(.071)	.516***	(.015)
2 years of upper secondary school	.245***	(.020)	.244***	(.019)
- Commerce			- .066***	(.023)
- Manufacturing			- .095**	(.038)
- Transport			- .291***	(.048)
Regional employment	1.507***	(.495)	1.189***	(.179)
Stockholm county				
Inland of Norrland	.064	(.044)	.076***	(.024)
1 child at home	.046*	(.025)	.097***	(.015)
2 children	.043*	(.025)	.138***	(.016)
3 children	.103***	(.034)	.186***	(.025)
4 children	.093	(.068)	.231***	(.050)
5 or more children	.133	(.112)	.236**	(.095)
Earnings 1985	.000	(.000)		
Earnings 1985 squared	- .000***	(.000)	- .000***	(.000)
Earnings difference 1982-1985			.000***	(.000)
Earnings difference 1982-1985 squared			.000*	(.000)
Birth-year dummies * earnings 1985 [b]	No		Yes	
Birth-year dummies * earnings 1985 sq. [b]	Yes		Yes	

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

[a)] Dummy variable.

[b] 13 coefficients, not displayed.

Table A. 2 Balancing tests of covariates between treated and matched comparison groups.

	Males			**Females**		
	Treated	Matched comparisons	*p*-value[a)]	Treated	Matched comparisons	*p*-value[a)]
N	1341	5300		4957	18702	
Registered AE credits	391.3	9.27	*.000*	457.3	16.7	*.000*
Age	46.34	46.33	*.964*	46.92	46.87	*.531*
Born 1931	.019	.018	*.803*	.035	.035	*.935*
Born 1932	.026	.026	*.927*	.036	.036	*.968*
Born 1933	.032	.034	*.828*	.042	.041	*.762*
Born 1934	.040	.043	*.697*	.048	.046	*.569*
Born 1935	.052	.054	*.813*	.044	.046	*.725*
Born 1936	.049	.050	*.912*	.061	.060	*.924*
Born 1937	.049	.055	*.474*	.067	.065	*.702*
Born 1938	.069	.063	*.546*	.071	.068	*.581*
Born 1939	.084	.079	*.646*	.078	.078	*.985*
Born 1940	.085	.077	*.426*	.085	.084	*.864*
Born 1941	.094	.095	*.895*	.089	.090	*.881*
Born 1942	.123	.120	*.836*	.099	.100	*.834*
Born 1943	.119	.114	*.707*	.116	.118	*.702*
Born 1944	.160	.173	*.364*	.130	.133	*.640*
Earnings 1985 [b)]	944.36	950.82	*.734*	617.37	623.74	*.281*
Earnings difference 1982-1985 [b)]	184.60	201.39	*.213*	161.11	164.12	*.505*
Earnings 1985 of born 1931 [b)]	19.10	16.99	*.705*	22.25	22.65	*.877*
Earnings 1985 of born 1932 [b)]	27.50	27.27	*.975*	21.51	22.62	*.660*
Earnings 1985 of born 1933 [b)]	31.22	32.85	*.831*	26.85	26.40	*.873*
Earnings 1985 of born 1934 [b)]	39.38	45.14	*.531*	30.01	29.45	*.851*
Earnings 1985 of born 1935 [b)]	50.61	56.02	*.579*	28.83	29.70	*.770*
Earnings 1985 of born 1936 [b)]	45.39	46.94	*.865*	37.99	37.69	*.929*
Earnings 1985 of born 1937 [b)]	44.41	53.90	*.305*	41.41	39.83	*.643*
Earnings 1985 of born 1938 [b)]	66.26	58.64	*.457*	46.14	45.65	*.892*
Earnings 1985 of born 1939 [b)]	80.76	74.80	*.603*	48.56	48.54	*.997*
Earnings 1985 of born 1940 [b)]	78.72	74.01	*.669*	51.88	52.38	*.897*
Earnings 1985 of born 1941 [b)]	91.12	93.15	*.869*	56.30	57.36	*.793*
Earnings 1985 of born 1942 [b)]	116.65	120.11	*.807*	59.77	60.36	*.885*
Earnings 1985 of born 1943 [b)]	107.85	.99.31	*.496*	70.67	71.25	*.895*
Earnings 1985 of born 1944 [b)]	145.38	151.69	*.669*	75.21	79.85	*.314*
Foreign-born	.178	.182	*.782*	.140	.140	*.965*
Foreign-born from Scandinavia	.084	.087	*.796*	.073	.074	*.908*
Born outside Scandinavia	.094	.095	*.908*	.067	.066	*.856*
Unemployment benefits 1985 > 0	.111	.123	*.352*	.093	.096	*.718*
Unemployment benefits 1985 [b)]	20.27	22.25	*.517*	13.77	14.02	*.834*
Less than 9 years of schooling	.374	.382	*.676*	.280	.278	*.797*
9 years of schooling	.140	.135	*.705*	.115	.119	*.574*
1 year of upper secondary school	.022	.022	*.895*	.303	.303	*.974*
2 years of upper secondary school	.464	.460	*.854*	.302	.300	*.861*
- Commerce	.065	.057	*.420*	.122	.119	*.601*
- Manufacturing	.280	.284	*.830*	.026	.026	*.975*
- Transport	.034	.039	*.505*	.013	.146	*.520*
Regional employment	.835	.835	*.847*	.767	.767	*.808*
Stockholm county	.178	.173	*.732*	.175	.174	*.942*
Inland of Norrland	.060	.053	*.380*	.064	.063	*.918*
Number of children at home	1.272	1.280	*.854*	1.244	1.250	*.763*
1 child at home	.253	.255	*.921*	.327	.320	*.465*
2 children	.281	.278	*.838*	.287	.287	*.956*
3 children	.107	.114	*.570*	.085	.088	*.561*
4 children	.022	.023	*.897*	.016	.016	*.826*
5 or more children	.008	.008	*.529*	.004	.006	*.350*

