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ABSTRACT

A Note on Evaluating Formal Education for Adults*

Evaluations of adults in formal education (AE) are typically based on earnings measured 5-10 years after program enrollment. This paper estimates returns up to 24 years after enrollment, and explore results for 15 cohorts of first-time registered in AE in Sweden 1994-2008 with at least a 10-year follow-up period. The results indicate substantially higher payoffs in absolute terms after the maximum length of follow-up compared with after 10 years. There is also weak support that multiplier effects increase the percentage returns to AE over time, regardless of gender or whether the level of AE is college or high school.

JEL Classification: H30, H52, I20, J24, O30
Keywords: adult education, self-selection, propensity score matching

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1. Introduction

In parallel with technological developments, the increase in educational attainment across OECD countries is slowing down. The educational attainment of the current generation is predicted to be similar to that of the preceding generation, and in the US it is actually decreasing (Goldin and Katz 2008, OECD 2021, Castro and Coen-Pirani 2016). Given that schooling is widely believed to be a key factor for economic growth and individual labor market outcomes, it is possible that late educational investments will increase (Neumark et al. 2011). However, for many adults, participation is linked to formidable obstacles due to opportunity costs and/or credit constraints. To counteract the fact that market imperfections reduce human capital investments, international bodies such as the OECD and the EU have long encouraged governments to stimulate adult education to adjust workers' skills to technical changes (OECD 1998, 2001, 2006, 2021, EU 2000, 2001). One suggestion is to use regular education for adults as a counter-cyclical policy tool, because the opportunity costs of education decrease during economic downturns (Pissarides 2011). However, national governments may for several reasons find it difficult to gain political support for such programs. First, because formal education for adults is an expensive measure, not least in terms of foregone earnings and production, and second, because the payoff to society (and the individual) is uncertain. Consequently, there are few countries that have applied such policies on a large scale, so research on formal adult education is therefore scant. This is unfortunate since research that reduces uncertainty about the potential payoff may, if results are positive, contribute to encouraging policy makers to initiate AE programs. The evaluations that do exist are typically concentrated in a few countries and are based on follow-up periods of five to 10 years. While this has been an important step forward, the theoretical predictions on a longer-term follow-up period are ambiguous. The long-term estimates may tend to taper off if human capital is depreciated (Ben-Porath 1967, Johnson 1970), or the returns may increase if

there are multipliers at work that enhance human capital accumulation (Cunha and Heckman 2007).

The purpose of this paper is to use Swedish data to assess to what extent evaluations of AE after 10 years are generalizable to longer-term follow-up periods. Sweden is suitable for the analyses because AE participation is very high, at both upper secondary and college levels, and high-quality data from population registers are available to provide appropriate samples of participating individuals as well as comparison groups. Using difference-in-difference propensity score matching with unusually rich data, longitudinal Swedish administrative population data allow for estimated returns of AE up to 24 years after the program's start, that is, from 1994 to 2018. This is substantially longer than most previous studies. The generalizability of the findings is examined by re-estimating close to identically specified models for first-time registered participants each year between 1994 and 2007, comparing estimates based on annual earnings in 2018 with those obtained 10 years after enrolment. Before concluding, the paper also provides a tentative discussion on potential explanations for variation in estimated magnitudes between cohorts of participants.

The contribution to the literature stems from the fact that few estimates that have been reported until now have been based on earnings more than 10 years after the program's start. Consequently, with the usual reservation that the analyses are limited to a particular context, this paper explicitly addresses the notion that the payoff could vary post-treatment depending on the length of the follow-up period. Due to data limitations, this issue has not received much attention in the literature. A better understanding of the long-term impact of AE may have important consequences for the interpretation of results based on shorter follow-up periods, notably on how we assess the benefits to society, as indicated by estimates presented in the existing literature.

The results indicate that estimates in percentage terms increase over time. When using the preferred specification, the increase is between 0.8 percent (females in college) and 1.9 percent (males in high school). Thus, there is weak support for the hypothesis that there are multiplier effects that increase the returns to AE over time. In contrast, there is no support for the notion of human capital depreciation. Turning to estimates in absolute terms, these typically increase by at least 50 percent if estimates were statistically significant after 10 years. This increase in absolute terms is of major importance for any assessment comparing the social costs and benefits, as it increases the probability that AE investments cover the investment costs involved.

2. Literature review

The overview in this section is focused on studies of community colleges from the US and on studies using Swedish data. Sweden is an interesting country as the institutional factors stimulate AE participation at both the upper secondary and college levels, largely in line with the recommendations of the EU and the OECD. One consequence of this is that enrollment among older students is relatively common.[1]

The literature on AE has developed during the last 15 to 20 years. Following the seminal work by Jacobson, Lalonde and Sullivan (1993) on the earnings consequences of being laid off, the same authors focused in later studies on laid-off workers aged 25 to 59 who registered at community colleges in Washington State (Jacobson et al. 2003, 2005a, 2005b). About two-thirds of the participants were females, a feature that has been relatively consistent in this literature. Their individual fixed effects estimates indicated earnings increases of about

[1] For more detailed overviews, see Belfield and Bailey (2017) for a US-focused survey, McCall et al. (2016) also covers labor market training programs with separate chapters for the US, the UK, France, Sweden and Denmark whereas Carruthers and Jepsen (2021) provides a discussion on the significance of vocational education in the broader international perspective.

10-13 percent for females and 7-9 percent for males, with their longest follow-up period being seven years after program entry. The estimated returns were slightly higher than suggested by the earlier studies of participants in community colleges. [2] Jepsen et al. (2014) compared individuals in Kentucky completing community college certificates, diplomas, or associate degrees with individuals who did not complete any of these awards (approximately 70 percent of the students). The average age at enrollment in their sample was 30 years, and their estimates were based on earnings five years after enrollment. The results implied returns comparable with Jacobson et al. or considerably higher. Stevens et al. (2019) evaluated career technical education programs in California community colleges, and notably had access to better pre-program control variables, enabling them to control for pre-enrollment earnings and earnings trends. They found estimated returns to Career and Technical Education (CTE) certificates and degrees to range from 15 to 23 percent, and even up to 45 percent for programs related to the healthcare sector. As longitudinal data have become increasingly accessible in a number of US states, several similar studies have been published. [3] These studies evaluate the impact of credit accumulation, certificates, and associate degrees from community colleges primarily for individuals under the age of 30. Several studies indicate increasing returns five to nine years after exiting community college. Belfield and Bailey (2017, p18) note that the results in Minaya and Scott-Clayton (2017) indicate greater estimates after nine years compared with after five years, and simulate calculations that include growth in returns over the life cycle. However, few studies have had access to data with longer follow-up periods to examine to what extent this is a reasonable assumption. This

[2] Prior to these, evaluations had mainly concerned enrollees aged below 30. The reported returns to a year of completed studies at a community college were similar to those observed for individuals of typical ages when attending education, about 5 percent for males and 5-10 percent for females (Kane and Rouse 1995, Light 1995, Monks 1997, Leigh and Gill 1997).

[3] Bahr et al. (2015), Dadgar and Trimble (2015), Liu, et al. (2015), Zeidenberg et al. (2015), Bettinger and Soliz (2016) and Xu and Trimble (2016). See Belfield and Bailey (2017) for a summary.

is unfortunate since it may have important implications for a cost-benefit assessment from the society's point of view.

Evaluations based on Swedish data provide a context that differs slightly from the US.[4] The education system in Sweden comprises nine years of compulsory school, with almost no tracking. At age 16, individuals apply for upper secondary school. Since the late 1980s, around 90 percent of all pupils have enrolled in either one of the 15-20 vocational 2-year programs or one of the five academic 3-year programs. A reform implemented in 1994 extended the vocational programs to also encompass 3-years, and thereby meet the general admission requirement for university studies. Thus, for individuals born in 1978 or later, the completion of 3-year vocational programs at upper secondary school also yields eligibility to university which may reduce the demand for upper secondary AE. However, the reform may also increase the demand for AE among drop-outs if completion of upper secondary school becomes a pre-requisite in the labor market.

In Sweden, participation in AE has been substantial since the 1970s, with several institutional factors stimulating the supply and the demand of AE, and also enhancing participants' completion rates. First, all public education in Sweden is free of charge. Second, employees have a legal right to be on study leave and then be reinstated by the employer with similar working conditions. Third, individuals are eligible to apply for study allowances, roughly equal to USD 1,200 per month, of which one-third is to be repaid within a period of 25 years. Fourth, education is free of charge, whether conducted at upper secondary level or at college. Fifth, municipalities have since 1969 been obliged by law to supply upper secondary

[4] The overview is focused on studies from the US and Sweden since evaluations of AE in Europe are rare. Böckerman et al. (2018) is an excellent exception as it evaluates polytechnic attendance in Finland including samples of older students aged 25-50. Their estimated earnings returns are 12.7 percent (comparing estimates with reported average earnings) 10 years after program entry.

education for adults. The institute responsible for this supply is known as *Komvux*. AE at Komvux presents an opportunity to redirect or extend fields of education, and/or to gain eligibility to university. Tertiary education at universities or polytechnics (*högskola*), referred to in the following as *college* education, is offered in about 30 cities (in a population of 10 million).

Stenberg (2011) analyzed a sample of adults at Komvux, aged between 24 and 43 at the time of first registration. The estimates indicated that one year of completed studies increased annual earnings 10 years after the program's start by 5.1 percent for females and 2.3 percent for males. Stenberg et al. (2014) study an older sample, aged 42 to 55, and find positive earnings estimates for females but insignificant results for males. [5] For college enrollees aged 29-55, Stenberg and Westerlund (2016) report positive earnings estimates of approximately 10 percent for females and 5.5 percent for males when divided by the average years of completed AE. The slightly lower estimates, compared with Jacobson et al. (2005a), may reflect a wider dispersion of wages and skills in the US or the fact that public support to encourage AE participation in Sweden potentially attracts individuals with lower expected returns on average.

3. Empirical method and estimations presented

The empirical analyses in this paper are based on population register data from Statistics Sweden, primarily the LISA database 1990-2018 which includes information on earnings and public transfers. Similar to most studies on Swedish data, referred to above, the samples of participants consider only first-time registered students in Komvux or college (the registers go back to 1977) where the age interval is set at 25 to 40 for participants in Komvux (upper

[5] Also, for low skilled individuals at Komvux, Stenberg and Westerlund (2008) find a payoff of between 15 and 20 percent for the long-term unemployed aged 25-55. The large impact in percentage terms is partly explained by that estimates are set against the relatively low average earnings of the sample.

secondary level) and 29 to 40 for college enrollees.[6] The comparison groups are gathered from population registers conditioned on the same criteria. The earliest participants were registered in 1994, which means we have earnings data from at least four years prior to enrollment. The treatment status of individuals in a particular year is not conditional on future events, including registration in AE later years.

3.1 Propensity score matching

To evaluate the impact of AE on earnings, the method used is difference-in-difference propensity score matching. The rich data make it possible to balance treated and matched comparisons on more than 125 variables. [7] In particular, focusing on individuals aged 25 or older allows controls for pre-treatment earnings levels and trends as important controls for selection. A novelty of the data is access to information on population ability rank as measured in the ninth grade (age 16) by the grade point average (GPA) across 10 to 12 school subjects. An advantage is that the curricula are standardized at a national level, which gives the GPA measure a high degree of comparability within a birth cohort. The GPA is available for the full population born in 1972 or later (from 1988) and for the majority of our samples

[6] The estimated probabilities are considerably higher for participants and non-participants. Taking the median value of the probability to enroll, the average share of participants with a probability above the median is 84 percent. Using younger age floors increases the share in the comparison groups who later enroll in AE, and estimates will then to an increasing extent compare individuals who only enrolled at different points in time. Earlier studies find that applying these age floors reduces this problem so that it has only a modest influence on the results.

[7] The estimated probabilities are considerably higher for participants and non-participants. Taking the median value of the probability to enroll, the average share of participants with a probability above the median is 84 percent. For estimates presented in Section 4, the matched samples are balanced on dummies for age, for school years prior to enrollment, for ability rank in the population, for type of upper secondary school (five categories), for local labor markets (21 categories), for the number of children at home, for the number of children (four categories), for age of children (six categories), for being married or divorced and for country of origin (seven categories). The matched samples are also balanced on labor market outcomes in each of the four years prior to enrollment. These include the level of labor earnings, a dummy for zero labor earnings, for being employed and for the sector of employment (two categories, but only measured in the year prior to enrollment). In each of the four years prior to enrollment, the treated and untreated are also balanced on the levels of received public insurance transfers, separately for parental leave, sick-leave and unemployment, as well as dummies for each year and each benefit indicating if level is non-zero, in that case also including early retirement pensions, social welfare. Following Heckman and Smith (1999), balancing variables also include transitions in the two years prior to enrollment between employment, unemployment and outside the labor force, in total nine possible transitions.

born earlier via administrative records of applications to high school (1971-1987). [8] For individuals whose GPA is missing, the mean of the estimation sample is imputed to minimize the influence on the individual's propensity score (specifications also include a dummy, which is one if the value is imputed and otherwise zero). Section 5.2 presents robustness checks where the ability rank is excluded from the specifications.

Our outcome variable of interest is the annual labor earnings difference, $\Delta Y_{it+} = (Y_{it+} - Y_{it-1})$, where t + denotes a year after treatment and t-1 is the year immediately before treatment. The approach builds on the idea of the potential outcomes framework, which means that for each individual we will observe only one of the post-treatment outcomes, ΔY_{1i} if treated, or ΔY_{0i} if non-treated. Letting $D_{it} = 1$ denote treated and $D_{it} = 0$ untreated, the key assumption is that controlling for our observable covariates, X_{it-1}, removes any relevant average differences between treated and untreated. If this assumption holds, Rosenbaum & Rubin (1983) showed that it also holds for some function $P(X_{it-1})$, the propensity scores, which for a particular sample are probit model estimates of the probability of treatment. The average treatment effect on the treated in a year t+ may be denoted:

$$\Delta Y_{ATT} = [\Delta Y_{1i} | P(X_{it-1}), D_{it} = 1] - [\Delta Y_{0i} | P(X_{it-1}), D_{it} = 0]$$

The size of the bias remaining in this estimator hinges on how well the model manages to estimate the unobserved counterfactual ΔY_{0i}, that is, the earnings change without treatment. Formally the bias can be expressed:

$$E[\Delta Y_{0it} | X_{it-1}, D_{it} = 1] - E[\Delta Y_{0it} | X_{it-1}, D_{it} = 0].$$

[8] For males with missing values, we impute the ability rank based on military enlistment test, usually taken at age 17 or 18. The share of missing observations is therefore higher for females, but reduced as our samples are born in later years (see Table 1 and Table 2).

While the model controls for a rich set of observables, the difference-in-difference framework also means we control for permanent unobserved individual factors that influence earnings. The main caveat is thus dynamic unobserved factors that are relevant for earnings. This raises the issue of upward bias due to an Ashenfelter's dip (Ashenfelter 1978), which is that the earnings of treated individuals tend to drop immediately before a program starts. [9] The results section will therefore include both an *extended specification* with a full set of controls and a *reduced specification* where covariates related to labor market outcomes in the year prior to enrollment are not considered. To assess the size of the potential remaining bias in the estimates, one may consider Heckman et al. (1998) and Heckman and Smith (1999), who were able to compare experimental evidence of training programs with results based on selection on observables. These studies found that high quality data and careful matching between treated and untreated pre-program characteristics eliminated most of the bias in the evaluation of a prototypical labor market program. Among the most important controls were local labor markets and labor market transitions prior to enrollment. They also speculated that the bias could have been even further reduced with better data on family background characteristics. The Swedish data include such controls as well as variables that were not available in those studies, for example, regarding family situation and public transfers related to parental leave. While modest bias is likely to remain, one would need strong assumptions regarding the influence of unobserved factors to dismiss the results presented.

The probit estimates that are used to estimate the propensity score are presented for the 1994 samples in Tables A1 and A2 in the Appendix, separately for females and males in

[9] The issue is addressed in detail for Komvux by de Luna et al. (2011) and for college by Stenberg and Westerlund (2016). The Ashenfelter's dip refers to that individuals prior to enrollment may lose motivation and earn less than they normally do, which generates a risk that the researcher over-estimates the payoff when the individual goes back to a "normal" income after the program. However, individuals who also earn less prior to enrollment may be on a path towards long term unemployment. In such a case it is correct to include the lower earnings prior to program as a control.

Komvux and college respectively. [10] The ensuing results from matching on the propensity score are based on four-to-one matching throughout, mainly motivated by the fact that it facilitates the balancing of samples, and limited to areas of common support (i.e. values of P(X) where there are both treated and untreated). The descriptive statistics of participants and comparisons are provided for a selection of the variables in the leftmost columns of Tables A3 through A6. The rightmost columns of these tables include balancing tests of these very variables. For the matched samples, all covariates mentioned in footnote 7 are balanced for each cohort and respective sample. [11]

3.2 Participants' characteristics 1994 – 2008

Table 1 (Komvux) and Table 2 (college) present descriptive means of selected variables for treated samples enrolled for the first time in 1994, 1995, 1996 etc, for each year until 2008. The time period was characterized by a secular increase in education such that the mean ability rank among college enrollees tended to drop slightly for each year between 1994 and 2008.

Participation in Komvux was closely related to the structure of the educational system at upper secondary level, where individuals choose a field of study in tenth grade at the age of 16. The choice was between five academic 3-year programs, and a variety of mostly vocational 2-year programs. Komvux participants often complete a year of upper secondary school, either to gain a different degree from the one they completed at a younger age or to

[10] The estimated probabilities are considerably higher for AE participants than for non-participants. Taking the median value of the probability to enroll 1994-2008, the average share of participants with a probability above the median is 84 percent.

