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Evaluating the Impact on Labor Earnings of Higher Vocational Education*

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Abstract: International bodies such as the EU and the OECD have repeatedly advocated improved opportunities for adults to improve their schooling. However, formal education for adults is expensive, particularly in terms of foregone earnings, and the returns to society and the individual is uncertain. Until recently, there is only scant evidence on the labor market impacts of post-secondary education for adults outside the US. This paper focuses on the earnings effects of higher vocational education (HVE) in Sweden. In just 25 years, HVE has expanded from a small pilot project to comprise one third of all entrants in tertiary education in Sweden. Using difference-in-difference matching methods and longitudinal populational register data, we find positive effects on annual earnings for HVE-entrants compared to non-entrants regardless of gender or age. For our young sample (aged 19-29), the short-term returns peak at 17 percent for men and 30 percent for women, and 11 years after the entry decision the returns remain at a fairly high 8 percent for men but drops to 3 percent for women. The steep drop in returns for young women is likely explained by postponed childbearing. For our older sample (aged 30-54), we find a more stable effect of about 4-6 percent for men and 6 percent for women. Back of the envelope calculations show a positive internal rate of return of HVE for society of at least 6.7 percent.

Keywords: Adult education, Self-selection, Propensity score matching

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

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1. Introduction

International bodies such as the EU and the OECD have repeatedly issued recommendations that countries should increase the possibilities for adults to improve their human capital (e.g., EU 2000, 2001, OECD 1998, 2001, 2021). However, in practice, large scale government policies favoring extensive education for adults are rare. This may be because it is difficult for national governments to gain political support for such measures. Formal education for adults is expensive, particularly in terms of foregone production, and the payoff to society and the individual is uncertain. One exception from this rule is the Swedish Higher Vocational Education (HVE), which started as a small pilot scheme in 1996, slowly grew and became permanent in 2001. HVE has since become increasingly popular and in 2020 HVE represented one third of newly registered in tertiary education in Sweden, of which 75 percent were aged 25 or above and half aged 30 or above. [1] The average HVE program length is 1.7 years. A strong focus is placed on the participation of employers, whose involvement is a pre-condition for government funding of HVE programs. However, an important caveat is that there is until now only scant evidence of its long-term impact on participants' labor market outcomes.

In this paper, we estimate the impact of HVE on annual earnings and employment using detailed Swedish population register data. We follow more than 100,000 participants enrolled in 2005 or later up to 11 years after their first year of registration. We address selection by applying difference-in-difference matching on the propensity score. Besides controlling for a large set of observable characteristics, the set-up also controls for time-invariant unobserved characteristics which correlate with earnings. Regarding time-varying characteristics, we check the stability of our results by applying two models which assume different dynamics prior to program enrollment.

The literature evaluating education for adults took off with Jacobson, Lalonde and Sullivan at the start of this century (Jacobson et al. 2003, 2005a, 2005b). They primarily focused on laid-off workers aged 25 to 59 who registered at community colleges in Washington State. Individual fixed effects estimates indicated earnings increases of about 10–13 percent for females and 7–9 percent for males, with the longest follow up being seven years. These returns were based on the number of credits completed, which was reasonable since a feature of community college is that most participants do not complete their studies. Analyzing community colleges in Kentucky, Jepsen et al. (2014) used non-completers (approximately 70 percent of the students) as comparison group to those completing certificates, diplomas, or associate degrees. The results implied returns comparable to or considerably higher than those reported by Jacobson et al., but short-term certificates yield small or no returns. As longitudinal data have become increasingly accessible in a number of US states, several similar studies of community colleges have been published which evaluate the impact of credit accumulation, certificates and associate degrees, primarily for individuals under age 30.[2]

1 Statistics Sweden homepage: https://www.statistikdatabasen.scb.se/

2 A summary is provided by Belfield and Bailey (2017); see also Bahr et al. (2015), Dadgar and Trimble (2015), Liu, et al. (2015), Zeidenberg et al. (2015), Bettinger and Soliz (2016), Xu and Trimble (2016) and Stevens et al. (2015).