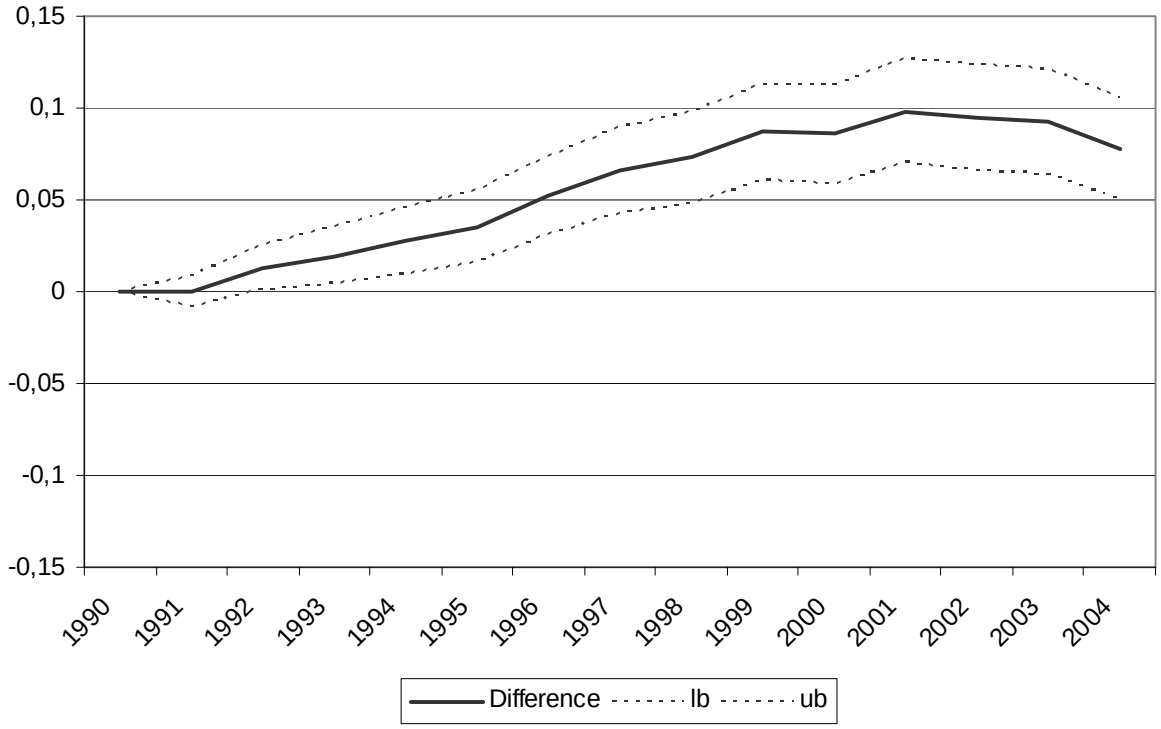
[a)] T-test of equality between treated and matched comparison means.
[b)] In 100s of SEK; equal mean values of squared terms are also rejected but not displayed.

Table A. 3 Survival rate confidence intervals of treated and matched comparison groups.

	Males			**Females**		
		Survival rate, 95 % confidence intervals			Survival rate, 95 % confidence intervals	
Time	N_T / N_C	Treated	Matched comparisons	N_T / N_C	Treated	Matched comparisons
1990	1341 / 5300	1	1	4957 / 18853	1	1
1991	1246 / 4936	.964 - .982	.977 - .985	4723 / 17271	.970 - .979	.967 - .972
1992	1208 / 4812	.938 - .962	.942 - .955	4591 / 16697	.940 - .953	.939 - .946
1993	1176 / 4625	.896 - .927	.906 - .922	4451 / 16177	.900 - .916	.904 - .913
1994	1120 / 4428	.859 - .896	.876 - .893	4253 / 15558	.870 - .889	.872 - .881
1995	1070 / 4258	.830 - .870	.848 - .868	4114/ 14956	.840 - .861	.841 - .852
1996	1030 / 4101	.798 - .840	.815 - .837	3960 / 14398	.800 - .823	.802 - .814
1997	986 / 3918	.746 - .794	.774 - .797	3763 / 13711	.755 - .779	.752 - .765
1998	923 / 3705	.698 - .748	.736 - .761	3540 / 12823	.707 - .732	.707 - .720
1999	859 / 3505	.655 - .707	.693 - .719	3307 / 11994	.648 - .675	.656 - .670
2000	802 / 3290	.593 - .648	.637 - .664	3022 / 11108	.588 - .616	.599 - .614
2001	726 / 2996	.548 - .604	.583 - .611	2737 / 10121	.521 - .550	.539 - .554
2002	668 / 2726	.490 - .546	.530 - .559	2425 / 9067	.452 - .481	.478 - .493
2003	597 / 2465	.427 - .483	.473 - .502	2103 / 8008	.397 - .426	.419 - .434
2004	440 / 1889	.357 - .413	.409 - .438	1553 / 5971	.335 - .363	.354 - .372