[11] Given that the balancing tests encompass such large number of covariates, one variable is allowed to be imbalanced (significantly different at a 5 percent level) in each test and a few additional imbalances are also allowed in cases where a covariate represents less than 1 percent of the treated individuals of that particular sample. These appear to have minimal influence on the presented estimates.

gain eligibility to college studies.[12] However, participants' characteristics were also influenced by the labor market policy of the period. Between 1997 and 2002, due to high unemployment levels by Swedish standards (slightly below 10 percent), the government introduced the Adult Education Initiative which enabled all individuals eligible for unemployment insurance (UI) benefits to study for one year at Komvux with financial support equal to their UI, which is higher than a study allowance and not tied to repayments. This became very popular and partly saturated the need for Komvux at the beginning of the 2000s, notably attracting groups with a lower GPA as measured at the age of 16.

4. Main results

This section presents the main results, separately for females and males, and separately for Komvux and college. As a point of departure, Section 4.1 introduces the data and estimations applied by presenting estimation trajectories of the 1994 cohorts. The main focus of the analyses is on how estimates vary with a longer follow-up period. Section 4.2 includes all cohorts of participants from 1994 to 2008 and discusses to what extent the relationship between 10-year-estimates and maximum length of follow-up remains similar across cohorts.

Results are presented throughout in both absolute terms and in percentage terms. Real wage growth is expected in a sample aged 25 to 40, which means the absolute estimates may increase even if percentage estimates remain constant. From a policy perspective, one may argue on the one hand that the absolute estimates are of primary interest as they measure the potential increase in tax revenue. On the other hand, the percentage returns account for

[12] A reform of the upper secondary school in 1992 extended the 2-year programs to 3 years, so the need for Komvux was slightly altered to become less focused on eligibility for college. This regard cohorts born in 1976 or later, at most seven of the 16 age-groups (enrolled in 2008).

increased productivity which may be considered more closely related to the theoretical question of education and labor market outcomes. [13]

4.1 Results trajectories for 1994 enrollees – do estimates taper off or increase over time?

Table 3 (Komvux) and Table 4 (college) present results of the reduced specification and extended specification for individuals enrolled in AE for the first time in 1994. To facilitate the discussion, the results of the extended specifications are also illustrated in Figure 1 in the form of earnings trajectories for the 1994 enrollees and their matched comparison groups. The focus for now is on the relationship between estimates 10 years and 24 years after program entry.

To start with females in Komvux, the absolute estimate after 24 years is more than twice as high as compared with after 10 years (29,281 vs. 11,765). [14] While the absolute estimates are interesting in their own right, they may partly reflect increased labor market experience as well as real earnings growth (about 30 percent in Sweden between 2004 and 2018). In fact, the increase in the estimates is roughly similar to the overall increase in earnings among matched comparisons, that is, the estimated counterfactual earnings of the treated individuals. The payoff in percentage terms therefore increases only modestly from 4.8 to 6.5 percent. For males in Komvux, the growth in the absolute estimates between 10 years and 24 years after program entry is similarly large (27,501 vs. 10,925) and the estimates expressed in percentage terms increase from 3.9 percent to 6.3 percent.

Turning to college enrollees, the absolute estimates after 24 years are 76 and 78 percent higher than after 10 years for females and males respectively (75,444 vs. 42,947 and 72,893 vs. 41,037). These increases are again mitigated in terms of percentage estimates as

[13] The percentages presented are defined as the ratio between the estimation of ATT in absolute terms and the counterfactual earnings in a given year (i.e. the average earnings of the comparison group), divided by the increase in completed school years as reported for a given year.
[14] Estimates are presented throughout in SEK; divide roughly by 8.50 to calculate the USD amount.

counterfactual earnings also grew during the period. For females, the percentage estimates increase from 8.8 to 10.2 percent whereas for males they increase from 8.2 percent after 10 years to 10.5 percent in 2018.

Considering that existing studies are typically based on estimates between five and 10 years after program entry, the main takeaway from the results in Table 3 and Table 4 is that there may be large increases in absolute estimates over time. The increase in absolute terms is important for calculations of social costs and benefits since a higher absolute estimate implies a larger tax base and raises the likelihood that the benefits cover the costs of providing AE, i.e. the costs of teachers and premises as well as foregone earnings during AE enrollment. However, there is only suggestive evidence that this would reflect multiplier effects of AE, since the increase in estimates over time in large part is parallel to the earnings increase of the matched comparisons. The results provide even less support for human capital depreciation, i.e. that the impact of AE tapers off, as the percentage estimates have not decreased after 24 years as compared with after 10 years.

One reservation about these conjectures is that the estimates of the 1994 cohort may have limited validity, for example, if results are sensitive to business cycle variations, technological changes, to the composition of the participants or if the educational contents changes. We turn next to comparing estimates for different cohorts registered for the first time between 1994 and 2008.

4.2 Results for 15 cohorts – do estimates taper off or increase over time?

To address the stability of the findings based on the 1994 cohort, this section presents estimates based on the same empirical approach but moved forward by one year, two years, three years etc, up to fourteen years later (enrollees in 2008). The comparison includes both estimates 10 years after program entry for *all* cohorts and estimates based on earnings in

2018. This allows us to assess to what extent the comparisons in Section 4.1 between 10-year-estimates and the maximum-length follow-up estimates, hold for later cohorts.

Estimates for first-time AE enrollees between 1994 and 2008 are presented in Table 5 (Komvux) and Table 6 (college) and illustrated in Figure 2 (Komvux) and Figure 3 (college). [15] The x-axis states the year of first-time enrollment and each point in the figure represents an estimated earnings return. For each cohort on the x-axis, the reported estimates are based on earnings 10 years after enrollment (solid black line) and earnings in 2018 (dashed black line). The follow-up period of the 2018 estimates varies from 24 years, for enrollees in 1994, to 10 years for those enrolled in 2008. If estimates after 10 years perfectly mimic estimates using longer follow-up periods, the solid black line should overlap with the dashed line representing the 2018 estimates. [16] The Figures labelled (a) and (b) illustrate results in absolute terms for females and males respectively, while the figures labelled (c) and (d) illustrate the corresponding results expressed in percentage terms.

Starting with Komvux and the absolute estimates, the previous section showed that estimates obtained 10 years after enrollment may differ from estimates based on a longer follow-up period. For Komvux, estimates for females and males share several features. First, Figures 2a and 2b show that the three cohorts enrolled in 1994, 1995 and 1996 had a very large increase between the 10-year estimates (solid black line) and the 2018 estimates (dashed black line). For later cohorts, the 2018 estimates are still higher than those after 10 years, but the differences compared with the 10-year estimates are more modest. Second, the corresponding comparison for the estimates in percentage terms also tends to indicate slightly

[15] To ease the exposition, Figures 2 and 3 do not include confidence intervals. For completeness, Figures A1 and A2 in the Appendix illustrate the same results with confidence intervals.

[16] As an additional reference point, the thin gray lines (dashed) represent results for participants in 1994 with the same number of years after the program's start. To clarify with an example, the length of the follow-up period for participants in 2002 is 16 years (until 2018). The dot for the 1994 reference (dashed) lines in 2002 also illustrates the estimate 16 years after enrollment for participants in 1994 (i.e. the estimate in 2010). If the results of the 1994 enrollees exactly mimic the estimates of later cohorts of enrollees, the dashed gray lines should be the same as the dashed black lines of the 2018 estimates.

higher estimates for Komvux in 2018 compared with after 10 years. However, when expressed in percentage terms, the 1994-1996 estimates do not stand out in the same way because the amount of completed AE was also larger in these years.

The increase in percentage estimates is modest. The weighted average increase for cohorts from 1994 to 2007 is for females on average 1.1 percent and for males slightly larger 1.9 percent (males complete less AE).[17] These changes are roughly consistent with or without including the cohorts 1994-1996 (0.9 and 1.7 percent respectively). Thus, the results suggest that the percentage impact slightly increases over time, which is reasonably in line with the findings on Komvux from Section 4.1.[18]

For college, Figures 3a and 3b show that there is a clear gap between the absolute estimates in 2018 (dashed black line) and the 10-year-estimates (solid black line). The findings in Section 4.1, based on the 1994 sample, indicated that absolute estimates were roughly 75 percent higher in 2018. This appears to remain a reasonable benchmark for both female and male cohorts, at least until 2003. [19] Naturally, the two sets of estimates converge as the follow-up period to 2018 becomes shorter, and for the cohort enrolled in 2008 both estimates are based on 2018 earnings. The estimates in percentage terms display less changes, which is expected as the level of the counterfactual earnings also increases for these age groups. However, there is still a small increase between the 10-year estimates and the 2018 estimates when expressed in percentage terms. Applying the same weighted averages as above for Komvux, the percentage increases are 0.8 percent for females and 1.6 percent for males.

[17] The weights applied are proportional to the number of years between the 10-year estimate and 2018, i.e. the greatest weights are given to 1994 (14/105) and 1995 (13/105) and the smallest to 2006 (2/105) and 2007 (1/105).
[18] One may note that the estimates for males, from the late 1990s, are in general very small and hover around zero. The variation in the level of the estimates between cohorts is discussed in Section 6.
[19] For the 1994-2002 cohorts, the weighted average increase between 10-year estimates and 2018 estimates is 59 percent for females and 73 percent for males.

In sum, the results for AE at both Komvux and college indicate relatively clearly that estimates in absolute terms increase when using a longer follow-up period. As a reminder, the higher absolute estimates imply a larger tax base, which is important for any calculation of costs and benefits. The percentage increases, albeit modest, are of similar magnitude for Komvux and college but in both cases are slightly larger for males. Thus, the estimates imply no support for the notion that the investment in AE is followed by human capital depreciation, but weak support for the notion of multiplier effects of AE.

5. Robustness

This section presents robustness tests using different versions of the propensity score matching approach. The estimates presented include the reduced specification discussed in Section 3, which allows for systematic unobservable differences in the year immediately before enrollment. Results are also presented using matched samples that have been trimmed. The underlying motivation of trimming is that, under certain assumptions, bias caused by unobservable characteristics is larger in the tails of the distribution (Black and Smith 2004). Further robustness checks also account for the fact that long-term estimates may be mitigated by that *untreated* individuals complete AE after year *t*. The percentage estimates have so far been calculated as the ratio between the absolute estimate and the completed AE of treated, i.e. without accounting for potential completion of AE among matched comparisons. An alternative is to define completed AE of the treated as the net difference between the average AE completion of the treated individuals and the average AE completion of their matched comparisons. This "net AE" does not account for the exact timing of AE enrollment among matched comparisons but it has the advantage of not violating the conditional independence assumption and leaves the propensity score specification intact to allow the best matches at time *t* to remain among the matched comparisons.

5.1 Results across different specifications

Figures A3 and A4 illustrate results when applying (a) the extended specification, (b) the reduced specification, (c) trimming of matched samples by 10 percent and (d) trimming of matched samples by 30 percent. The results are summarized in Table 7, which reports weighted averages of the difference between 2018 estimates and 10-year estimates from the 1994 to 2007 cohorts. Weighted averages of the bootstrapped standard errors are presented within parentheses. Overall, Table 7 indicates increased percentage estimates regardless of whether the estimates concern AE is at Komvux or college, regardless whether they concern females or males and regardless of the specification chosen. In all but two cases the weighted average of the estimate exceeds the standard errors by a factor of two. This also holds for all cases in the lower half of Table 7, panel B, where the corresponding summary of the percentage estimates is based on "net AE". These cases also always indicate increasing percentage estimates.

In sum, there is a persistent pattern supporting the hypothesis of a modest multiplier effect, with the differences between the estimates in 2018 and 10-year estimates in almost all cases at least as large as the increase reported in the previous section (and repeated as the first line in Table 7). In contrast, there is thus no support for human capital depreciation following AE investments. Both these conjectures strengthen the impressions from Section 4.

5.2 Comparing results with and without controlling for ability rank (GPA)

A novelty of the current data is that there is also information on cohort ability rank based primarily on GPA from the age of 16. [20] The conventional view is that education tends to attract individuals with unobserved abilities that are expected to generate upward bias in estimates based on observable controls. Many studies on AE rely on individual fixed effects

[20] The GPA is the average grade obtained across 10 to 12 school subjects and it is used as a sorting tool between pupils who apply to upper secondary school (see Section 3 for details).

or, at best, earnings trajectories to account for selection. This appears as a reasonable strategy, especially if one also has access to information on other indicators of labor force attachment. With access to information on the ability rank, however, it is interesting to examine to what extent its inclusion among the covariates alters the estimates. Assuming positive selection into AE, it is expected to reduce the estimates, but only to the extent that it captures some ability that earnings and the other covariates fail to capture.

Figure 4 illustrates how estimates differ depending on whether the specification includes the ability rank measure. These figures again include the 10-year-estimates and the maximum length estimates displayed in Figure 2 and Figure 3, but now also include dashed lines with corresponding estimates where ability rank has *not* been part of the specification neither for the estimates of the propensity score, nor as part of the balancing tests.

The findings by and large show the expected pattern for both Komvux and college, regardless of gender. The inclusion of ability rank has a small effect, which in the majority of cases reduces the estimates. The mean difference between the estimates in percentage terms is less than 1 percent for both the 10-year-estimates and the maximum-length-estimates. It is interesting to note that, without access to the GPA variable, we would have documented the same tendency of human capital multiplier effects from AE in Komvux or college but with magnitudes being a fraction (less than half a percent) higher compared with the extended specification estimates in the first row of Table 7.

6. Why do estimates vary between cohorts?

One may note that even if the relation between the 10-year-estimates and the 2018 estimates is reasonably similar between cohorts, the results in Figures 2 and 3 also reveal substantial variation in estimated *magnitudes* between different cohorts. In this section, the variation in estimated levels is discussed in relation to cohort characteristics presented in Figures 5 and 6, which include participants' average ability rank, amount of completed AE and completed

fields of study. Of course, the estimated returns may vary for a number of different reasons related to classroom activities, changes in curricula, specific labor market demand and overall business cycles. It is therefore beyond the scope of this paper to pin down the exact reasons behind these variations. The ambition is rather to provide a tentative discussion about possible explanations for the variation in magnitude of the estimates between cohorts.

6.1 Komvux estimates between cohorts

The estimates 10 years after the program's start provide a useful guidance to assess to what extent magnitudes are generalizable across cohorts. Perfectly stable estimates in Figure 2 and Figure 3 would imply a horizontal straight line (solid black).

For females in Komvux, the absolute 10-year-estimates dropped slightly from 1995 to 1999 and then increased again from 2003. The variation in the estimates is relatively modest as the magnitude mostly hovers around SEK 10,000 (approximately USD 1,200) and in percentage terms around 3 percent for the period 1998-2002 and closer to 5 percent in the other years. With such a modest variation in the results, it is difficult to point out factors that may explain the heterogeneity. None of the characteristics in Figure 5 appear to follow patterns that may explain the estimations, although the initial slump in estimates coincides with decreasing levels in ability rank among the participants (as well as a slight decrease in completed AE), and a drop in the share taking classes in math or science between 1996 and 1998 (Figure 5c). [21] The recovery in estimates that follows after 2002 coincides to some extent with the increased share in healthcare-related subjects, but separate estimates do not support the notion that these were linked with higher estimates.

For males there is also a gradual drop in the 10-year-estimates. For cohorts enrolled up and until 1997, the average estimate is SEK 11,500 and in percentage terms 4.2 percent.

[21] However, ability rank continues to decrease when estimates recover. One may note that lower average ability rank does not necessarily imply lower expected estimates. Stenberg and Westerlund (2008) also report relatively strong positive estimates for long-term unemployed individuals participating in Komvux.

However, the average of the remaining 11 estimates (1998-2008) is close to zero, whether expressed in absolute or percentage terms (see Table 5). This implies that the 1994 estimates in Table 3 provide a poor guide to the impact of Komvux on earnings for future cohorts. The decrease in results between 1996 and 1998 coincides with lower levels of ability rank (Figure 5a), and the fact that the completed AE of participants dropped from about 1 year to an average of about 0.7 years (Figure 5b). There was also a decrease in the share of participants registered in math or science classes (Figure 5c), and an increase in the share of participants registered in some form of vocational course (Figure 5d), among males this is primarily in electronics, vehicle engineering or administration. Additional analyses indicate that vocational classes are linked with negative estimates in most years from 1998 and onwards. [22] The poor estimates in 1998-2008 may thus be partly explained by that larger shares of the treated samples participated in vocational courses and by that participants on average completed less AE.

6.2 College estimates between cohorts

For females in college, the absolute 10-year-estimates are very stable, always between SEK 39,000 and SEK 51,000 (approximately USD 4,600 and 6,000). The percentage estimates drop somewhat from roughly 8.5 percent to around 7.0 percent, which partly reflects that the amount of completed AE increased during the period 1994-2000. In the second half of the period, after 2000, Figures 6c and 6d show that the field of study increasingly involved education related to teaching and health. There has been excess demand for these professions throughout the 2000s in Sweden. It is possible that these fields of study have contributed to the stable returns for females.

[22] Separate 10-year estimates for males in vocational courses are lower than the average 10-year estimates in 14 out of the 15 cases (not displayed), with the mean difference being SEK 7,300.

Males in college are linked to 10-year-estimates which are virtually halved for cohorts from 2000 to 2008 compared with the cohorts from 1994 to 1999 (Figure 3b). The lower absolute estimates coincide with that participants completed less AE (Figure 6b), about one year less in 2008 compared with in 2000. Figure 6e indicates that the share registered in engineering was at its highest, 32 percent, in 1997 and thereafter gradually decreased to only 12 percent in 2004 and the subsequent years. Separate analyses of individuals in engineering indicate 10-year estimates that are higher than the average estimates for 14 out of the 15 cohorts (average SEK 2,100). Together with the lower amount of completed AE, the fields of study may explain some of the decrease in the 10-year estimates. The percentage estimates dropped in a fashion similar to the absolute values, by about 3 percentage points, despite the drop in the amount of AE completed.