An important difference between the European and US contexts, as pointed out by Böckerman et al. (2018), is that most students in Europe attend studies full time. Students are also entitled to some form of financial support, and their explicit objective is to complete their studies. Compared with completion rates in US community colleges of 30–40 percent (Jepsen et al. 2014, Shapiro et al. 2014), the completion rate of the Polytechnique program in Finland is 70 percent and for the Swedish HVE 74 percent with 90 percent attending full-time studies.

There are few published studies evaluating the payoff to higher education for adults in Europe, perhaps partly explained by the scarcity of programs offered. Böckerman (2018) evaluate polytechnic attendance in Finland and include samples of both traditional-age students and older students. Ten years after entry into the program, the earnings returns are about €3,800 for older students (aged 25–50), whether males or females. Students aged 19–24 have comparable estimates, with males slightly lower and females slightly higher estimates. Results by field of study indicate that for those aged 25–50 the highest payoff is found for technology and health, about €5,000, whereas the returns for business participants are about €3,500. Stenberg and Westerlund (2016) evaluate Swedish college enrollees aged 29–55 and report positive earnings estimates of approximately 10 percent for females and 5.5 percent for males. In a previous study, they also found college for registered 42–55 years old to delay retirement age by about 6 months (Stenberg and Westerlund 2013).[3] There is only one published study evaluating HVE. Andersson et al. (2014) focus on 546 participants aged 21–35 enrolled in 2000, when the HVE was still a pilot project. They follow individuals eight years after enrolment and report positive estimates on annual earnings ranging from 3 to 8 percent.

The contribution of this paper is to provide the first comprehensive evaluation of the economic impact of HVE. In short, our results indicate support for the international calls to increase opportunities for low or mid-skilled to enhance their vocational skills. Our results imply that HVE has positive effects on annual earnings regardless of gender or age, which persist during our decade-long observation window post treatment. A feature of our estimates for males and females aged 19–29 is that a short-term follow up is likely to overstate the long-term impact of HVE. The impact on employment is about 10 percent in the years following HVE completion, presumably reflecting its close link to the labor market. This effect is reduced to zero or small employment differences during our observations window. From society's point of view, back of the envelope calculations imply positive internal rates of return of at least 6.7 percent.

2. Higher vocational education in Sweden

In Sweden, compulsory school consists of nine years of education at the primary and lower secondary levels (typically ending when the pupils are 15 or 16 years old). After

3 In a slightly different vein, Hällsten (2012) evaluate tertiary level education for Swedish workers 30 years or older between 1985 and 2003, but condition on stable employment after treatment, and report a 2 percent return per year of studies.

compulsory school, individuals can apply to upper secondary education (comparable to high school in the United States). Students choose between different academic and vocational three-year programs. The vocational programs previously lasted two years, but after a major educational reform in 1991, they were extended to three years and included more general education, thereby ensuring basic eligibility for university studies.

At the post-secondary level, higher vocational education (HVE) has become an important part of the Swedish school system. HVE was introduced as a pilot project in 1996, became permanent in 2001 and since 2009 has been organized under one governing authority, The Swedish National Agency for Higher Vocational Education. A distinguishing feature of HVE is its strong link to the labor market. Programs are offered in specific fields where there is an explicit demand for labor. Theoretical studies are combined with workplace training. Approximately 25 percent of the program content is devoted to workplace training. Employers and industry actively contribute to and participate in HVE by initiating programs, taking part as lecturers, and by providing work placements.

The number of HVE entrants has steadily increased over time. In 2020, approximately 36,000 students started HVE, corresponding to one-third of all entrants in tertiary education in Sweden.[4] Close to 90 percent of the students attend HVE full time. HVE spans a wide range of fields. The most popular programs are 1) business and administration, 2) health care and social work, 3) planning and construction, 4) technology and engineering and 5) computer science and information technology. About 80 percent of the students attend one of these five programs. Most HVE programs require two years to complete, but some are one-year programs, and a few are up to three years in length. The completion rate is slightly above 70 percent.[5] Depending on the length of the program, students receive HVE diplomas at two different levels after completion. Note that credits from HVE does not automatically count as corresponding credits from traditional university education.