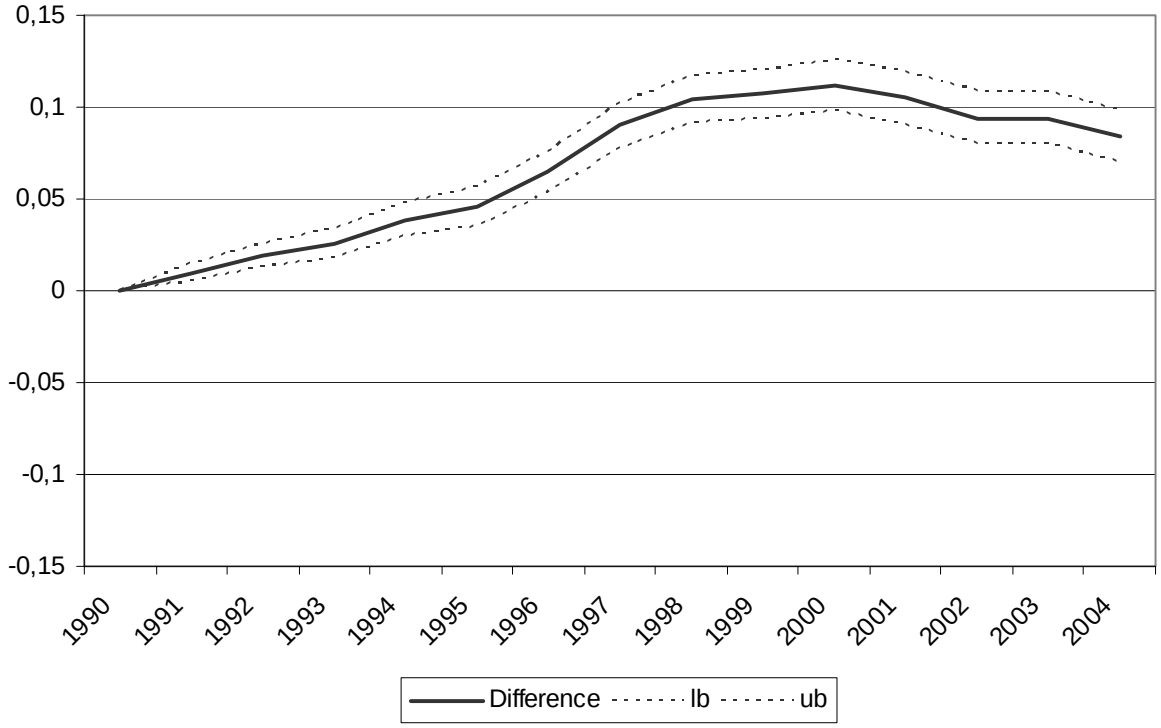
Note: N_T / N_C yields the number of individuals in the treated and the matched comparison group.

Figure A. 2 Males, unmatched samples. Treated vs. untreated differences in Kaplan-Meier estimates of survival rates in the labour force 1990–2004.



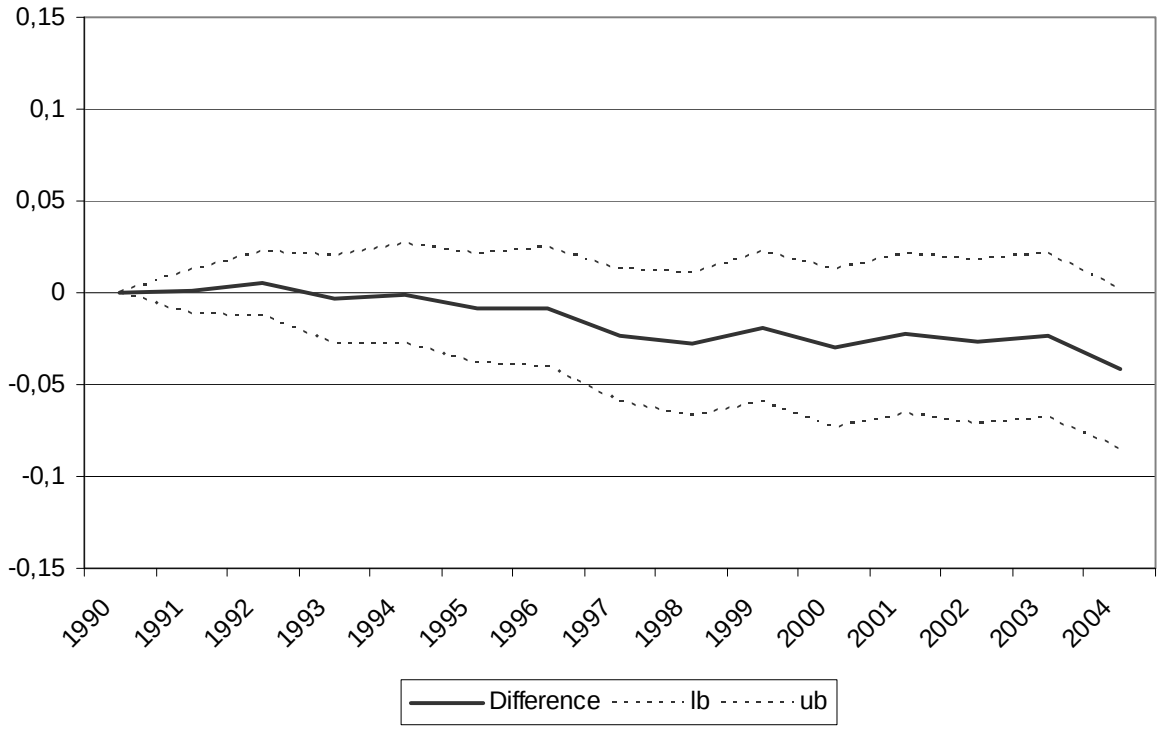
Note: No. of obs., see Table A.3, lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 3 Females, unmatched samples. Treated vs. untreated differences in Kaplan-Meier estimates of survival rates in the labour force 1990–2004.