7. Conclusion

This paper explores longitudinal data to investigate whether evaluations of AE yield similar estimates if the length of the follow-up period is expanded. Theoretically, the answer is ambiguous. Human capital may either depreciate such that any positive impact on earnings may taper off (Ben-Porath 1967), or multiplier effects may make an initial human capital investment increase future human capital accumulation and increase the payoff (Cunha and Heckman 2007).

Using difference-in-differences propensity score matching, the results indicate evidence of multiplier effects as percentage estimates increase over time for AE participants in Komvux (upper secondary level) as well as in college, for both males and females, as well as across different specifications of the matching procedure. In contrast, no support is found for the hypothesis of human capital depreciation.

Importantly, with or without AE, in the age groups studied it is natural that real earnings levels increase with experience. A constant percentage estimate therefore implies an increase

in estimates expressed in absolute terms. Consequently, absolute estimates based on maximum length of the follow-up period are often substantially higher than the 10-year-estimates, typically more than 50 percent higher, given that the 10-year estimate is significantly different from zero. Such differences may be key for calculations of social costs and benefits. The payoff to AE in absolute labor earnings is of importance to policy makers as it increases the tax base, and also because higher absolute estimates increase the probability that AE covers the costs of providing AE, that is, the costs of teachers and premises as well as foregone earnings during AE enrollment. While all evaluations are to some extent context-specific, the results nevertheless suggest that evaluation based on earnings 10 years after program entry may risk underestimating the impact of AE.

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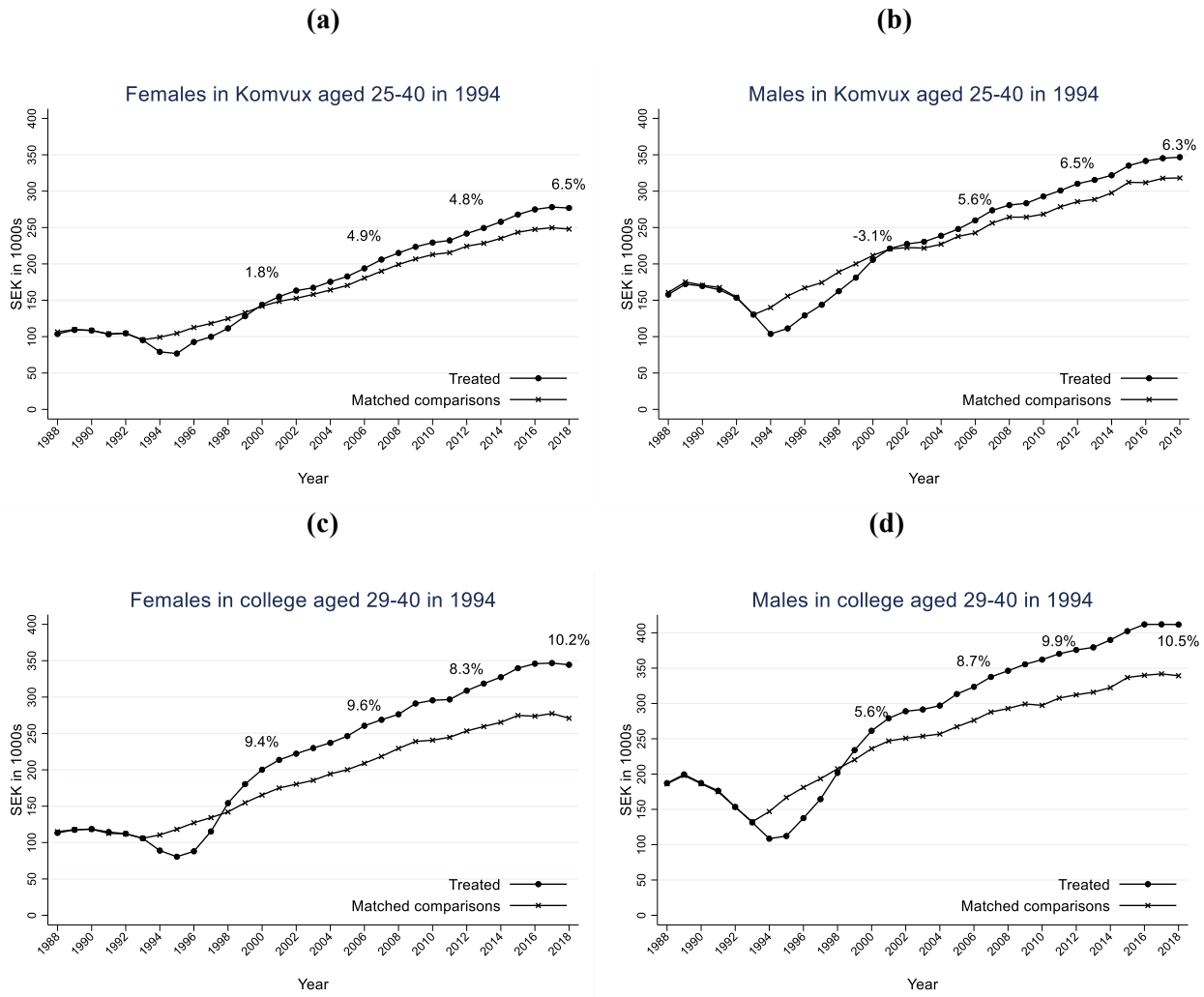
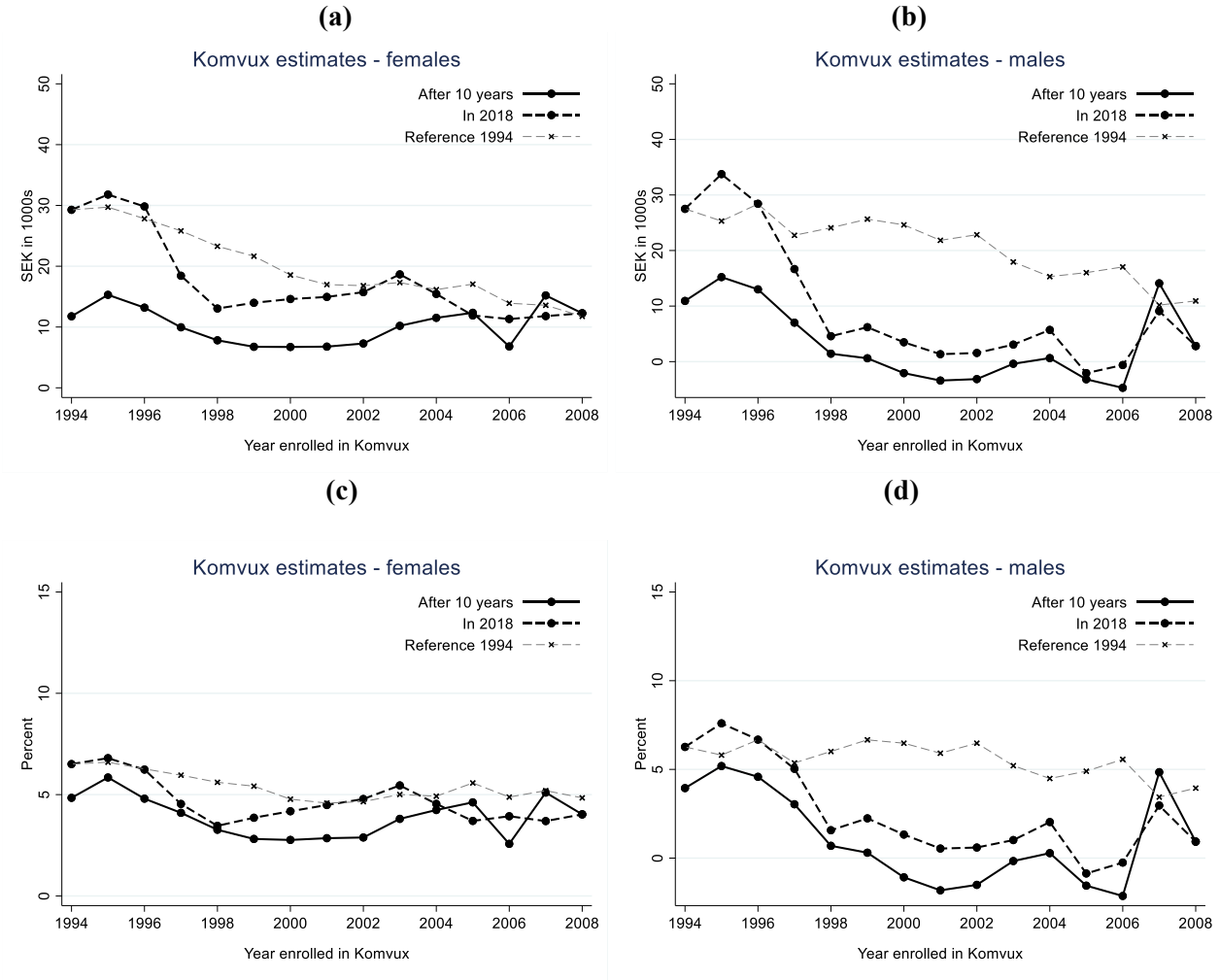
Figure 1. Annual earnings trajectories, treated and matched comparison groups.



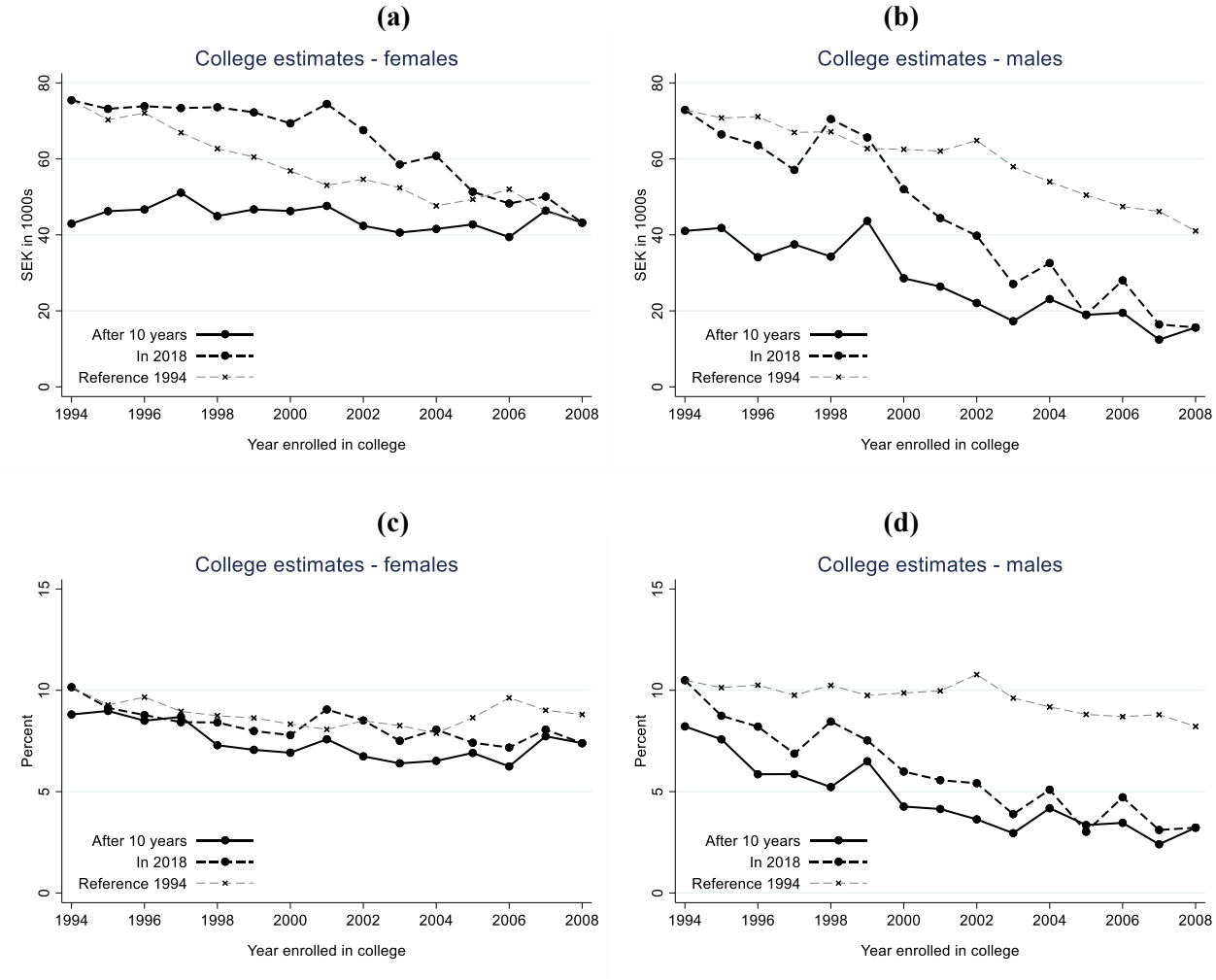
Note: First time enrollees in 1994. SEK in 1000s, 2016 values. The percentages indicated after 6, 12, 18 and 24 years after program entry are obtained by (ATT / earnings) / (Δschooling), i.e. a numerator value representing the estimated average treatment effect on the treated divided by the counterfactual (matched comparison) earnings, and in the denominator the average change in the number of years of completed schooling among participants.

Figure 2. First time Komvux enrollees for the years 1994-2008. Estimated differences in annual earnings 2018 between participants and matched comparison groups.



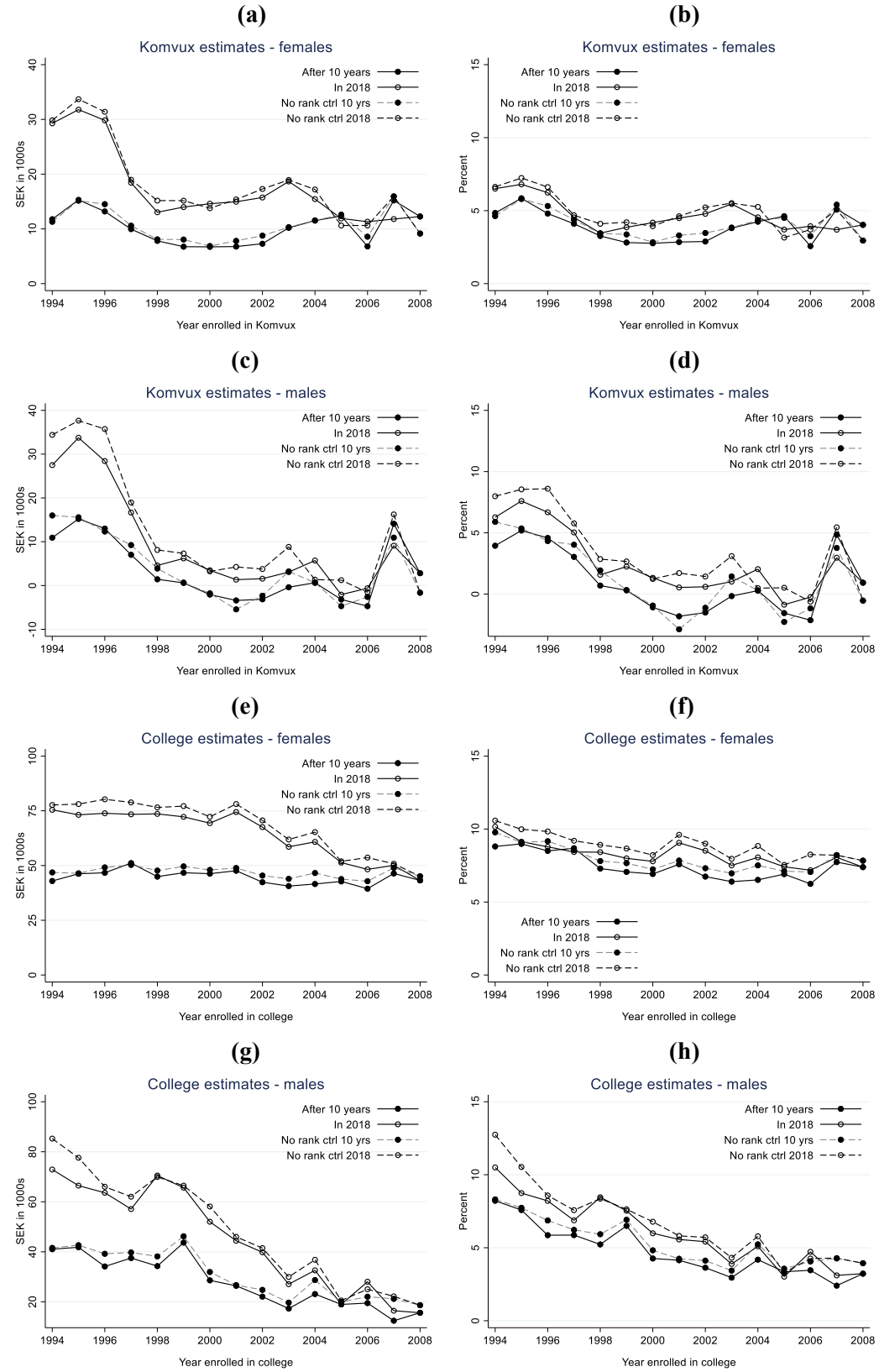
Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE enrollees in that year. The solid black lines represent estimates on annual earnings 10 years after program entry while the black dashed lines are estimates on annual earnings 2018. The dots along the gray dashed lines reflect the results for participants enrolled for the first time in 1994, applying the same length of the follow-up period. For example, the length of the follow-up period for participants 2005 is 13 years (until 2018). The dot on the gray dashed lines the represents the participants in 1994 13 years after enrollment (2007).