To be qualified for HVE, an applicant must have prior knowledge equivalent to upper secondary education, and some programs have additional entry requirements. On average, there are two qualified applicants for each available slot. Applicants compete for slots on the basis of tests, interviews, prior education, and grades. The admission process is highly decentralized, and each school handles admissions to their programs.

With few exceptions, HVE programs are free of charge. HVE participants are entitled to the same type of study allowance as students at traditional universities. A full-time student can receive up to $1,200 per month, of which approximately one-third is a grant, and two-thirds is a loan with an interest rate below market level to be repaid within a maximum of 25 years.

4 All data in this section refer to the latest available year (2019 or 2020) and can be found at Statistics Sweden's homepage: https://www.statistikdatabasen.scb.se/

5 In addition, about 10 percent of the students participate during the entire length of a program but end without formally graduating.

3. Data

The analysis is based on detailed population-wide longitudinal register data administered by Statistics Sweden. The data cover the period 2000–2019 and are updated annually. Using unique personal identifiers, we can combine information from different registers and construct a panel data set covering all residents in Sweden. The Swedish register data are known to be highly reliable and to have nearly complete coverage.

The data set includes all first-time entrants in HVE during the years 2005–2016 who were between 19 and 54 years old at the time of entry and had upper secondary education as their highest level of education.[6] Since we will be focusing on the returns to enrolling in HVE, we do not condition on completion status. In total, there are about 130,000 first-time entrants that meet these requirements and they constitute our treatment group. For each year in 2005–2016, we draw a three percent random sample of individuals who neither the year in question, nor previously, are registered as HVE entrants and who fulfill the same age and educational requirements as the treatment group. This is our comparison group, which in total includes about 900,000 non-entrants. Both groups are followed three to five years before potential entry in HVE and up to eleven years after. Note that in the sampling, we do not condition on events occurring after the entry decision. For instance, we do not condition on future HVE enrollment in the comparison group[7] or future university enrollment in either of the two groups.[8]

We focus primarily on the effect of HVE on annual gross labor earnings. Annual earnings are deflated to 2019 prices using the national consumer price index. By definition, annual earnings can be expressed as a function of e.g., hourly earnings and the number of hours employed during a year. Annual earnings thus capture wage effects as well as effects on unemployment and labor supply decisions.

The data include a rich set of pre-treatment covariates. Up to five years before potential entry in HVE, we have information on: annual earnings; number of days registered as unemployed; and five different benefits/transfers related to unemployment, labor market programs, parental leave, studies, and social welfare. The data set also includes ability related variables, such as grade point average from compulsory school and parents' educational background. In addition, the data set contain basic information, such as gender, age, family situation, country of birth, level and field of education, and region of residence (all measured the year before potential entry in HVE).

Table 1 presents means of selected variables separately by gender and two age groups, 19–29 and 30–54 years, which we will refer to as the younger and the older samples, respectively. The treated individuals in the younger sample are, on average, less likely to be married and to have children at home, and they also have slightly higher educational attainment in terms of years of schooling. Regarding the ability related variables, the

6 A total of 98 percent of all first-time entrants during the period were in the age group 19 to 54, and 77 percent had upper secondary education as the highest level of education.

7 In the comparison group, 3.4 percent enter HVE at some point after the year in which treatment status is defined.

8 Andersson et al. (2014) exclude individuals who later attend university. Böckerman et al. (2018) also exclude individuals who at or after the entry decision attend university, but in a supplementary analysis they find that not conditioning on future university education gives qualitatively similar results.