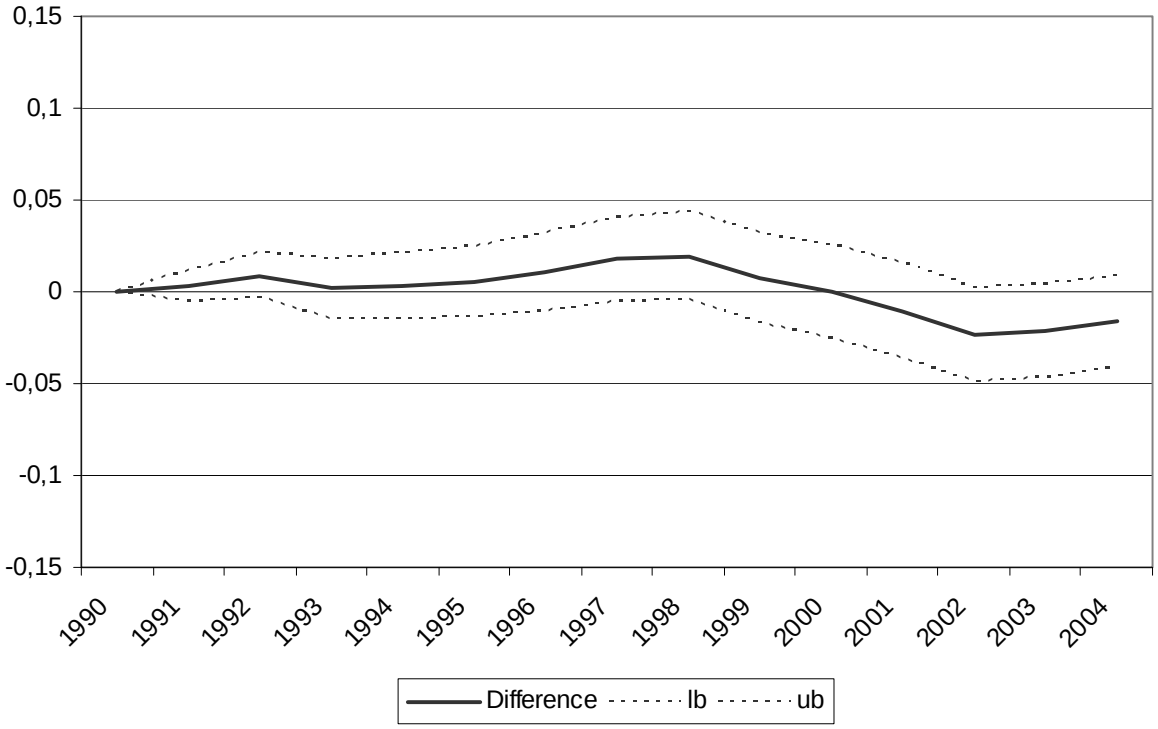
Note: No. of obs., see Table A.3, lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 4 Males with no upper secondary education in 1985. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



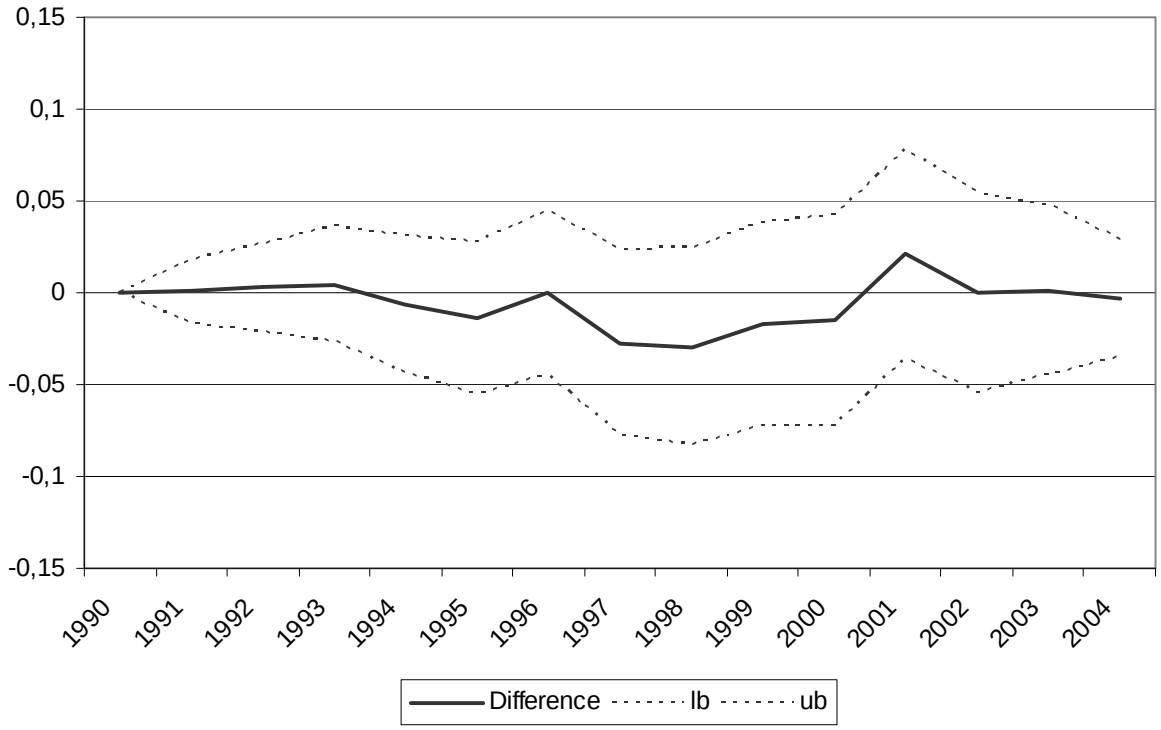
Note: N_T / N_C = 690/2741. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 5 Females with no upper secondary education in 1985. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