Figure 3. First time college enrollees for the years 1994-2008. Estimated differences in annual earnings 2018 between participants and matched comparison groups.



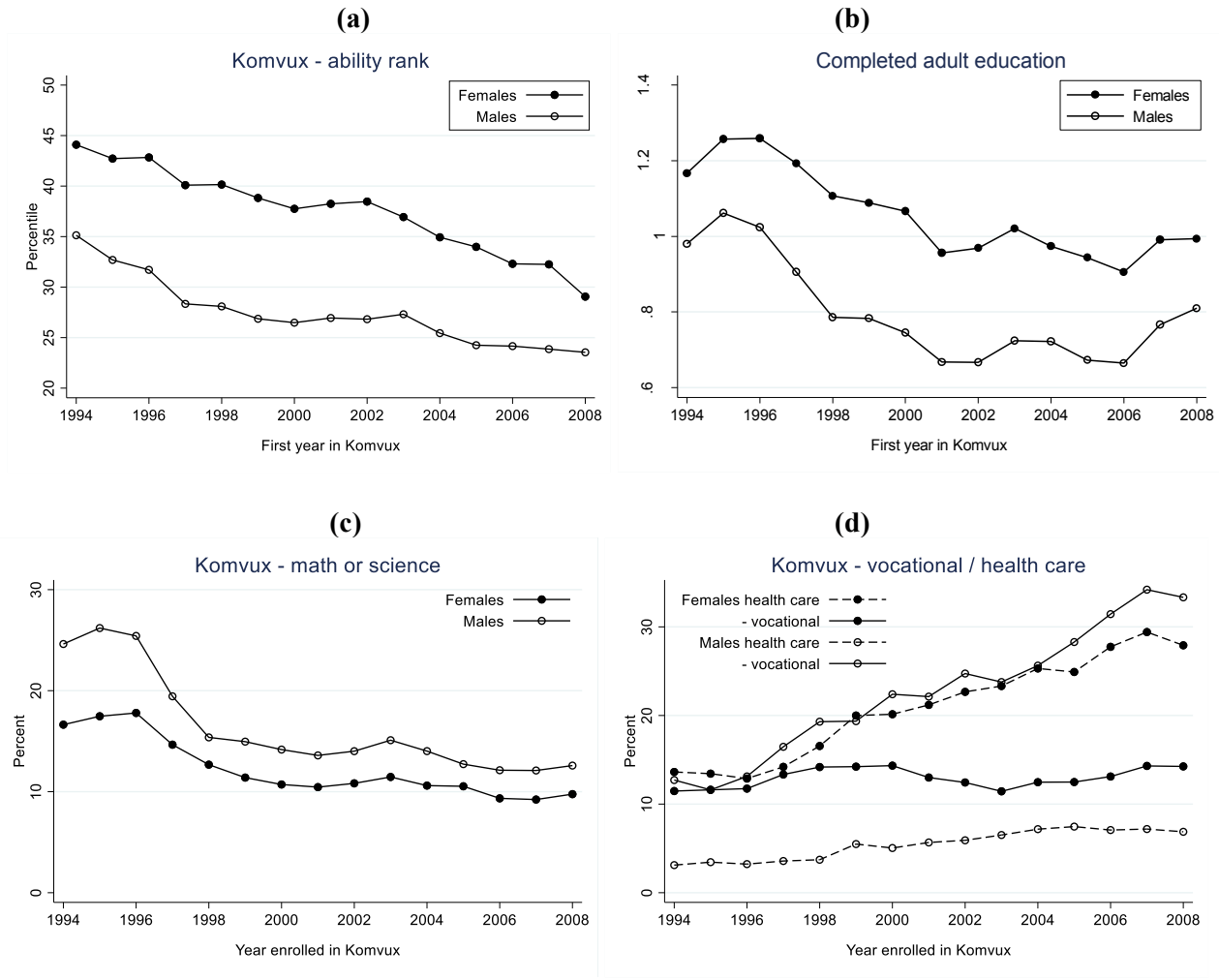
Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE for enrollees in that year. The solid black lines represent estimates on annual earnings 10 years after program entry the black dashed lines are estimates on annual earnings 2018. The dots along the gray dashed lines reflect the results for participants enrolled for the first time in 1994, applying the same length of the follow-up period. For example, the length of the follow-up period for participants 2005 is 13 years (until 2018). The dot on the gray dashed lines the represents the participants in 1994 13 years after enrollment (2007).

Figure 4. Comparing results with specification including or excluding ability rank. First time enrollees for the years 1994-2008. Estimated differences in annual earnings 2018 between participants and matched comparison groups.



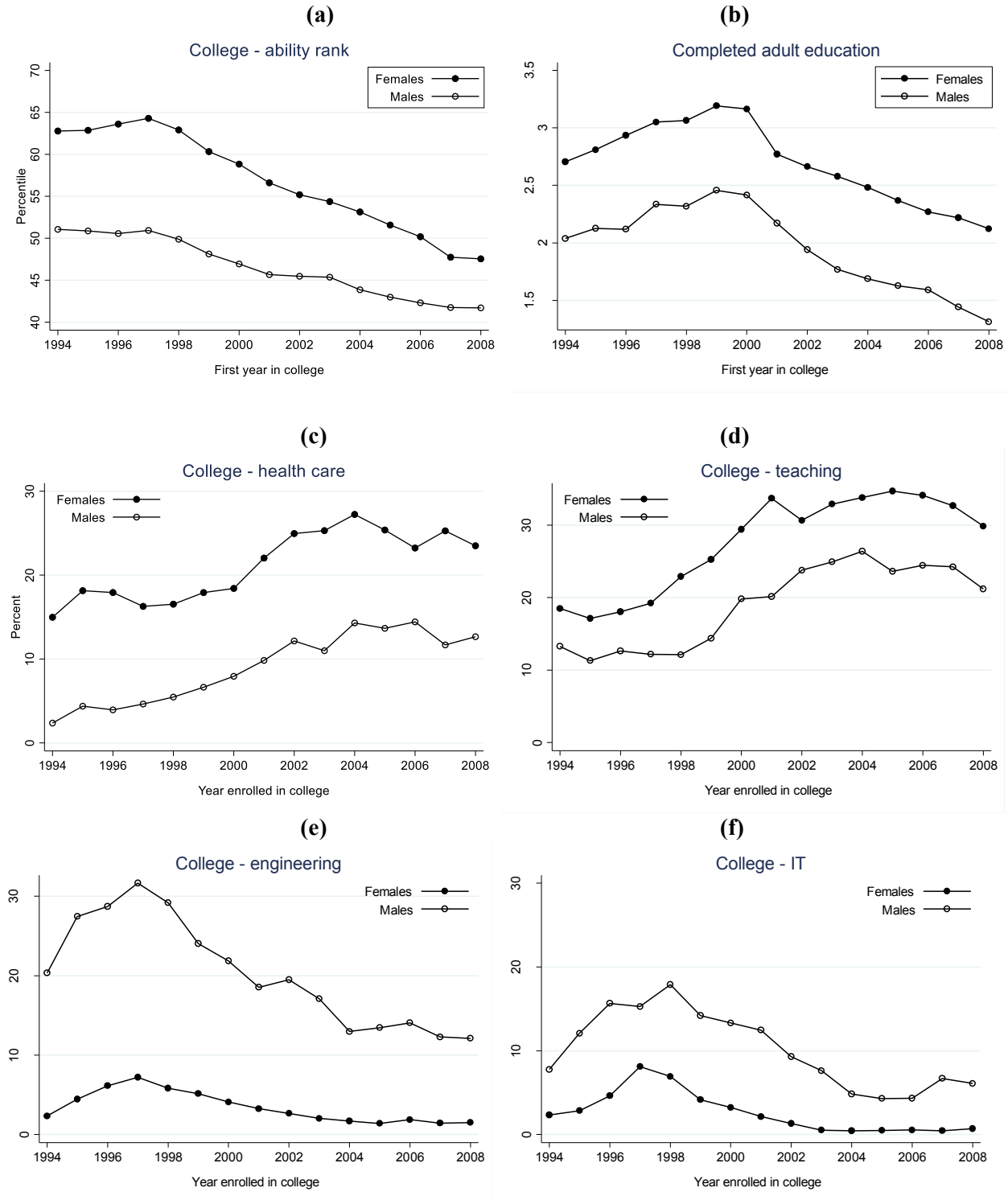
Note: The x-axis states the year of first-time enrollees. Each dot represents the estimated impact of AE enrollees in that year. The solid lines represent extended specification estimates on annual earnings 10 years after program entry (black dots) and on annual earnings 2018 (white dots). The dashed lines represent the corresponding estimates if the specification of the propensity score estimate, and the ensuing balancing texts, does not consider the ability rank measure.

Figure 5. Komvux first time enrollees 1994-2008. Ability rank (a), years of completed AE (b), type of studies (c and d). Note that the scale on the y-axis differs between figures.



Note: The x-axis states the year of first-time enrollees. Each dot represents AE enrollees in that year and their observed average ability rank (figure a), the amount of completed education 10 years after first program entry (figure b) and their share in the respective fields of study (figures c and d).

Figure 6. College first time enrollees 1994-2008. Ability rank (a), years of completed AE (b), type of studies (c, d, e and f). Note that the scale on the y-axis differs between figures.



Note: The x-axis states the year of first-time enrollees. Each dot represents AE enrollees in that year and their observed average ability rank (figure a), the amount of completed education 10 years after first program entry (figure b) and their share in the respective fields of study (figures c and d).

Table 1. Komvux – mean characteristics of participants enrolled for the first time 1994, 1995, 1996 etc until 2008.

Females Komvux – all variables measured year prior to enrollment, t-1, except if stated otherwise.

	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Age	30.84	30.89	30.95	31.18	31.65	31.96	32.45	32.93	33.28	33.48	33.45	33.24	33.30	32.84	32.30
Years of schooling	10.44	10.44	10.47	10.45	10.47	10.43	10.42	10.51	10.50	10.44	10.36	10.27	10.25	10.15	10.04
Ability rank	0.44	0.43	0.43	0.40	0.40	0.39	0.38	0.38	0.38	0.37	0.35	0.34	0.32	0.32	0.29
Rank imputed	0.16	0.12	0.11	0.11	0.10	0.10	0.10	0.09	0.08	0.08	0.08	0.08	0.08	0.07	0.07
Parent years of sch.	10.83	10.90	10.93	10.78	10.78	10.78	10.73	10.82	10.86	10.91	10.94	11.02	11.03	11.12	11.16
Earnings	95.08	96.41	96.53	91.97	109.49	111.74	118.43	127.60	132.91	128.33	124.28	122.71	128.10	131.77	133.69
Employed	0.48	0.49	0.49	0.45	0.53	0.52	0.54	0.56	0.57	0.55	0.52	0.50	0.52	0.50	0.48
Unemployed	0.33	0.36	0.37	0.48	0.40	0.41	0.35	0.29	0.24	0.21	0.25	0.28	0.25	0.22	0.18
Outside the LF	0.28	0.26	0.25	0.19	0.18	0.20	0.23	0.25	0.29	0.33	0.32	0.32	0.30	0.36	0.41
Earnings t-4 – t-2	-3.84	-5.48	-6.00	2.05	12.30	11.74	13.51	14.69	13.50	8.43	2.83	-1.51	1.04	1.99	4.74
Study allowance	0.02	0.02	0.03	0.02	0.01	0.01	0.02	0.02	0.01	0.02	0.02	0.02	0.01	0.02	0.03
Married	0.39	0.38	0.37	0.35	0.35	0.35	0.35	0.35	0.37	0.37	0.34	0.33	0.33	0.32	0.31
Child 0-3 at home	0.38	0.38	0.37	0.38	0.36	0.36	0.36	0.34	0.35	0.34	0.33	0.35	0.34	0.36	0.37
Parental leave	0.31	0.32	0.31	0.31	0.30	0.31	0.31	0.31	0.33	0.32	0.30	0.32	0.32	0.34	0.34
Social welfare	0.16	0.14	0.13	0.15	0.12	0.11	0.09	0.08	0.06	0.08	0.08	0.10	0.10	0.10	0.12
Sick leave	0.29	0.24	0.23	0.23	0.19	0.24	0.27	0.30	0.33	0.35	0.32	0.30	0.28	0.26	0.22
Observations	6635	9031	10263	20092	21507	15871	12892	9710	6652	5201	4271	3786	3369	2004	1722

Males Komvux – all variables measured year prior to enrollment, t-1, except if stated otherwise

	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Age	29.88	30.02	30.19	30.62	31.14	31.63	32.14	32.55	32.82	32.65	32.64	32.48	32.37	31.68	31.38
Years of schooling	10.57	10.55	10.56	10.51	10.50	10.46	10.46	10.51	10.47	10.34	10.23	10.16	10.09	10.05	9.92
Ability rank	0.35	0.33	0.32	0.28	0.28	0.27	0.26	0.27	0.27	0.27	0.25	0.24	0.24	0.24	0.24
Rank missing	0.02	0.02	0.03	0.03	0.03	0.04	0.04	0.03	0.03	0.04	0.06	0.06	0.08	0.07	0.10
Parent years of sch.	11.17	11.13	11.18	11.04	10.98	10.93	10.95	10.98	11.04	11.15	11.32	11.28	11.37	11.58	11.73
Earnings	130.14	134.08	135.16	119.55	145.55	153.73	166.60	183.24	194.09	181.62	168.51	164.93	170.24	187.98	185.84
Employed	0.58	0.59	0.60	0.51	0.60	0.61	0.64	0.68	0.71	0.66	0.60	0.58	0.60	0.64	0.61
Unemployed	0.38	0.38	0.38	0.49	0.39	0.39	0.34	0.26	0.24	0.22	0.26	0.27	0.25	0.18	0.13
Outside the LF	0.18	0.16	0.16	0.14	0.13	0.12	0.14	0.16	0.17	0.22	0.25	0.25	0.26	0.27	0.33
Earnings t-4 – t-2	-16.18	-19.61	-10.62	10.03	19.46	13.57	20.12	22.02	19.00	16.40	0.31	-6.39	-1.31	7.36	12.44
Study allowance	0.02	0.02	0.03	0.03	0.01	0.02	0.01	0.01	0.02	0.02	0.02	0.02	0.03	0.02	0.03
Married	0.21	0.21	0.19	0.18	0.19	0.19	0.20	0.21	0.23	0.21	0.21	0.20	0.20	0.21	0.19
Child 0-3 at home	0.22	0.20	0.19	0.19	0.20	0.20	0.20	0.20	0.21	0.19	0.20	0.19	0.20	0.21	0.19
Parental leave	0.10	0.09	0.09	0.10	0.10	0.12	0.13	0.14	0.16	0.16	0.17	0.17	0.18	0.20	0.18
Social welfare	0.16	0.13	0.14	0.16	0.14	0.12	0.12	0.10	0.08	0.10	0.12	0.14	0.15	0.13	0.14
Sick leave	0.25	0.18	0.18	0.17	0.13	0.16	0.19	0.20	0.22	0.22	0.21	0.18	0.15	0.14	0.13
Observations	4063	5037	5678	10316	12189	9764	8795	6394	4336	3143	2904	2510	1990	1325	1182

Table 2. College – mean characteristics of participants enrolled for the first time 1994, 1995, 1996 etc until 2008.

Females college – all variables measured year prior to enrollment, t-1, except if stated otherwise.

	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Age	33.52	33.30	33.25	33.29	33.45	33.68	33.87	33.98	34.02	34.11	34.09	34.20	34.25	34.22	34.18
Years of schooling	11.48	11.46	11.49	11.48	11.46	11.41	11.40	11.63	11.63	11.67	11.70	11.72	11.74	11.76	11.81
Ability rank	0.63	0.63	0.64	0.64	0.63	0.60	0.59	0.57	0.55	0.54	0.53	0.52	0.50	0.48	0.48
Rank missing	0.15	0.08	0.07	0.07	0.06	0.05	0.05	0.05	0.04	0.05	0.03	0.04	0.04	0.04	0.03
Parent years of sch.	11.25	11.36	11.42	11.53	11.54	11.51	11.49	11.49	11.57	11.57	11.71	11.75	11.78	11.70	11.77
Earnings	105.37	103.26	98.20	100.29	100.95	96.61	102.50	111.06	124.04	129.84	130.46	134.45	137.99	146.29	158.26
Employed	0.50	0.50	0.46	0.45	0.43	0.38	0.40	0.43	0.47	0.49	0.49	0.51	0.51	0.54	0.54
Unemployed	0.33	0.36	0.37	0.41	0.41	0.41	0.39	0.37	0.31	0.27	0.29	0.28	0.29	0.27	0.17
Outside the LF	0.26	0.23	0.24	0.22	0.23	0.27	0.29	0.29	0.30	0.31	0.31	0.30	0.29	0.28	0.35
Earnings t-4 – t-2	-6.32	-5.38	-9.89	-0.55	3.14	2.21	-5.43	0.86	8.65	4.69	3.36	-2.97	0.77	3.49	8.50
Study allowance	0.12	0.08	0.16	0.18	0.15	0.10	0.11	0.10	0.15	0.22	0.18	0.16	0.15	0.15	0.17
Married	0.47	0.46	0.47	0.44	0.43	0.42	0.42	0.41	0.40	0.40	0.39	0.38	0.39	0.39	0.39
Child 0-3 at home	0.30	0.33	0.33	0.32	0.29	0.26	0.28	0.29	0.31	0.32	0.35	0.35	0.37	0.38	0.39
Parental leave	0.26	0.29	0.28	0.27	0.23	0.22	0.23	0.27	0.29	0.31	0.33	0.36	0.36	0.38	0.41
Social welfare	0.08	0.07	0.07	0.07	0.06	0.05	0.04	0.03	0.02	0.03	0.03	0.03	0.03	0.02	0.02
Sick leave	0.21	0.17	0.16	0.12	0.12	0.12	0.15	0.18	0.22	0.23	0.25	0.23	0.21	0.23	0.20
Observations	2757	3341	3632	4068	4313	5034	5350	6107	6537	5921	5135	4566	4001	3872	3693

Males college – all variables measured year prior to enrollment, t-1, except if stated otherwise.