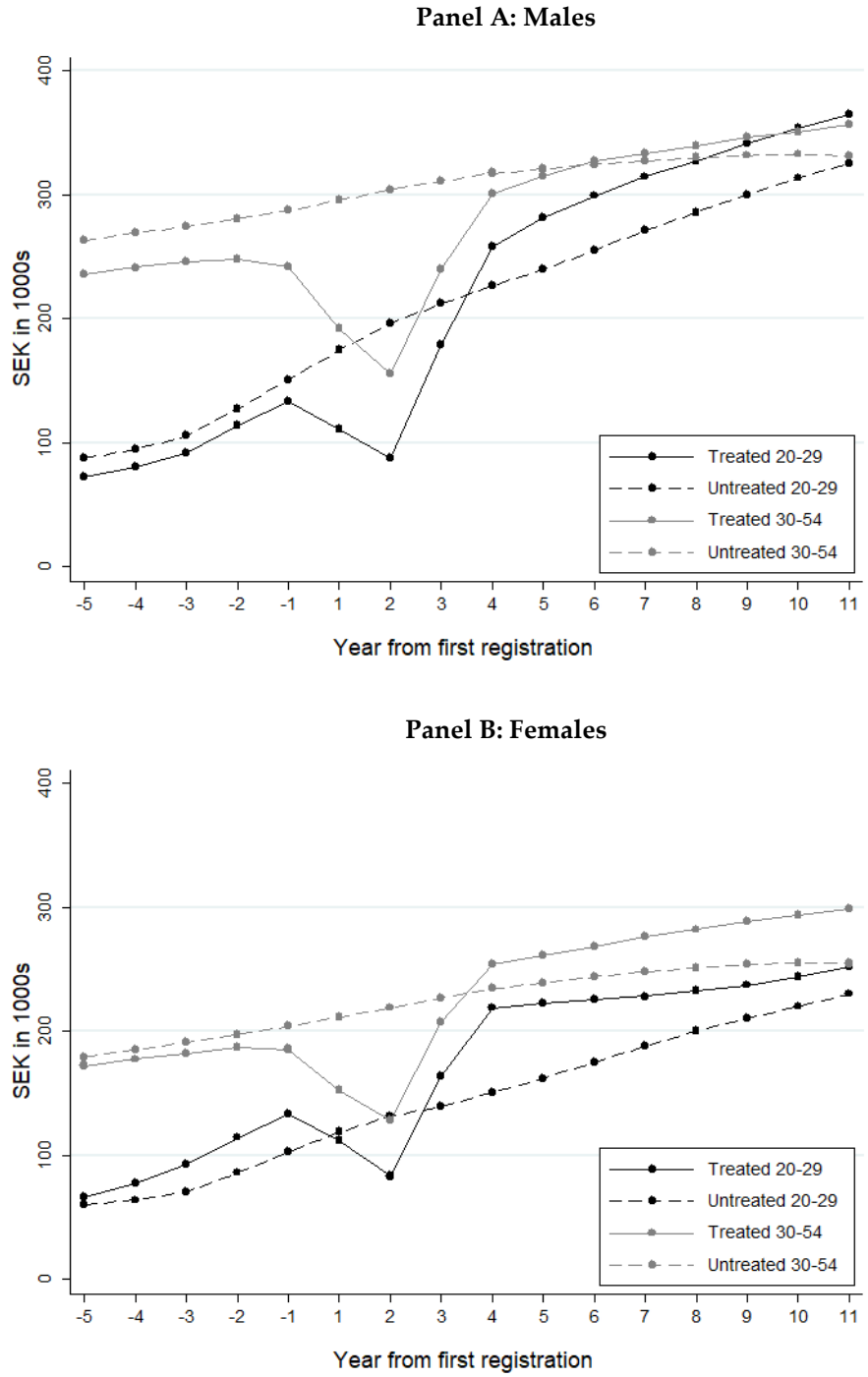
treated in the younger sample have higher GPA from compulsory school (about seven percentile ranks) and have parents with somewhat higher educational attainment. In terms of unemployment history, treated males in the younger sample seem to be on a more negative path than untreated males. Treated females in the younger sample have higher earnings (year t-2) than untreated females and more often receive study allowance (year t-1). The opposite holds for younger males. Treated individuals in the older sample are, on average, four to five years younger and have completed slightly more years of schooling. The average earnings are lower for treated individuals in the older sample (particularly for males). This finding might be explained by a more negative unemployment history and greater participation in formal education (as indicated by a higher incidence of study allowance) among treated compared to untreated in the older sample.