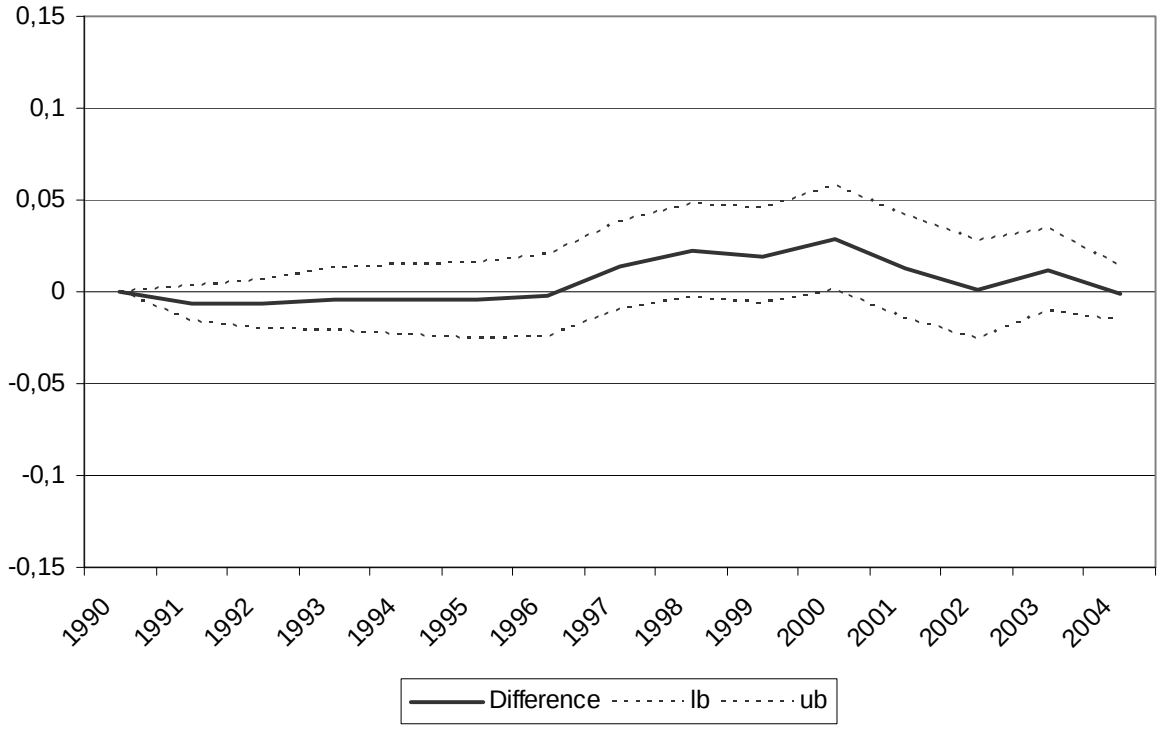
Note: N_T / N_C = 1961/7637. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 6 Males born 1935–1939. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



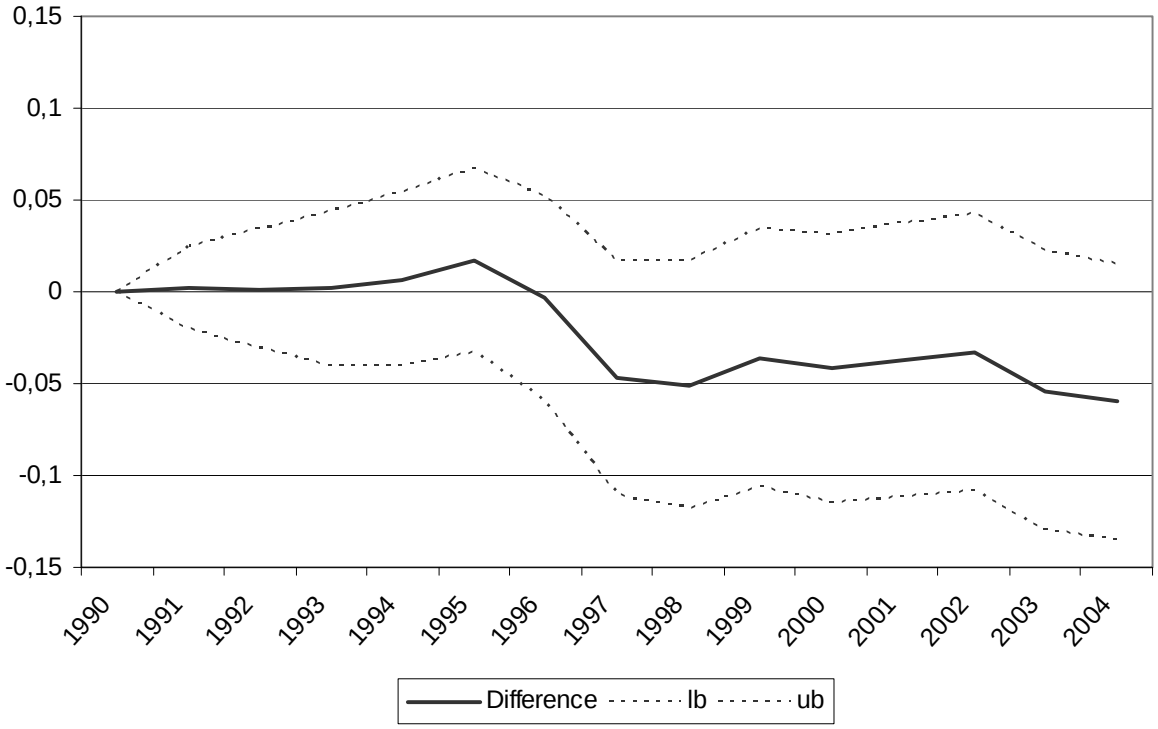
Note: N_T / N_C = 406/1597. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 7 Females born 1935–1939. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