	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Age	32.88	32.78	32.62	32.66	32.83	32.95	33.00	33.29	33.37	33.43	33.51	33.62	33.54	33.73	33.61
Years of schooling	11.65	11.68	11.67	11.63	11.62	11.53	11.54	11.72	11.75	11.77	11.78	11.76	11.78	11.81	11.85
Ability rank	0.51	0.51	0.51	0.51	0.50	0.48	0.47	0.46	0.45	0.45	0.44	0.43	0.42	0.42	0.42
Rank missing	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.03	0.02
Parent years of sch.	11.39	11.51	11.55	11.56	11.74	11.71	11.70	11.76	11.81	11.98	12.03	12.16	12.23	12.11	12.18
Earnings	131.32	142.46	149.94	147.41	160.48	153.26	165.35	179.73	198.56	203.42	202.02	201.98	217.20	241.05	258.37
Employed	0.54	0.58	0.59	0.57	0.60	0.55	0.59	0.62	0.65	0.65	0.66	0.65	0.69	0.74	0.77
Unemployed	0.39	0.37	0.38	0.38	0.34	0.35	0.33	0.30	0.25	0.24	0.27	0.26	0.24	0.19	0.13
Outside the LF	0.18	0.14	0.13	0.14	0.13	0.16	0.17	0.16	0.18	0.19	0.18	0.19	0.17	0.16	0.18
Earnings t-4 – t-2	-33.83	-31.38	-18.84	-1.85	4.05	-1.58	-3.15	2.44	14.09	14.17	-6.86	-9.21	2.39	5.35	19.72
Study allowance	0.13	0.09	0.16	0.18	0.16	0.12	0.11	0.11	0.15	0.19	0.16	0.17	0.14	0.12	0.12
Married	0.34	0.33	0.29	0.30	0.28	0.26	0.25	0.25	0.26	0.25	0.24	0.25	0.25	0.26	0.27
Child 0-3 at home	0.29	0.29	0.26	0.25	0.24	0.20	0.22	0.22	0.21	0.21	0.21	0.22	0.23	0.25	0.26
Parental leave	0.13	0.11	0.12	0.13	0.11	0.10	0.14	0.14	0.14	0.16	0.17	0.17	0.20	0.21	0.25
Social welfare	0.07	0.06	0.06	0.06	0.05	0.05	0.03	0.03	0.03	0.03	0.03	0.04	0.03	0.03	0.02
Sick leave	0.21	0.14	0.10	0.09	0.07	0.07	0.10	0.11	0.12	0.13	0.12	0.10	0.11	0.08	0.09
Observations	2108	2490	2556	2577	2544	2664	2508	2603	2904	2702	2463	2027	1670	1554	1471

Table 3. Komvux - difference-in-difference propensity score matching estimates.

	Females					Males				
	N	Reduced		Extended	% [a)]	N treated	Reduced		Extended	% [a)]
1995	6,604	-30,333		-26,369		4,041	-49,823		-45,896	
		(1,020)		(1,016)			(1,753)		(1,740)	
1996	6,595	-22,165		-19,439		4,033	-41,068		-38,290	
		(1,230)		(1,187)			(1,944)		(1,990)	
1997	6,581	-19,657		-17,699		4,021	-34,892		-31,448	
		(1,257)		(1,246)			(1,942)		(2,158)	
1998	6,565	-14,503		-12,049		4,003	-28,913		-26,642	
		(1,276)		(1,367)			(2,105)		(2,260)	
1999	6,548	-5,482		-4,074		3,995	-19,916		-19,877	
		(1,392)		(1,447)			(2,137)		(2,333)	
2000	6,547	1,635	1.0	2,957	1.8	3,985	-5,705	-2.6	-6,585	-3.1
		(1,492)		(1,554)			(2,300)		(2,479)	
2001	6,545	5,873	3.1	7,856	4.2	3,980	1,603	0.7	1,687	0.7
		(1,561)		(1,645)			(2,359)		(2,420)	
2002	6,545	7,812	3.7	11,583	5.7	3,976	4,227	1.6	5,324	2.1
		(1,695)		(1,644)			(2,405)		(2,549)	
2003	6,532	8,514	3.8	10,504	4.7	3,968	5,190	1.9	8,240	3.1
		(1,590)		(1,735)			(2,501)		(2,561)	
2004	6,526	11,423	4.7	11,765	4.8	3,959	9,260	3.3	10,925	3.9
		(1,659)		(1,661)			(2,626)		(2,671)	
2005	6,517	13,921	5.3	13,582	5.2	3,947	9,944	3.3	10,179	3.4
		(1,712)		(1,829)			(2,640)		(2,724)	
2006	6,494	14,252	5.0	13,910	4.9	3,932	12,813	4.1	17,040	5.6
		(1,839)		(1,784)			(2,643)		(2,812)	
2007	6,481	15,043	4.9	17,058	5.6	3,917	13,756	4.1	16,023	4.9
		(1,837)		(1,858)			(2,823)		(2,916)	
2008	6,474	17,351	5.3	16,150	4.9	3,911	15,782	4.6	15,306	4.5
		(1,791)		(1,890)			(3,041)		(2,998)	
2009	6,470	15,486	4.5	17,321	5.0	3,903	14,742	4.2	17,945	5.2
		(1,877)		(1,868)			(3,107)		(3,112)	
2010	6,457	16,742	4.6	16,803	4.7	3,888	22,607	6.3	22,843	6.5
		(1,890)		(2,006)			(3,084)		(3,055)	
2011	6,452	17,516	4.7	16,983	4.6	3,871	24,476	6.6	21,840	5.9
		(2,009)		(1,989)			(3,334)		(3,111)	
2012	6,445	16,567	4.2	18,565	4.8	3,864	24,385	6.4	24,618	6.5
		(2,082)		(2,077)			(3,338)		(3,305)	
2013	6,435	20,519	5.1	21,656	5.4	3,864	23,418	6.0	25,651	6.7
		(2,041)		(2,097)			(3,377)		(3,417)	
2014	6,415	23,416	5.6	23,284	5.6	3,854	20,307	5.0	24,091	6.0
		(2,191)		(2,191)			(3,550)		(3,442)	
2015	6,402	25,509	5.9	25,823	6.0	3,842	25,951	6.2	22,741	5.4
		(2,317)		(2,158)			(3,801)		(3,719)	
2016	6,381	28,342	6.4	27,825	6.3	3,828	24,361	5.6	28,371	6.7
		(2,279)		(2,369)			(3,792)		(3,724)	
2017	6,366	30,755	6.9	29,719	6.6	3,819	30,065	6.9	25,310	5.8
		(2,466)		(2,395)			(3,694)		(3,924)	
2018	6,358	28,111	6.2	29,281	6.5	3,797	26,332	6.0	27,501	6.3
		(2,371)		(2,499)			(4,103)		(3,724)	

Note: Bootstrapped standard errors in parentheses (1000 replications). Earnings in 2016 values.
[a)] Percent in each year is given by (ATT / earnings) / (Δschooling).

Table 4. College - difference-in-difference propensity score matching estimates.

	Females					Males				
	N treated	Reduced		Extended	% [a]	N treated	Reduced		Extended	% [a]
1995	2,732	-42,876		-36,870		2,090	-60,800		-54,307	
		(1,732)		(1,756)			(2,497)		(2,401)	
1996	2,727	-44,372		-39,483		2,086	-48,781		-42,863	
		(1,988)		(1,951)			(2,762)		(2,688)	
1997	2,722	-25,457		-18,884		2,079	-32,132		-27,810	
		(2,114)		(2,128)			(3,079)		(2,991)	
1998	2,716	7,491	3.0	12,550	5.1	2,076	-9,257		-6,273	
		(2,319)		(2,306)			(3,347)		(3,344)	
1999	2,703	24,021	8.1	25,703	8.8	2,074	8,368	2.7	13,780	4.6
		(2,360)		(2,419)			(3,380)		(3,445)	
2000	2,703	30,814	7.8	36,028	9.4	2,072	23,257	5.3	24,524	5.6
		(2,493)		(2,586)			(3,774)		(3,910)	
2001	2,696	37,937	9.1	40,441	9.7	2,068	29,687	6.3	32,582	7.0
		(2,683)		(2,787)			(3,710)		(3,653)	
2002	2,702	39,574	8.9	42,948	9.8	2,061	36,867	7.7	40,661	8.5
		(2,858)		(2,798)			(3,826)		(3,951)	
2003	2,699	42,202	9.1	44,677	9.7	2,059	39,067	8.0	39,406	8.1
		(2,796)		(2,856)			(3,782)		(3,912)	
2004	2,697	44,003	9.0	42,947	8.8	2,054	36,253	7.1	41,037	8.2
		(2,996)		(2,976)			(3,996)		(3,997)	
2005	2,690	42,265	8.1	46,293	9.0	2,055	40,996	7.7	46,162	8.8
		(3,223)		(3,168)			(6,108)		(6,342)	
2006	2,676	48,216	8.8	52,020	9.6	2,053	44,623	8.1	47,457	8.7
		(3,229)		(3,212)			(4,183)		(4,435)	
2007	2,672	46,108	7.9	49,341	8.6	2,048	42,414	7.2	50,480	8.8
		(3,192)		(3,092)			(4,465)		(4,659)	
2008	2,670	47,982	8.0	47,653	7.9	2,043	50,466	8.4	53,970	9.2
		(3,222)		(3,192)			(4,494)		(4,755)	
2009	2,669	53,603	8.5	52,412	8.3	2,033	59,707	10.0	57,946	9.6
		(3,427)		(3,397)			(4,410)		(4,939)	
2010	2,669	51,151	7.8	54,630	8.5	2,032	58,262	9.5	64,845	10.8
		(3,254)		(3,287)			(5,145)		(5,118)	
2011	2,663	53,574	8.2	53,049	8.1	2,029	63,969	10.3	62,056	10.0
		(3,271)		(3,456)			(6,134)		(6,186)	
2012	2,662	53,338	7.7	56,845	8.3	2,030	59,294	9.2	62,527	9.9
		(3,595)		(3,531)			(5,230)		(5,315)	
2013	2,660	56,875	8.1	60,513	8.6	2,022	56,983	8.6	62,727	9.8
		(3,592)		(3,701)			(4,968)		(5,186)	
2014	2,652	60,181	8.3	62,706	8.7	2,014	61,609	9.2	67,144	10.2
		(3,824)		(3,767)			(5,065)		(5,303)	
2015	2,644	63,849	8.5	66,917	9.0	2,007	65,286	9.5	66,978	9.8
		(3,816)		(3,996)			(5,720)		(5,732)	
2016	2,638	67,733	8.9	72,080	9.7	2,006	72,272	10.4	71,099	10.3
		(4,147)		(4,196)			(5,743)		(5,872)	
2017	2,633	66,344	8.7	70,293	9.3	2,001	67,948	9.7	70,788	10.1
		(4,171)		(4,021)			(5,598)		(5,891)	
2018	2,628	69,675	9.2	75,444	10.2	1,996	73,284	10.6	72,893	10.5
		(4,164)		(4,450)			(5,814)		(5,942)	

Note: Bootstrapped standard errors in parentheses (1000 replications). Earnings in 2016 values.
[a] Percent in each year is given by (ATT / earnings) / (Δschooling).

Table 5. Komvux - difference-in-difference propensity score matching – 10-year-estimates and estimates 2018 for cohorts enrolled for the first time in each year from 1994 to 2008.

	Females					Males				
	N treated	10-year		2018	%[a)]	N treated	10-year		2018	%[a)]
1994	6,526	11,765	4.8	29,281	6.5	3,959	10,925	3.9	27,501	6.3
		(1,699)		(2,364)			(2,579)		(3,802)	
1995	8,802	15,297	5.8	31,788	6.8	4,887	15,214	5.2	33,753	7.6
		(1,532)		(1,986)			(2,282)		(3,309)	
1996	9,957	13,190	4.8	29,834	6.2	5,475	13,007	4.6	28,420	6.7
		(1,390)		(1,866)			(2,165)		(3,343)	
1997	19,637	9,958	4.1	18,437	4.5	9,971	7,012	3.0	16,664	5.0
		(977)		(1,218)			(1,609)		(2,125)	
1998	20,938	7,819	3.3	13,043	3.5	11,742	1,425	0.7	4,579	1.6
		(909)		(1,199)			(1,490)		(1,914)	
1999	15,417	6,751	2.8	13,984	3.9	9,373	603	0.3	6,195	2.2
		(1,047)		(1,379)			(1,682)		(2,143)	
2000	12,437	6,718	2.8	14,604	4.2	8,422	-2,069	-1.1	3,484	1.3
		(1,151)		(1,497)			(1,707)		(2,168)	
2001	9,331	6,775	2.9	14,953	4.5	6,111	-3,404	-1.8	1,343	0.5
		(1,364)		(1,791)			(2,010)		(2,380)	
2002	6,379	7,289	2.9	15,734	4.8	3,966	-3,143	-1.5	1,559	0.6
		(1,712)		(2,109)			(2,601)		(3,111)	
2003	5,058	10,198	3.8	18,663	5.5	2,943	-389	-0.2	3,065	1.0
		(1,883)		(2,408)			(3,211)		(3,659)	
2004	4,024	11,510	4.2	15,444	4.5	2,700	639	0.3	5,708	2.0
		(2,345)		(2,643)			(3,535)		(3,872)	
2005	3,704	12,342	4.6	11,899	3.7	2,390	-3,183	-1.6	-2,073	-0.9
		(2,428)		(2,613)			(3,685)		(4,026)	
2006	3,293	6,807	2.6	11,319	3.9	1,910	-4,714	-2.1	-615	-0.3
		(2,733)		(2,850)			(4,022)		(4,323)	
2007	1,961	15,202	5.1	11,797	3.7	1,272	14,100	4.8	9,094	3.0
		(3,511)		(3,612)			(4,821)		(5,299)	
2008	1,668	12,261	4.0	12,261	4.0	1,131	2,814	0.9	2,814	0.9
		(3,826)		(3,795)			(5,620)		(5,462)	

Bootstrapped standard errors in parentheses (1000 replications).

[a)] Percent in each year is given by (ATT / earnings) / (Δschooling).

Table 6. College - difference-in-difference propensity score matching – 10-year-estimates and estimates 2018 for cohorts enrolled for the first time in each year from 1994 to 2008.

	Females					Males				
	N treated	**10-year**		**2018**	**%** [a)]	**N treated**	**10-year**		**2018**	**%** [a)]
1994	2,696	42,947	8.8	75,444	10.2	2,054	41,037	8.2	72,893	10.5
		(2,983)		(4,360)			(3,797)		(5,764)	
1995	3,247	46,233	9.0	73,170	9.1	2,423	41,813	7.6	66,453	8.7
		(2,588)		(3,832)			(3,612)		(5,542)	
1996	3,541	46,692	8.5	73,865	8.8	2,499	34,129	5.9	63,612	8.2
		(2,612)		(3,457)			(3,810)		(5,428)	
1997	3,960	51,106	8.7	73,385	8.4	2,504	37,499	5.9	57,096	6.9
		(2,557)		(3,326)			(3,628)		(4,752)	
1998	4,201	44,937	7.3	73,593	8.4	2,484	34,283	5.2	70,487	8.5
		(2,393)		(3,100)			(3,681)		(5,060)	
1999	4,904	46,713	7.1	72,249	8.0	2,581	43,666	6.5	65,648	7.5
		(2,144)		(2,735)			(3,626)		(4,749)	
2000	5,204	46,275	6.9	69,363	7.8	2,444	28,573	4.3	52,016	6.0
		(2,077)		(2,705)			(3,816)		(4,450)	
2001	5,945	47,627	7.6	74,453	9.1	2,524	26,391	4.1	44,413	5.6
		(1,949)		(2,408)			(3,612)		(4,792)	
2002	6,396	42,389	6.7	67,563	8.5	2,827	22,066	3.6	39,794	5.4
		(2,037)		(2,375)			(3,903)		(4,522)	
2003	5,728	40,619	6.4	58,549	7.5	2,594	17,276	3.0	27,060	3.9
		(2,196)		(2,465)			(4,158)		(4,462)	
2004	4,947	41,566	6.5	60,793	8.1	2,380	23,100	4.2	32,575	5.1
		(2,280)		(2,628)			(4,452)		(5,972)	
2005	4,455	42,748	6.9	51,338	7.4	1,969	18,961	3.4	18,984	3.0
		(2,648)		(2,709)			(4,695)		(4,888)	
2006	3,816	39,426	6.2	48,259	7.2	1,625	19,488	3.5	28,035	4.7
		(2,721)		(3,070)			(5,052)		(5,710)	
2007	3,771	46,385	7.7	50,077	8.1	1,471	12,432	2.4	16,436	3.1
		(2,917)		(2,973)			(6,056)		(5,915)	
2008	3,522	43,201	7.4	43,201	7.4	1,392	15,626	3.2	15,626	3.2
		(3,093)		(2,960)			(5,903)		(5,690)	

Bootstrapped standard errors in parentheses (1000 replications).

[a)] Percent in each year is given by (ATT / earnings) / (Δschooling).

Table 7. Summary statistics, weighted means, comparing 2018 estimates with 10-year estimates. Weighted means of bootstrapped standard errors obtained for the 2018 estimates in parentheses.