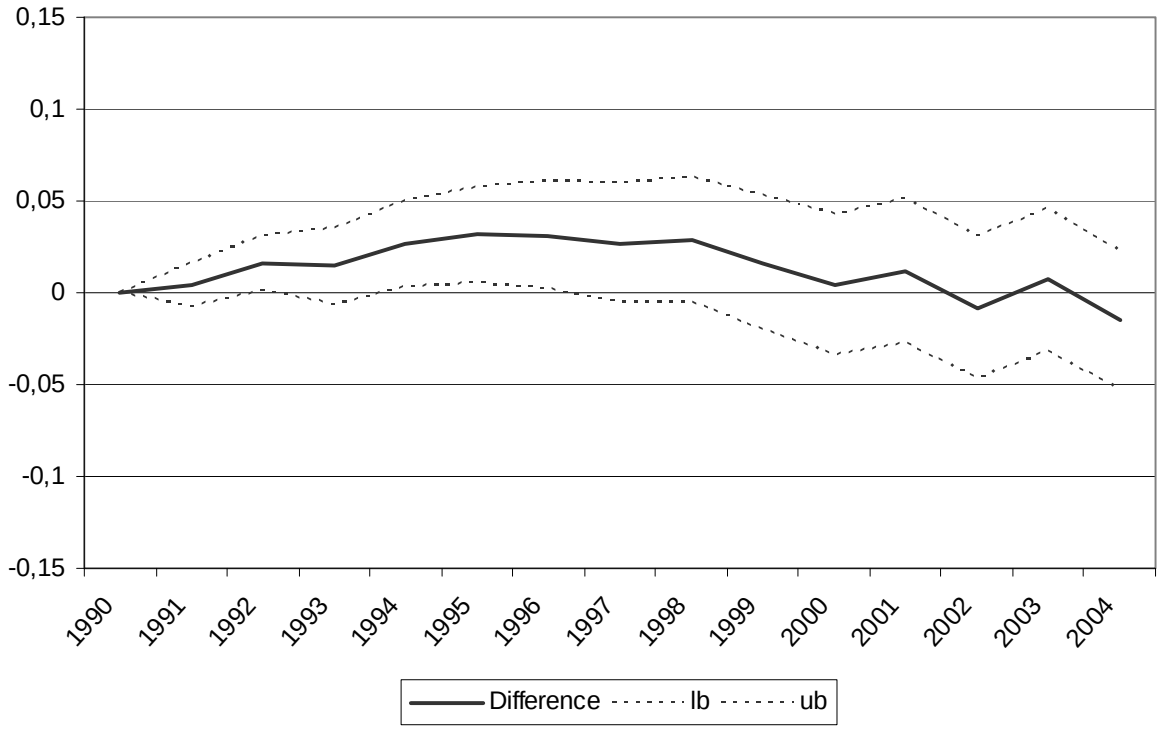
Note: N_T / N_C = 1592/5808. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 8 Males residing in Stockholm in 1985. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