	Komvux		College	
	Females	Males	Females	Males
Panel A.				
Extended specification	1.06	1.93	0.80	1.65
	(0.48)	(0.90)	(0.40)	(0.68)
Reduced specification	1.35	2.75	0.76	2.07
	(0.47)	(0.90)	(0.39)	(0.67)
Trim 10%	1.36	2.38	0.78	1.90
	(0.51)	(0.96)	(0.43)	(0.79)
Trim 30%	1.39	3.35	1.47	1.96
	(0.61)	(1.09)	(0.53)	(0.96)
Panel B.				
Extended spec. net AE	2.42	2.69	1.58	2.16
	(0.78)	(1.18)	(0.51)	(0.80)
Reduced spec. net AE	2.83	3.66	1.48	2.60
	(0.78)	(1.18)	(0.51)	(0.80)
Trim 10% net AE	2.78	3.11	1.54	2.37
	(0.82)	(1.20)	(0.56)	(0.91)
Trim 30% net AE	2.72	4.10	2.27	2.29
	(0.97)	(1.31)	(0.66)	(1.06)

Note: For different specifications, the weighted means represent the difference between estimates obtained in 2018 and the 10-year estimates. The weights applied are proportional to the number of years between the 10-year estimate and 2018, i.e. the greatest weights are given to 1994 (14/105) and 1995 (13/105) and the smallest to 2007 (1/105). Standard errors in parentheses are similarly weighted averages of the bootstrapped standard errors obtained for the 2018 estimates.

APPENDIX FIGURES AND TABLES

"A Note on Evaluating Formal Education for Adults"

by Anders Stenberg

Table A1. Probit regression results on Komvux enrollment to estimate propensity score.

	Females		Males	
	Reduced	Extended	Reduced	Extended
Sch 11 years	.048***	.050***	.069***	.065***
	(.014)	(.014)	(.015)	(.015)
Parent years of sch.	.034***	.034***	.043***	.044***
	(.003)	(.003)	(.003)	(.003)
Earnings 1993				-.059***
				(.011)
Earnings 1991			-.082***	
			(.010)	
Earnings diff 1990-1992	.052***	.042***		
	(.010)	(.009)		
1 child	-.107***	-.109***	-.096***	-.098***
	(.014)	(.014)	(.017)	(.017)
2 children			-.095***	-.096***
			(.018)	(.018)
3 children	.121***	.119***		
	(.015)	(.015)		
Child 0-3	-.132***	-.149***	-.100***	-.102***
	(.021)	(.021)	(.018)	(.019)
Married		.010		
		(.013)		
Divorced	.109***	.106***		
	(.022)	(.023)		
Parental leave 1993	-.090***	-.086***	.061**	.074***
	(.017)	(.018)	(.024)	(.024)
Parental leave 1992	.062***	.069***		
	(.019)	(.019)		
Parental leave 1991			.071***	.063**
			(.026)	(.027)
Amount parental 1992	.010***	.006**	.022***	.015*
	(.003)	(.003)	(.008)	(.008)
Amount parental 1991	.003	.005*	.013	.017
	(.003)	(.003)	(.010)	(.010)
Amount parental 1990				.025**
				(.011)
Study allowance t-1	.471***	.453***	.293***	.265***
	(.066)	(.067)	(.097)	(.097)
Early retirement 1993	-.546***	-.522***	-.269***	-.255***
	(.053)	(.053)	(.064)	(.065)
Social welfare 1993	.208***	.194***	.196***	.173***
	(.018)	(.018)	(.021)	(.021)
Social welfare 1991	.059***	.067***		
	(.020)	(.020)		
Social welfare 1990			-.021	-.017
			(.024)	(.024)
Sick leave 1992	.053***	.045***	.068***	.055***
	(.012)	(.012)	(.014)	(.014)
Sick leave 1991	.029**		.060***	.056***
	(.014)		(.016)	(.017)
Sick leave 1990			.074***	.078***
			(.017)	(.018)
Amount sick leave 1993	.006*	.007*	.029***	.026***
	(.004)	(.004)	(.003)	(.003)
Amount sick leave 1990			-.000***	-.000***
			(.000)	(.000)
UI benefits 1993		.340***		.275***
		(.017)		(.022)
UI benefits 1992	.161***		.077***	-.141***
	(.016)		(.017)	(.022)
UI benefits 1990			.120***	.110***
			(.033)	(.033)
Amount UI 1991	-.006	-.010**	-.014***	-.006

	(.005)	(.005)	(.004)	(.004)
Amount UI 1990			-.025***	-.027***
			(.009)	(.010)
Zero earnings 1992			-.098***	-.084***
			(.026)	(.025)
Zero earnings 1991			-.164***	-.119***
			(.031)	(.031)
Zero earnings 1990	-.104***	-.094***		
	(.023)	(.022)		
Employed 1993		-.073***		
		(.015)		
Employed 1990				-.055***
				(.020)
Outside LF 1993				.065**
				(.031)
Farm & mining sector	-.183***	-.158***	-.372***	-.302***
	(.054)	(.054)	(.047)	(.046)
Construction sector			-.154***	-.109***
			(.025)	(.023)
Manufacturing sector	-.239***	-.212***	-.217***	-.116***
	(.024)	(.024)	(.023)	(.021)
Finance sector	-.138***	-.116***	-.102***	
	(.026)	(.026)	(.030)	
Public sector	-.097***	-.064***	.071***	.152***
	(.017)	(.017)	(.025)	(.023)
Other sector	-.130***	-.102***	-.149***	-.053***
	(.018)	(.019)	(.022)	(.019)
Unemp-unemp		-.097***		
		(.023)		
Unemp-OLF		.179***		.151**
		(.041)		(.066)
Unemp-emp		-.051*		
		(.028)		
OLF-emp				.134***
				(.043)
Imputed rank	-.123***	-.124***	-.185***	-.183***
	(.017)	(.017)	(.040)	(.040)
Ability rank	.273***	.289***	.585***	.601***
	(.024)	(.024)	(.028)	(.028)
N	299,988	299,988	424,483	424,483

Note: Standard errors within parentheses. * p<0.10, ** p<0.05, *** p<0.01. Specifications consider that propensity score estimates based on irrelevant variables may generate bias or increase variance of matching estimators (de Luna et al. 2011). Explanatory variables therefore are only included if p-values are below .2 or if they are required to achieve balanced samples. All specifications include a constant term, age-dummies and dummies for regional residence. Earnings are expressed in SEK 100,000. Outside the labor force (OLF). is defined as annual earnings below SEK 20,000 (approximately €2,200) and no transfers related to unemployment insurance.

Table A2. Probit regression results on college enrollment to estimate propensity score.

	Females		Males	
	Reduced	Extended	Reduced	Extended
Sch 11 years	-.385***	-.397***	-.291***	-.301***
	(.036)	(.036)	(.026)	(.026)
Sch 12 years	-.217***	-.224***	-.112***	-.107***
	(.036)	(.036)	(.031)	(.031)
Parent years of sch.	.042***	.043***	.032***	.032***
	(.004)	(.004)	(.004)	(.004)
Earnings 1992	-.056***		-.151***	-.156***
	(.021)		(.013)	(.023)
Earnings 1991				.001
				(.020)
Earnings diff 1990-1992		-.051***		
		(.013)		
Earnings diff 1993-1992				-.058***
				(.020)
1 child	-.082***	-.083***	-.071***	-.073***
	(.022)	(.022)	(.024)	(.024)
2 children	-.062***	-.065***	-.071***	-.066***
	(.018)	(.018)	(.023)	(.023)
Child 0-3	-.154***	-.205***	-.081***	-.078***
	(.024)	(.029)	(.023)	(.024)
Married			.033	.038*
			(.020)	(.020)
Divorced	.110***	.098***		
	(.030)	(.030)		
Parental leave 1993	-.083***	-.088***	.078***	.087***
	(.023)	(.027)	(.029)	(.030)
Parental leave 1992		.049*		-.066**
		(.028)		(.032)
Parental leave 1991		.051*		
		(.027)		
Parental leave 1990	.124***	.090***		
	(.021)	(.024)		
Amount parental 1993		-.008		
		(.005)		
Amount parental 1991			.025**	.023**
			(.011)	(.011)
Amount parental 1990			.047***	.048***
			(.012)	(.012)
Study allow. 1993	.703***	.650***	.977***	.924***
	(.061)	(.064)	(.079)	(.080)
Early retirement 1993	-.394***	-.387***	-.351***	-.277**
	(.083)	(.084)	(.109)	(.109)
Social welfare 1993	.003			-.090**
	(.036)			(.042)
Social welfare 1992	-.070*	-.094**	-.149***	-.093**
	(.039)	(.039)	(.041)	(.045)
Social welfare 1991		.050		
		(.038)		
Social welfare 1990			-.183***	-.158***
			(.046)	(.047)
Sick leave 1993	-.021	-.054***	.015	-.037
	(.019)	(.020)	(.026)	(.026)
Sick leave 1990			.059***	
			(.021)	
Amount sick leave 1993			.036***	.036***
			(.003)	(.003)
Amount sick leave 1990	.000***	.000***		
	(.000)	(.000)		
UI benefits 1993		.356***		.537***
		(.029)		(.033)
UI benefits 1992	.182***		.270***	

	(.037)		(.035)	
Amount UI 1993		-.019***	.015***	-.046***
		(.006)	(.003)	(.005)
Amount UI 1992	-.023***	-.023***	-.042***	-.031***
	(.007)	(.006)	(.005)	(.005)
Zero earnings 1991			-.126***	-.089*
			(.045)	(.046)
Zero earnings 1990			-.134***	-.127***
			(.049)	(.049)
Employed 1993		-.160***		-.218***
		(.031)		(.044)
Outside lf 1992	-.088**	-.082***		
	(.043)	(.030)		
Farm & mining sector	-.266***	-.238***	-.301***	-.196***
	(.081)	(.081)	(.061)	(.058)
Construction sector			-.242***	-.133***
			(.038)	(.031)
Manufacturing sector	-.164***	-.130***	-.145***	
	(.033)	(.031)	(.033)	
Finance sector			-.040	.074***
			(.037)	(.028)
Public sector	.085***	.122***	.128***	.234***
	(.020)	(.017)	(.033)	(.025)
Other sector	-.050**		-.131***	
	(.023)		(.030)	
Unemp-OLF		.186***		
		(.068)		
Unemp-emp		-.104**		-.122***
		(.042)		(.047)
OLF-emp		.084*		.139**
		(.043)		(.061)
OLF-OLF				-.215***
				(.045)
Engineering			-.049	-.066
			(.041)	(.041)
Humanities	.156	.158		
	(.103)	(.103)		
Natural science	.167***	.172***	.066	.067
	(.047)	(.047)	(.053)	(.053)
Imputed rank	-.051**	-.049*	-.097	-.092
	(.026)	(.027)	(.066)	(.068)
Ability rank	.652***	.673***	.636***	.644***
	(.035)	(.035)	(.037)	(.037)
N	249,307	249,267	335,991	335,966

Note: Standard errors within parentheses. * p<0.10, ** p<0.05, *** p<0.01. Specifications consider that propensity score estimates based on irrelevant variables may generate bias or increase variance of matching estimators (de Luna et al. 2011). Explanatory variables therefore are only included if p-values are below .2 or if they are required to achieve balanced samples. All specifications include a constant term, age-dummies and dummies for regional residence. Earnings are expressed in SEK 100,000. Outside the labor force (OLF). is defined as annual earnings below SEK 20,000 (approximately €2,200) and no transfers related to unemployment insurance.

Table A3. Females in komvux and comparison group. Descriptive means and balancing tests of matched samples. Selected variables. All amounts in 1000s of SEK, 2016 values.

	Unmatched					Matched samples			
	Treated		Untreated			Treated		Untreated	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Age	30.84	4.57	32.26	4.66	***	30.84	4.57	30.85	4.56
Years of schooling	10.44	0.82	10.34	0.86	***	10.44	0.82	10.44	0.82
Imputed rank	0.16	0.36	0.23	0.42	***	0.16	0.36	0.16	0.36
Ability rank	0.44	0.23	0.41	0.22	***	0.44	0.23	0.43	0.43
Parent years of sch.	10.83	1.89	10.42	1.71	***	10.82	1.89	10.83	10.83
Earnings 1993	95.08	80.00	106.82	77.89	***	95.22	80.02	95.59	77.88
Earnings 1992	104.46	77.40	111.19	77.90	***	104.62	77.37	104.65	78.10
Earnings 1991	103.19	73.11	108.80	74.58	***	103.22	73.09	103.80	73.61
Earnings 1990	108.41	72.83	113.99	74.92	***	108.42	72.81	108.64	73.02
Earnings diff 1990-92	-3.84	70.69	-2.78	70.24		-3.80	70.63	-3.99	72.30
Earnings diff 1992-93	-9.31	55.62	-4.33	53.32	***	-9.40	55.59	-9.05	54.14
Foreign born	0.04	0.19	0.03	0.18		0.04	0.19	0.03	0.18
No. of children	1.53	1.22	1.56	1.16	*	1.53	1.22	1.52	1.23
Child 0-3	0.38	0.49	0.38	0.49		0.38	0.49	0.38	0.49
Married	0.39	0.49	0.43	0.50	***	0.39	0.49	0.39	0.49
Divorced	0.07	0.25	0.05	0.23	***	0.07	0.25	0.07	0.25
Study allowance 1993	0.02	0.15	0.01	0.12	***	0.02	0.15	0.02	0.15
Parental leave 1993	0.31	0.46	0.33	0.47	***	0.31	0.46	0.31	0.46
Parental leave 1992	0.35	0.48	0.34	0.47	***	0.35	0.48	0.36	0.48
Parental leave 1991	0.36	0.48	0.34	0.47	***	0.36	0.48	0.36	0.48
Parental leave 1990	0.34	0.47	0.31	0.46	***	0.34	0.47	0.34	0.47
Amount parental 1993	120.15	263.33	122.82	262.76		120.32	263.58	125.48	266.32
Amount parental 1992	140.99	273.39	130.95	263.42	***	141.32	273.64	144.75	276.98
Amount parental 1991	142.39	265.10	130.48	255.69	***	142.50	265.17	144.10	268.71
Amount parental 1990	128.03	242.85	115.07	231.60	***	128.20	242.96	127.41	240.17
Early retirement 1993	0.01	0.08	0.03	0.18	***	0.01	0.08	0.01	0.08
Social welfare 1993	0.16	0.37	0.09	0.29	***	0.16	0.37	0.16	0.37
Sick leave 1993	0.29	0.45	0.26	0.44	***	0.29	0.45	0.29	0.45
Sick leave 1992	0.41	0.49	0.36	0.48	***	0.41	0.49	0.41	0.49
Sick leave 1991	0.79	0.41	0.76	0.43	***	0.79	0.41	0.78	0.41
Sick leave 1990	0.82	0.38	0.79	0.41	***	0.82	0.38	0.81	0.39
Amount sick leave 1993	46.34	162.27	40.58	141.76	***	46.41	162.47	47.46	158.47
UI benefits 1993	0.33	0.47	0.19	0.39	***	0.33	0.47	0.33	0.47
UI benefits 1992	0.22	0.42	0.15	0.35	***	0.22	0.42	0.22	0.41
UI benefits 1991	0.15	0.36	0.11	0.31	***	0.15	0.36	0.15	0.36
UI benefits 1990	0.12	0.33	0.09	0.29	***	0.12	0.33	0.13	0.33
Amount UI 1993	135.32	252.47	75.67	199.87	***	135.39	252.44	134.08	252.65
Amount UI 1992	79.20	193.90	50.87	160.90	***	79.28	194.06	77.79	194.07
Amount UI 1991	41.22	131.58	29.47	113.00	***	41.25	131.63	41.00	131.43
Amount UI 1990	24.24	90.42	18.46	79.26	***	24.26	90.48	26.20	94.50
Zero earnings 1993	0.17	0.38	0.14	0.35	***	0.17	0.38	0.18	0.38
Employed 1993	0.48	0.50	0.56	0.50	***	0.48	0.50	0.48	0.50
Employed 1992	0.52	0.50	0.56	0.50	***	0.52	0.50	0.52	0.50
Employed 1991	0.52	0.50	0.55	0.50	***	0.52	0.50	0.52	0.50
Employed 1990	0.53	0.50	0.57	0.50	***	0.53	0.50	0.53	0.50
Outside lf 1993	0.28	0.45	0.31	0.46	***	0.28	0.45	0.28	0.45
Construction sector	0.01	0.10	0.01	0.09		0.01	0.10	0.01	0.10
Manufacturing sector	0.07	0.26	0.12	0.32	***	0.07	0.26	0.08	0.26
Finance sector	0.06	0.24	0.06	0.24		0.06	0.24	0.06	0.24
Public sector	0.44	0.50	0.43	0.50	*	0.44	0.50	0.45	0.50
Emp-OLF	0.05	0.22	0.06	0.24	***	0.05	0.22	0.06	0.23
Emp-unemp	0.14	0.34	0.08	0.27	***	0.14	0.34	0.14	0.34
OLF-emp	0.06	0.24	0.08	0.27	***	0.06	0.24	0.06	0.24
OLF-unemp	0.07	0.25	0.03	0.18	***	0.07	0.25	0.07	0.25
Unemp-OLF	0.02	0.14	0.01	0.12	***	0.02	0.14	0.02	0.15
Unemp-emp	0.05	0.22	0.05	0.21	**	0.05	0.22	0.05	1.92
Observations	6635		293863			6604		24483	

Note: * p<0.10, ** p<0.05, *** p<0.01 balancing test of the difference. See note to Table A1 and text for variable definitions.

Table A4. Males in komvux and comparison group. Descriptive means and balancing tests of matched samples. Selected variables. All amounts in 1000s of SEK, 2016 values.