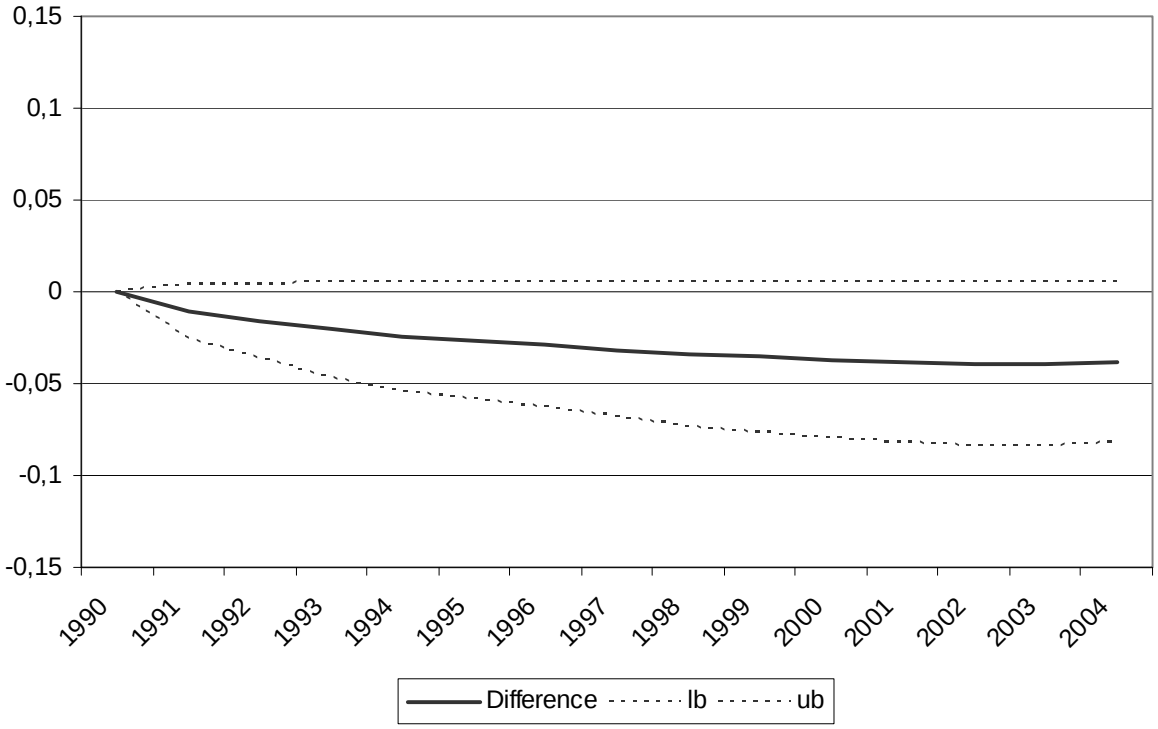
Note: N_T / N_C = 239/938. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 9 Females residing in Stockholm in 1985. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



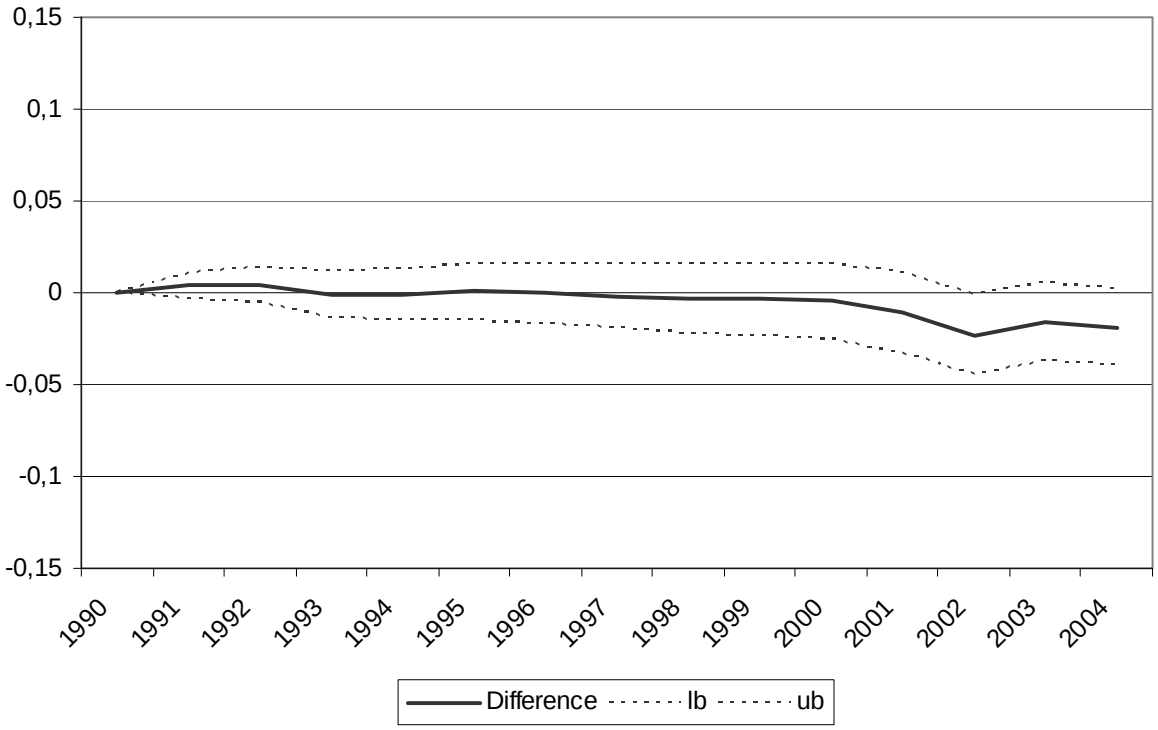
Note: N_T / N_C = 867/3282. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 10 Males treated with at least 250 registered course credits. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