	Unmatched					Matched samples			
	Treated		Untreated			Treated		Untreated	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Age	29.88	4.31	32.17	4.62	***	29.88	4.31	29.87	4.29
Years of schooling	10.57	0.81	10.37	0.92	***	10.58	0.80	10.59	0.80
Imputed rank	0.02	0.15	0.04	0.18	***	0.02	0.15	0.02	0.16
Ability rank	0.35	0.22	0.28	0.21	***	0.35	0.22	0.35	0.23
Parent years of sch.	11.16	2.07	10.45	1.73	***	11.16	2.07	11.16	2.08
Earnings 1993	130.14	104.09	163.57	106.70	***	130.33	104.05	129.30	102.42
Earnings 1992	153.25	97.56	175.53	101.90	***	153.42	97.51	153.53	98.76
Earnings 1991	164.40	89.91	181.48	95.62	*	164.51	89.85	167.70	89.60
Earnings 1990	169.46	84.16	185.93	92.23	*	169.54	84.14	171.73	85.14
Earnings diff 1990-92	-16.18	85.58	-10.38	76.22	***	-16.12	85.59	-18.20	83.65
Earnings diff 1992-93	-22.97	67.94	-11.93	60.17	***	-23.09	67.89	-24.23	67.47
Foreign born	0.03	0.18	0.03	0.17		0.03	0.18	0.03	0.17
No. of children	0.80	1.10	1.07	1.14	**	0.80	1.10	0.78	1.07
Child 0-3	0.22	0.42	0.29	0.46	***	0.22	0.42	0.22	0.41
Married	0.21	0.41	0.30	0.46	***	0.21	0.41	0.21	0.41
Divorced	0.03	0.18	0.04	0.19	***	0.03	0.18	0.03	0.18
Study allowance 1993	0.02	0.14	0.01	0.08	***	0.02	0.14	0.02	0.14
Parental leave 1993	0.10	0.30	0.11	0.31	***	0.10	0.30	0.10	0.30
Parental leave 1992	0.10	0.31	0.11	0.31	***	0.10	0.31	0.10	0.30
Parental leave 1991	0.10	0.30	0.10	0.30		0.10	0.30	0.10	0.30
Parental leave 1990	0.08	0.27	0.09	0.28	*	0.08	0.27	0.08	0.28
Amount parental 1993	17.00	88.26	15.07	77.63	***	17.07	88.42	15.95	86.23
Amount parental 1992	17.09	87.05	14.11	73.49	***	17.12	87.13	17.39	91.27
Amount parental 1991	14.46	76.60	11.52	62.81	***	14.48	76.66	14.77	78.41
Amount parental 1990	10.56	60.32	8.62	49.91		10.58	60.36	10.82	63.26
Early retirement 1993	0.01	0.09	0.02	0.15	***	0.01	0.09	0.01	0.09
Social welfare 1993	0.16	0.36	0.09	0.28	**	0.16	0.36	0.16	0.36
Sick leave 1993	0.25	0.43	0.18	0.38	***	0.25	0.43	0.24	0.43
Sick leave 1992	0.34	0.48	0.26	0.44		0.34	0.48	0.34	0.48
Sick leave 1991	0.78	0.41	0.72	0.45	*	0.78	0.41	0.78	0.41
Sick leave 1990	0.81	0.39	0.75	0.43		0.81	0.39	0.81	0.39
Amount sick leave 1993	71.79	262.38	36.71	170.28		71.77	262.43	73.50	269.20
UI benefits 1993	0.38	0.49	0.24	0.43	***	0.39	0.49	0.39	0.49
UI benefits 1992	0.26	0.44	0.19	0.39	***	0.26	0.44	0.26	0.44
UI benefits 1991	0.16	0.36	0.11	0.32	***	0.16	0.36	0.15	0.35
UI benefits 1990	0.10	0.30	0.07	0.26	***	0.10	0.30	0.10	0.30
Amount UI 1993	221.67	351.37	144.68	307.95	***	222.49	351.76	228.43	358.91
Amount UI 1992	144.62	303.97	106.98	270.99	***	144.87	304.17	151.56	310.75
Amount UI 1991	58.91	176.58	46.11	165.14	***	59.01	176.72	57.68	178.53
Amount UI 1990	24.68	97.57	20.13	95.69	***	24.71	97.63	23.63	95.79
Zero earnings 1993	0.17	0.38	0.15	0.35	***	0.17	0.38	0.18	0.38
Employed 1993	0.58	0.49	0.70	0.46	***	0.58	0.49	0.57	0.49
Employed 1992	0.67	0.47	0.75	0.43	***	0.67	0.47	0.67	0.47
Employed 1991	0.75	0.43	0.80	0.40	**	0.75	0.43	0.76	0.42
Employed 1990	0.77	0.42	0.81	0.39	***	0.77	0.42	0.78	0.42
Outside lf 1993	0.18	0.38	0.16	0.36	***	0.18	0.38	0.18	0.38
Construction sector	0.13	0.34	0.14	0.35		0.13	0.34	0.12	0.33
Manufacturing sector	0.19	0.39	0.27	0.44	***	0.19	0.39	0.19	0.39
Finance sector	0.07	0.25	0.06	0.24		0.07	0.25	0.07	0.26
Public sector	0.13	0.34	0.07	0.25	***	0.13	0.34	0.14	0.34
Emp-OLF	0.04	0.19	0.02	0.14	**	0.04	0.19	0.03	0.18
Emp-unemp	0.22	0.42	0.14	0.35	***	0.22	0.42	0.22	0.42
OLF-emp	0.02	0.15	0.02	0.13	***	0.02	0.15	0.02	0.16
OLF-unemp	0.02	0.15	0.01	0.10	***	0.02	0.15	0.02	0.15
Unemp-OLF	0.01	0.11	0.01	0.08	*	0.01	0.11	0.01	0.11
Unemp-emp	0.06	0.25	0.07	0.25		0.06	0.25	0.07	0.25
Observations	4063		421139			4041		15406	

Note: * p<0.10, ** p<0.05, *** p<0.01 balancing test of the difference. See note to Table A1 and text for variable definitions.

Table A5. Females in college and comparison group. Descriptive means and balancing tests of matched samples. Selected variables. All amounts in 1000s of SEK, 2016 values.

	Unmatched					Matched samples			
	Treated		Untreated			Treated		Untreated	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Age	33.52	3.44	33.76	3.46	***	33.53	3.44	33.53	3.43
Years of schooling	11.48	0.62	11.27	0.50	***	11.48	0.62	11.50	0.62
Imputed rank	0.15	0.36	0.18	0.38	***	0.15	0.36	0.15	0.36
Ability rank	0.61	0.24	0.50	0.24	***	0.61	0.24	0.61	0.24
Parent years of sch.	11.23	2.20	10.65	1.88	***	11.23	2.20	11.26	2.20
Earnings 1993	105.37	89.16	116.29	83.63	***	105.82	89.19	106.10	85.10
Earnings 1992	111.91	82.81	119.46	83.04	***	112.14	82.73	112.04	83.74
Earnings 1991	114.29	78.69	116.83	80.34	***	114.48	78.64	112.68	80.80
Earnings 1990	118.39	80.06	121.17	80.23	***	118.52	79.96	117.58	80.79
Earnings diff 1990-92	-6.32	74.49	-1.69	74.89	***	-6.38	74.41	-5.54	75.55
Earnings diff 1992-93	-6.39	60.45	-3.13	57.73	***	-6.32	60.40	-5.94	57.79
Foreign born	0.03	0.16	0.03	0.17		0.03	0.16	0.02	0.15
No. of children	1.54	1.18	1.59	1.12	***	1.55	1.18	1.52	1.19
Child 0-3	0.30	0.46	0.39	0.49	***	0.30	0.46	0.30	0.46
Married	0.47	0.50	0.49	0.50	***	0.47	0.50	0.48	0.50
Divorced	0.08	0.27	0.06	0.24		0.08	0.27	0.08	0.28
Study allowance 1993	0.12	0.33	0.01	0.11	***	0.12	0.33	0.11	0.31
Parental leave 1993	0.26	0.44	0.34	0.47	**	0.26	0.44	0.26	0.44
Parental leave 1992	0.31	0.46	0.35	0.48		0.31	0.46	0.31	0.46
Parental leave 1991	0.35	0.48	0.35	0.48		0.35	0.48	0.34	0.47
Parental leave 1990	0.35	0.48	0.34	0.47		0.35	0.48	0.35	0.48
Amount parental 1993	86.91	234.56	125.43	272.34		87.05	235.14	90.29	233.14
Amount parental 1992	114.00	254.98	136.78	274.44	**	114.05	254.99	118.57	260.52
Amount parental 1991	122.91	251.02	139.31	266.75	***	123.15	251.26	123.36	251.54
Amount parental 1990	128.71	249.19	126.18	243.75	**	128.81	249.27	121.43	236.57
Early retirement 1993	0.01	0.08	0.02	0.14	***	0.01	0.08	0.01	0.08
Social welfare 1993	0.08	0.27	0.07	0.25	***	0.08	0.27	0.08	0.28
Sick leave 1993	0.21	0.41	0.23	0.42	***	0.21	0.41	0.21	0.41
Sick leave 1992	0.33	0.47	0.33	0.47	***	0.33	0.47	0.33	0.47
Sick leave 1991	0.74	0.44	0.75	0.43	***	0.74	0.44	0.74	0.44
Sick leave 1990	0.78	0.42	0.79	0.41	***	0.78	0.41	0.78	0.42
Amount sick leave 1993	38.89	174.78	38.80	144.44	***	39.22	175.47	34.73	140.50
UI benefits 1993	0.33	0.47	0.19	0.39	***	0.33	0.47	0.33	0.47
UI benefits 1992	0.22	0.41	0.14	0.35	***	0.22	0.41	0.22	0.41
UI benefits 1991	0.14	0.35	0.10	0.30	***	0.14	0.35	0.15	0.36
UI benefits 1990	0.11	0.31	0.09	0.28	***	0.11	0.31	0.11	0.32
Amount UI 1993	118.45	229.83	75.79	201.10	***	118.64	229.28	122.47	234.75
Amount UI 1992	70.11	172.38	49.41	158.75	***	70.43	172.73	70.66	178.86
Amount UI 1991	37.17	117.96	27.90	109.67	***	37.30	118.14	40.88	130.78
Amount UI 1990	21.94	86.02	17.86	78.63	***	22.00	86.12	21.71	83.26
Zero earnings 1993	0.14	0.35	0.12	0.33	***	0.14	0.35	0.13	0.34
Employed 1993	0.50	0.50	0.59	0.49	***	0.50	0.50	0.51	0.50
Employed 1992	0.53	0.50	0.58	0.49	***	0.54	0.50	0.54	0.50
Employed 1991	0.55	0.50	0.57	0.49	***	0.55	0.50	0.55	0.50
Employed 1990	0.55	0.50	0.58	0.49	***	0.55	0.50	0.55	0.50
Outside lf 1993	0.26	0.44	0.28	0.45	***	0.26	0.44	0.25	0.43
Construction sector	0.01	0.09	0.01	0.11	**	0.01	0.09	0.01	0.10
Manufacturing sector	0.06	0.24	0.11	0.31	***	0.06	0.24	0.06	0.24
Finance sector	0.09	0.29	0.10	0.30	**	0.09	0.29	0.10	0.29
Public sector	0.45	0.50	0.39	0.49	***	0.45	0.50	0.45	0.50
Emp-OLF	0.05	0.22	0.06	0.24	***	0.05	0.22	0.05	0.22
Emp-unemp	0.13	0.33	0.08	0.27	***	0.13	0.34	0.13	0.34
OLF-emp	0.07	0.25	0.09	0.28	***	0.07	0.25	0.07	0.25
OLF-unemp	0.06	0.23	0.03	0.18	***	0.06	0.24	0.06	0.24
Unemp-OLF	0.02	0.13	0.01	0.11	***	0.02	0.13	0.02	0.13
Unemp-emp	0.05	0.21	0.04	0.21		0.05	0.21	0.04	0.20
Observations	2757		247313			2732		10282	

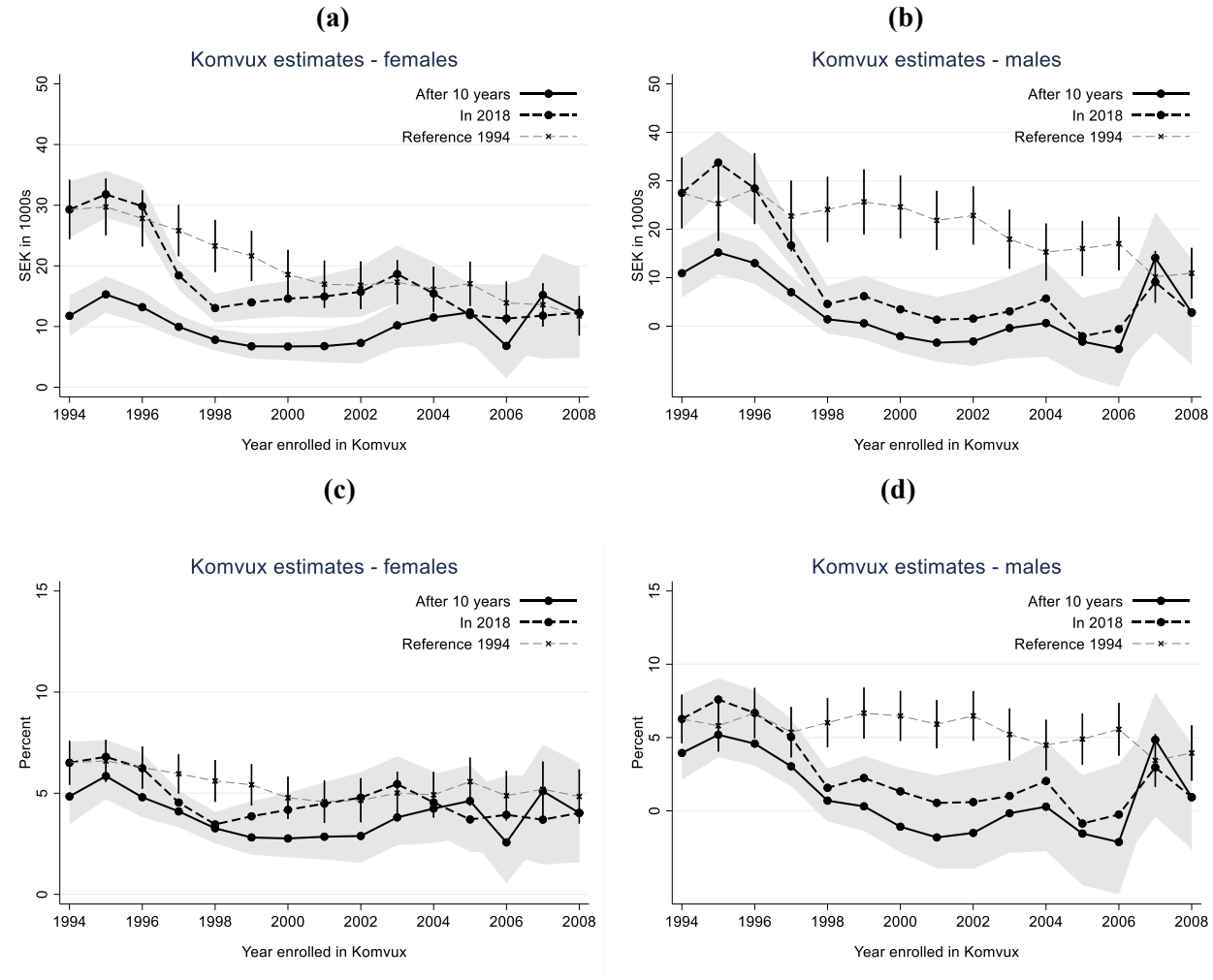
Note: * p<0.10, ** p<0.05, *** p<0.01 balancing test of the difference. See note to Table A1 and text for variable definitions.

Table A6. Males in college and comparison group. Descriptive means and balancing tests of matched samples. Selected variables. All amounts in 1000s of SEK, 2016 values.