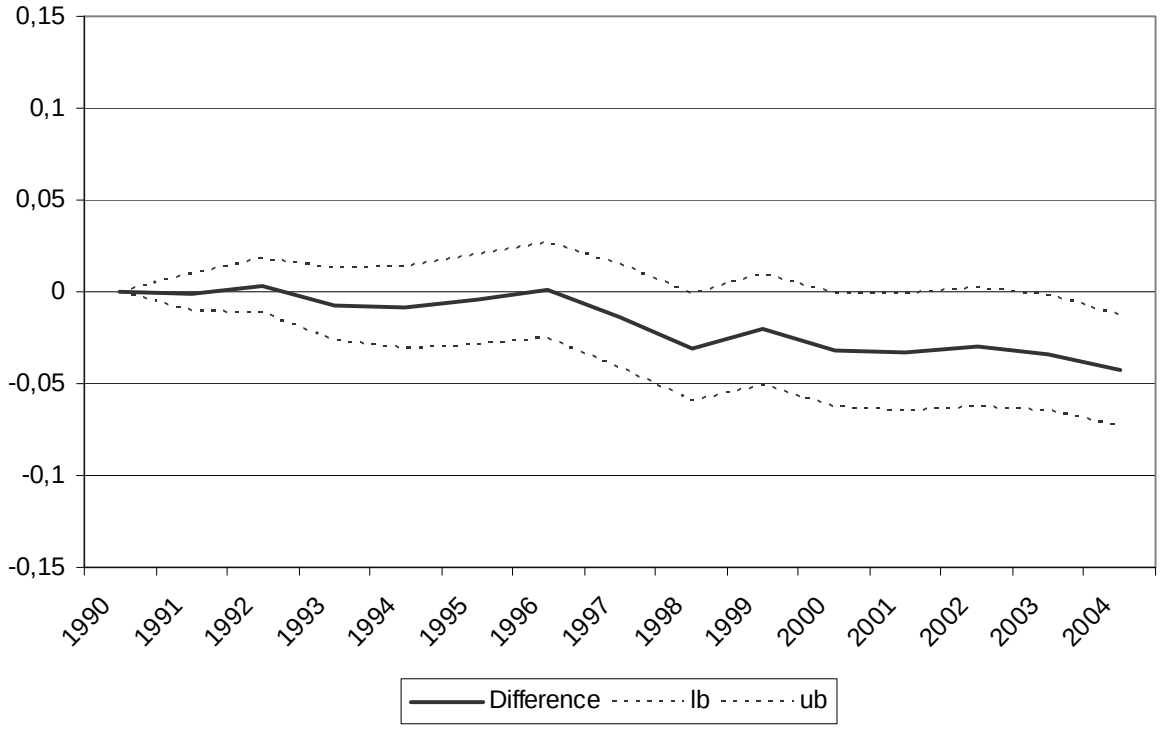
Note: N_T / N_C = 696/2746. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 11 Females treated with at least 250 registered course credits. Treated vs. matched comparison group differences in Kaplan-Meier survival rates.



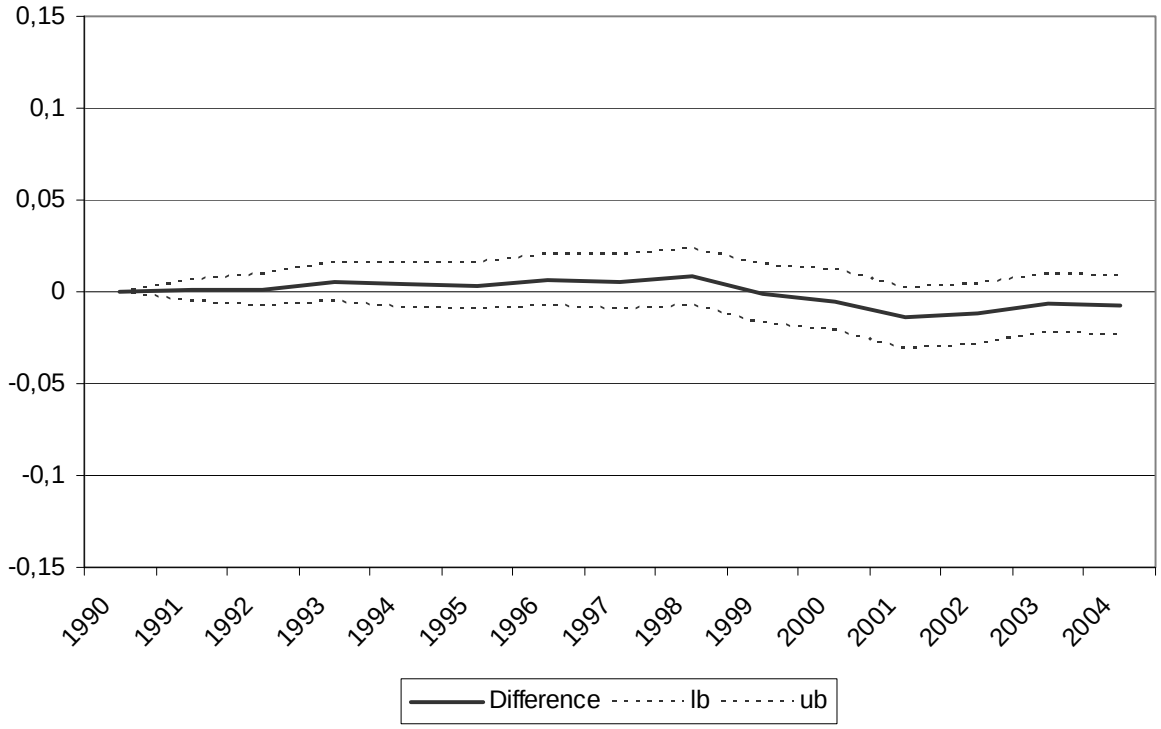
Note: N_T / N_C = 2845/10937. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 12 Exit defined by the time pensions (p_{it}) > zero; males. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



Note: N_T / N_C = 1341/5300. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

Figure A. 13 Exit defined by time pensions (p_{it}) > zero; females. Treated vs. matched comparison group differences in Kaplan-Meier estimates of survival rates.



Note: N_T / N_C = 4957/18702. lb = lower bound of 95 per cent confidence interval; ub = upper bound.

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