	Unmatched					Matched samples				
	Treated		Untreated			Treated		Untreated		
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
Age	32.88	3.37	34.01	3.47	***	32.88	3.37	32.81	3.34	
Years of schooling	11.65	0.79	11.35	0.65	***	11.66	0.80	11.68	0.80	
Imputed rank	0.02	0.13	0.02	0.14		0.02	0.13	0.01	0.12	
Ability rank	0.51	0.26	0.37	0.25	***	0.51	0.26	0.52	0.28	
Parent years of sch.	11.37	2.28	10.68	1.90	***	11.36	2.28	11.45	2.29	
Earnings 1993	131.32	117.09	185.62	119.55	***	131.56	117.21	131.84	111.31	
Earnings 1992	153.10	109.45	195.65	110.43	***	153.40	109.39	151.89	107.39	
Earnings 1991	176.18	98.01	201.09	103.44	***	176.35	97.92	173.76	97.84	
Earnings 1990	187.20	93.60	205.71	98.24	***	187.33	93.52	186.02	92.79	
Earnings diff 1990-92	-33.83	95.94	-10.03	77.98	***	-33.92	95.96	-34.13	89.07	
Earnings diff 1992-93	-21.68	71.73	-9.95	80.04	***	-21.84	71.61	-20.04	70.25	
Foreign born	0.03	0.18	0.03	0.17		0.03	0.18	0.04	0.19	
No. of children	0.98	1.12	1.19	1.16	***	0.98	1.12	0.98	1.15	
Child 0-3	0.29	0.46	0.34	0.47	***	0.29	0.46	0.29	0.45	
Married	0.34	0.48	0.39	0.49	***	0.35	0.48	0.34	0.47	
Divorced	0.04	0.20	0.04	0.20		0.04	0.20	0.03	0.18	
Study allowance 1993	0.13	0.33	0.01	0.07	***	0.13	0.33	0.11	0.31	*
Parental leave 1993	0.13	0.34	0.13	0.34		0.13	0.34	0.14	0.35	
Parental leave 1992	0.11	0.32	0.13	0.34	**	0.11	0.32	0.12	0.32	
Parental leave 1991	0.13	0.34	0.12	0.33		0.13	0.34	0.13	0.34	
Parental leave 1990	0.12	0.33	0.11	0.31		0.12	0.33	0.12	0.32	
Amount parental 1993	21.87	101.27	20.35	91.02		21.96	101.54	24.34	109.15	
Amount parental 1992	18.17	85.21	18.88	85.38		18.23	85.34	22.88	101.94	
Amount parental 1991	20.21	89.10	15.61	74.14	***	20.28	89.24	21.16	95.11	
Amount parental 1990	18.47	82.24	11.82	58.95	***	18.51	82.34	19.03	89.43	
Early retirement 1993	0.00	0.07	0.01	0.12	***	0.00	0.07	0.00	0.06	
Social welfare 1993	0.07	0.26	0.07	0.26		0.07	0.26	0.08	0.26	
Sick leave 1993	0.21	0.41	0.16	0.37	***	0.21	0.40	0.22	0.41	
Sick leave 1992	0.29	0.46	0.24	0.43	***	0.29	0.46	0.29	0.46	
Sick leave 1991	0.72	0.45	0.71	0.45		0.72	0.45	0.71	0.45	
Sick leave 1990	0.77	0.42	0.75	0.44	***	0.77	0.42	0.75	0.43	
Amount sick leave 1993	95.86	357.78	37.93	182.58	***	95.54	357.51	107.52	362.10	
UI benefits 1993	0.39	0.49	0.20	0.40	***	0.39	0.49	0.39	0.49	
UI benefits 1992	0.27	0.45	0.16	0.36	***	0.28	0.45	0.26	0.44	
UI benefits 1991	0.16	0.37	0.09	0.29	***	0.16	0.37	0.15	0.36	
UI benefits 1990	0.10	0.30	0.06	0.24	***	0.10	0.30	0.09	0.29	
Amount UI 1993	183.40	302.10	122.14	288.56	***	183.14	301.30	181.73	301.65	
Amount UI 1992	128.77	264.94	88.16	248.97	***	129.20	265.28	129.30	279.14	
Amount UI 1991	61.55	183.45	39.46	154.73	***	61.75	183.72	61.11	186.01	
Amount UI 1990	28.10	108.91	18.48	93.24	***	27.78	107.73	27.58	110.51	
Zero earnings 1993	0.19	0.40	0.12	0.33	***	0.19	0.40	0.18	0.38	
Employed 1993	0.54	0.50	0.75	0.43	***	0.54	0.50	0.54	0.50	
Employed 1992	0.63	0.48	0.79	0.41	***	0.64	0.48	0.64	0.48	
Employed 1991	0.76	0.42	0.83	0.38	***	0.77	0.42	0.75	0.43	
Employed 1990	0.80	0.40	0.84	0.36	***	0.81	0.39	0.79	0.40	
Outside lf 1993	0.18	0.38	0.13	0.34	***	0.18	0.38	0.18	0.39	
Construction sector	0.08	0.26	0.14	0.35	***	0.08	0.26	0.07	0.26	
Manufacturing sector	0.18	0.39	0.25	0.43	***	0.18	0.39	0.19	0.39	
Finance sector	0.11	0.31	0.09	0.29	***	0.11	0.31	0.11	0.32	
Public sector	0.17	0.37	0.08	0.27	***	0.17	0.37	0.18	0.38	
Emp-OLF	0.04	0.20	0.02	0.14	***	0.04	0.20	0.05	0.21	
Emp-unemp	0.19	0.39	0.12	0.33	***	0.19	0.39	0.19	0.39	
OLF-emp	0.03	0.17	0.02	0.13	***	0.03	0.17	0.03	0.18	
OLF-unemp	0.03	0.18	0.01	0.09	***	0.03	0.18	0.04	0.20	
Unemp-OLF	0.01	0.11	0.01	0.08	***	0.01	0.11	0.01	0.11	
Unemp-emp	0.04	0.20	0.06	0.23	***	0.04	0.20	0.05	0.21	
Observations	2108		334473			2090		7710		

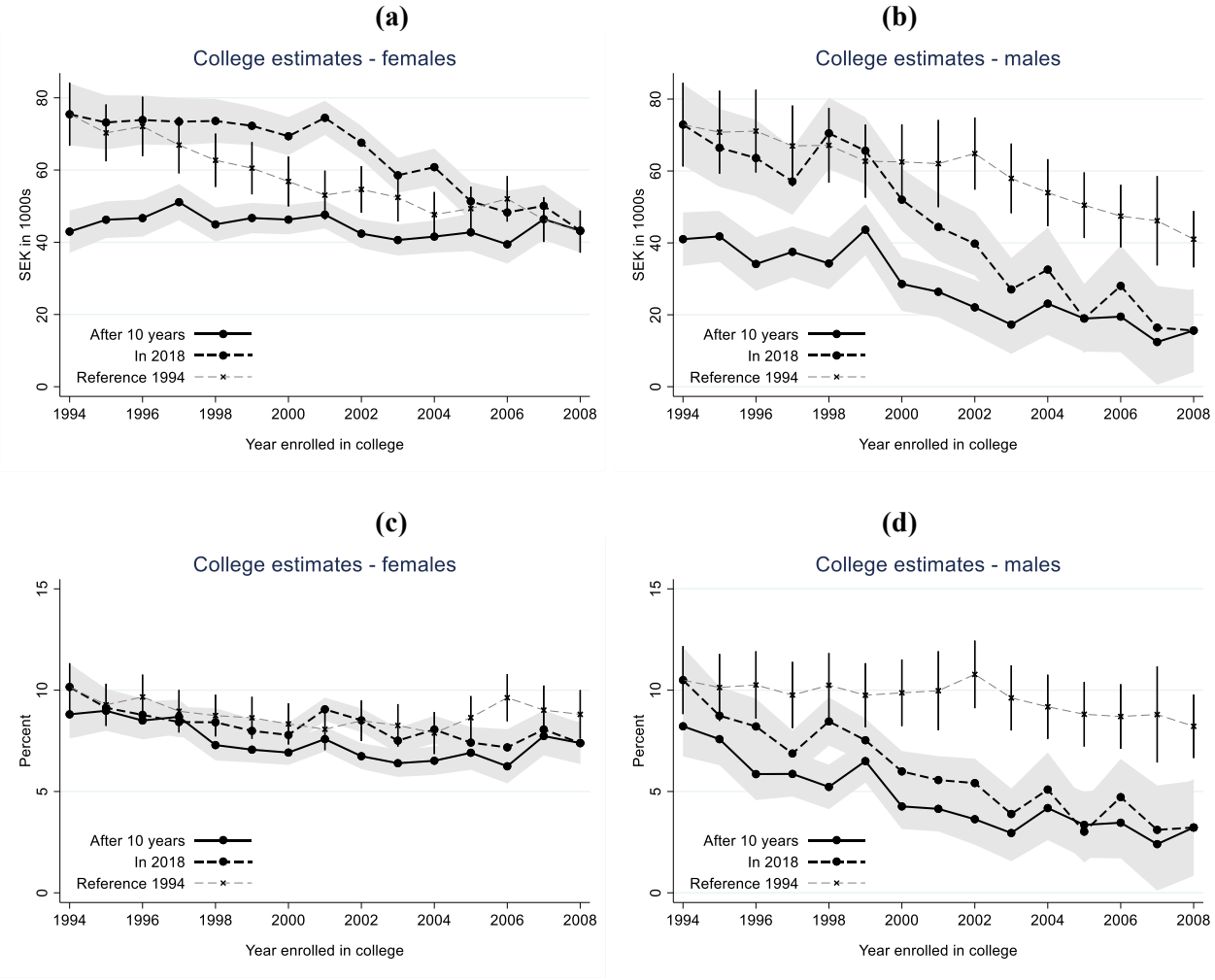
Note: * p<0.10, ** p<0.05, *** p<0.01 balancing test of the difference. See note to Table A1 and text for variable definitions.

Figure A1. Figure 2 repeated with confidence intervals. First time Komvux enrollees for the years 1994-2008. Estimated differences in annual earnings 2018 between participants and matched comparison groups.



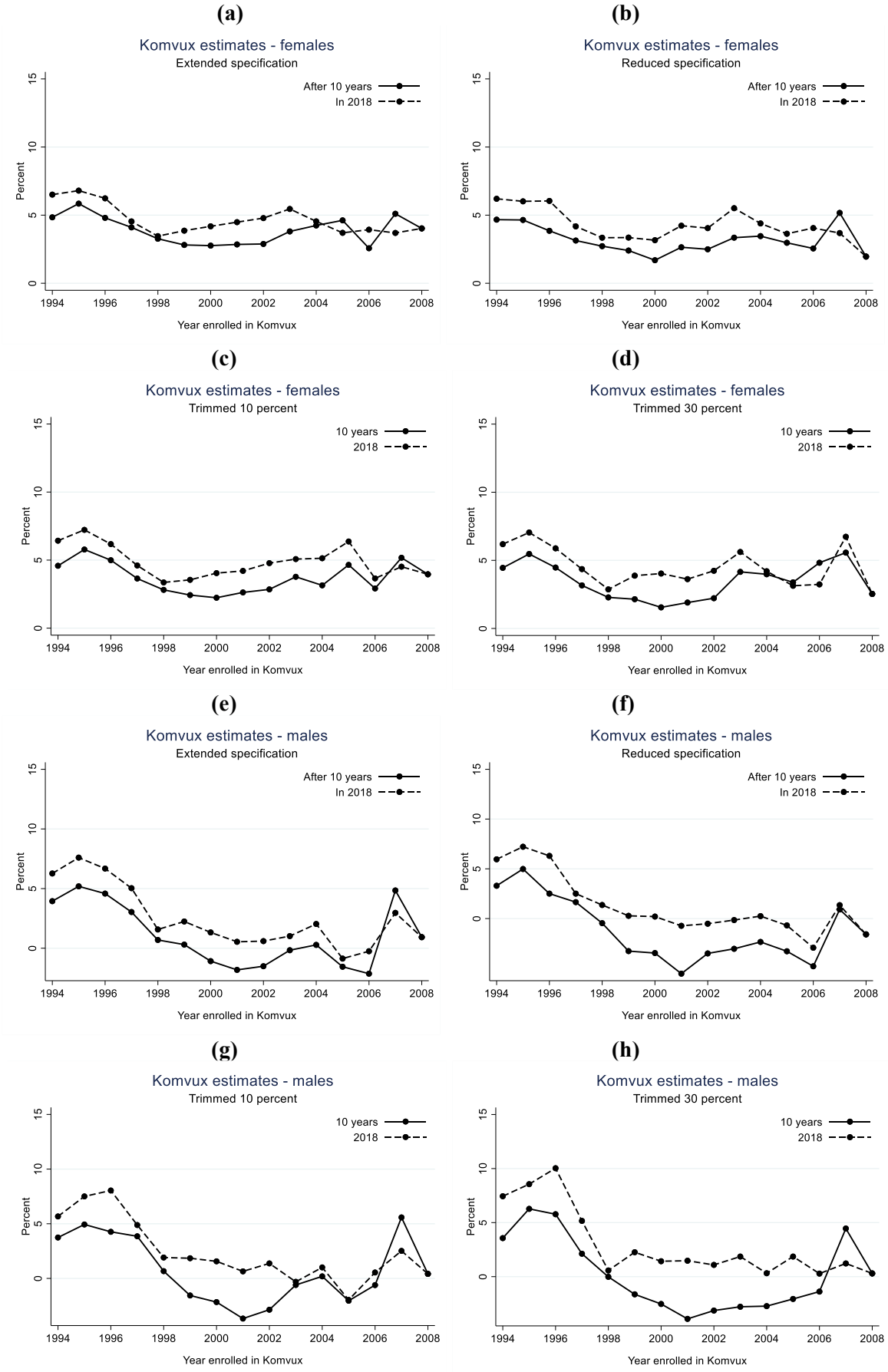
Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE enrollees in that year. The solid black lines represent estimates on annual earnings 10 years after program entry while the black dashed lines are estimates on annual earnings 2018. The dots along the gray dashed lines reflect the results for participants enrolled for the first time in 1994, applying the same length of the follow-up period. For example, the length of the follow-up period for participants 2005 is 13 years (until 2018). The dot on the gray dashed lines the represents the participants in 1994 13 years after enrollment (2007). Shaded areas (spikes for the 1994 reference line) indicate 95-percent confidence intervals based on bootstrapped standard errors (1000 replications).

Figure A2. Figure 3 repeated with confidence intervals. First time college enrollees for the years 1994-2008. Estimated differences in annual earnings 2018 between participants and matched comparison groups.



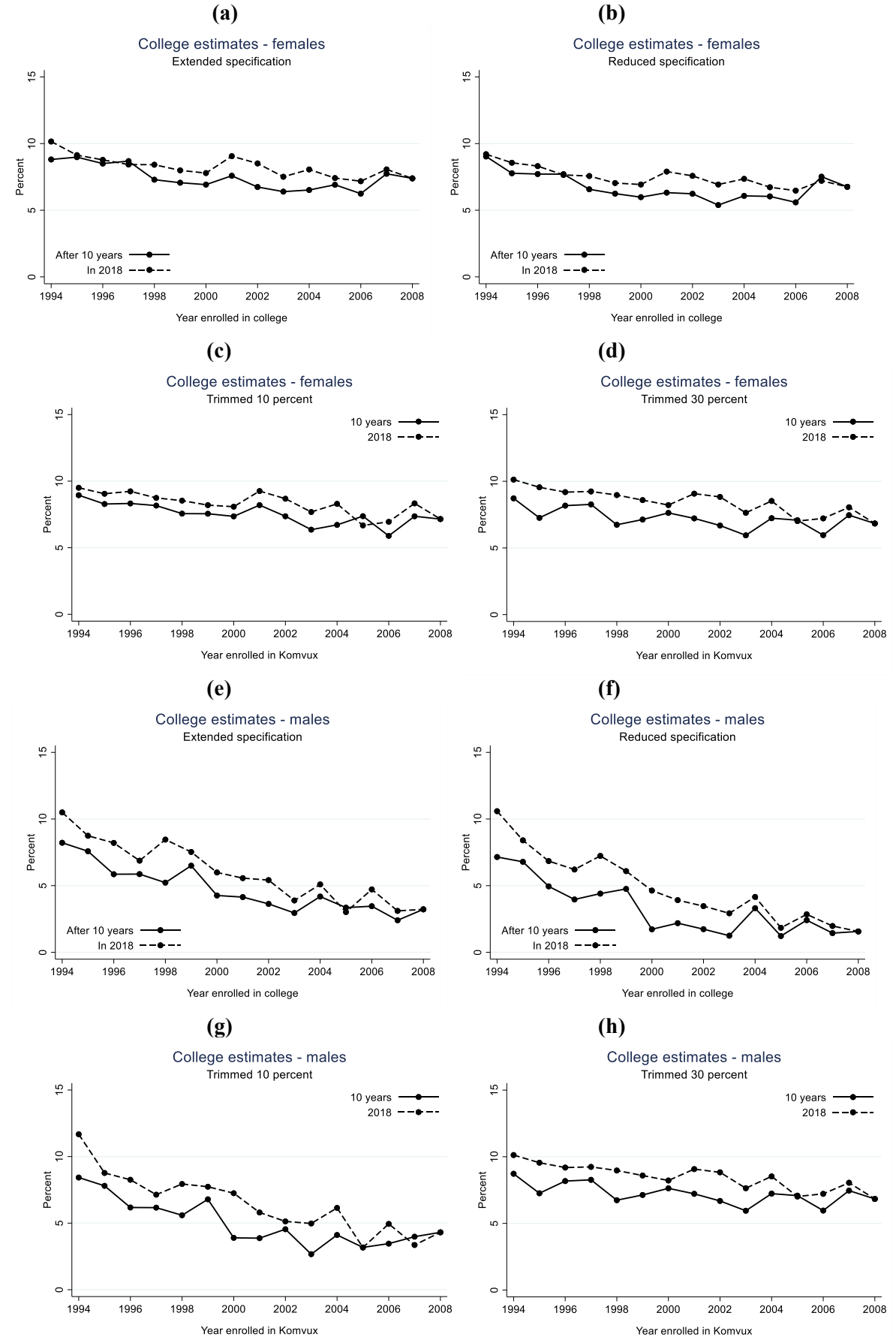
Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE enrollees in that year. The solid black lines represent estimates on annual earnings 10 years after program entry while the black dashed lines are estimates on annual earnings 2018. The dots along the gray dashed lines reflect the results for participants enrolled for the first time in 1994, applying the same length of the follow-up period. For example, the length of the follow-up period for participants 2005 is 13 years (until 2018). The dot on the gray dashed lines the represents the participants in 1994 13 years after enrollment (2007). Shaded areas (spikes for the 1994 reference line) indicate 95-percent confidence intervals based on bootstrapped standard errors (1000 replications).

Figure A3. Komvux AE, results from alternative matching specifications.



Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE enrollees in that year. The solid lines represent estimates on annual earnings 10 years after program entry while the dashed lines are estimates on annual earnings 2018.

Figure A4. College AE, results from alternative matching specifications.



Note: The x-axis states the year of first-time enrollees. Each dot along solid lines represent the estimated impact of AE enrollees in that year. The solid lines represent estimates on annual earnings 10 years after program entry while the dashed lines are estimates on annual earnings 2018.