Table 1 Descriptive mean statistics (selected variables)

	Females				**Males**			
	Below 30 years old		30 years or older		Below 30 years old		30 years or older	
	Treated	Untreated	Treated	Untreated	Treated	Untreated	Treated	Untreated
Age	23.24	23.04	38.95	42.97	22.77	23.34	37.35	42.57
Married/cohabit	0.09	0.15	0.64	0.61	0.03	0.08	0.49	0.55
Child at home	0.10	0.17	0.77	0.70	0.03	0.07	0.47	0.54
Years of schooling	11.61	11.06	11.50	11.00	11.36	11.03	11.39	10.88
GPA at age 16	53.91	46.84	-	-	43.44	36.50	-	-
Days unemp t-3	21.42	25.21	30.81	30.19	23.51	31.52	38.51	30.66
Days unemp t-2	25.05	29.66	31.64	30.52	29.07	36.89	40.00	30.39
Days unemp t-1	30.52	33.34	39.28	30.77	39.87	41.26	50.26	30.08
Income t-2	114.57	86.49	187.21	198.59	114.24	128.37	249.56	283.17
Study allowance t-1	0.38	0.44	0.12	0.03	0.44	0.35	0.09	0.01
Born in Sweden	0.92	0.89	0.88	0.84	0.91	0.89	0.85	0.86
Parent years of schooling	12.93	12.76	11.66	11.13	13.26	12.81	12.10	11.27
Metropolitan area	0.57	0.51	0.54	0.50	0.55	0.49	0.56	0.49
Observations	42,060	141,447	26,188	272,249	49,146	168,331	15,509	332,836

Figure 1 displays trajectories of annual earnings for males (panel A) and females (panel B), starting from five years before potential entry in HVE and ending eleven years after. Except for females in the younger sample, the earnings before entry are lower for treated individuals. The pre-treatment earnings differences are fairly stable until the second year before entry. In the year immediately preceding HVE entry, there is a slight drop in earnings for treated males and females in the older sample. This result may indicate a so-called Ashenfelter's dip, and we will return to this issue in Section 4. All treated groups experience a sharp decline in earnings while enrolled in HVE, males more so than females and older individuals more so than younger individuals. The earnings of the treated tend to have recovered by the fourth year after entry (slightly later for older males) and are higher than the earnings of the untreated for the remainder of the observation window.

Figure 1 Mean annual labor earnings of treated and untreated



4. Method

The effect of HVE enrollment on annual earnings is estimated by using conditional difference-in-differences propensity score matching (DID-PSM) (Heckman et al. 1998). This method extends conventional cross-sectional matching because it not only adjusts

for selection bias due to observable differences between treated and untreated individuals, but also adjusts for bias due to time-invariant unobserved individual heterogeneity affecting earnings. Below, we describe the employed estimator and argue why it is a reasonable estimation strategy in our context.

Let $\Delta Y = Y_{t+} - Y_{t-}$ represent the change in annual earnings when comparing earnings before treatment ($t-$) and after treatment ($t+$). Furthermore, let D be an indicator variable denoting treatment status, and let ΔY_1 and ΔY_0 represent the earnings change in the treated and untreated states, respectively. The average treatment effect on the treated (ATT) is:

$$\Delta Y_{ATT} = E[\Delta Y_1 | D = 1] - E[\Delta Y_0 | D = 1]$$

For each individual, we can only observe ΔY_1 or ΔY_0, but never both. We therefore make the assumption that conditional on a vector of observable pre-treatment covariates, X, we can use the earnings change of untreated individuals as an approximation of the counterfactual earnings change of treated individuals. Formally, we assume $E(\Delta Y_0 | X, D = 1) = E(\Delta Y_0 | X, D = 0)$. Rosenbaum and Rubin (1983) demonstrate that if this assumption holds for X, it also holds for a scalar function of X referred to as the propensity score, $P(X)$, which in our case is a probit estimate of the probability of enrollment in HVE. Estimation of ATT by matching further relies on a common support or overlap condition, formally expressed as $\Pr(D = 1|X) < 1$. This condition prevents X from being a perfect predictor of treatment status, thereby ensuring that all treated individuals have a counterpart in the group of non-treated individuals. This gives the following expression for the estimated ATT:

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

Note that this parameter differs from the parameter estimated using conventional regression under conditional mean independence. While regression typically assumes that the treatment effect is homogeneous, matching estimators consider that treatment effects are heterogeneous by explicitly seeking to compare comparable individuals.

To give $\widehat{\Delta Y_{ATT}}$ a causal interpretation, it is important that we do not condition or match on post-treatment events (Rosenbaum 1984). A further necessary assumption is that HVE enrollment does not affect the earnings of individuals in the comparison group (the stable unit treatment value assumption, SUTVA, or no-interference assumption). Whether this assumption is reasonable depends on the scale of HVE, and we will return to this issue in the Section 6.

The critical assumption in this estimation strategy is that of conditional mean independence. We argue that the richness of the data available for this study contributes to the plausibility of this assumption. The treated and matched comparisons are always balanced in terms of pre-treatment trajectories of annual earnings, days registered as unemployed and with regard to incidence of five different benefits/transfers related to unemployment, labor market programs, parental leave, studies, and social welfare (in all cases up to five years before potential entry in HVE). We also balance on grade point average from compulsory school (for the younger sample), prior education level (4 categories) and educational track (10 categories), parents' educational background (7 categories) and country of birth (4 categories). We further balance on age, marital status

(2 categories), number of children in the household (4 categories), country of birth (9 categories), region of residence (21 categories), and cohort (12 categories). In total, our balancing tests always include more than 85 variables.

A central question is how ability affects selection into education and the returns to education (Card 1999). For the younger sample, the estimation of the propensity score includes the cohort specific rank of the GPA from compulsory school as an indicator of ability.[9] For both the younger and the older samples, we further match on the parents' educational background, which may also pick up some ability related differences. In addition, the pre-treatment trajectories from t-5 of earnings as well as other labor market related variables that are included in the propensity score are also likely to capture differences in ability.

A well-known criticism of difference-in-differences estimators is that a potentially temporary drop in earnings among the treated before program entry (Ashenfelter's dip) may generate an upward bias in estimated effects (Ashenfelter 1978, Heckman and Smith 1999). Therefore, we define the change in annual earnings for any given post-treatment year $(t+)$ as $\Delta Y_{t+} = (Y_{t+} - Y_{t-2})$. That is, in the differencing, we disregard earnings in the immediate year before potential HVE enrollment. For the same reason, in our baseline model, we do not match on earnings or other labor market related variables measured the year before entry in HVE. However, if a decline in earnings before HVE enrollment represents a permanent change in the earnings capacity of the treated, our model will yield downward bias in the estimated effects. In an extended model specification, we therefore expand the set of matching variables to include levels as well as changes in earnings and other labor market related variables the year before potential HVE entry. For transparency, we will report results from both specifications.

By construction, annual earnings are a function of hourly earnings and the number of hours employed during a year. Our outcome measure therefore captures the effects of HVE enrollment on hourly earnings as well as on unemployment and labor supply. In a sense, unemployment and factors influencing labor supply, e.g., investments in education and childrearing, can be regarded as mediators through which annual earnings are partially affected. However, they are also post-treatment events that we must not control for or condition on if we seek to estimate the full causal effect of HVE entry on annual earnings. To learn more about the potential mechanisms underlying the development of annual earnings, we will also focus on the effect of HVE enrollment on employment status and discuss underlying mechanisms behind the results.

Table 4 in the Appendix present probit estimates of the propensity score for males and females in the younger and older sample.[10] As we generate our sample of matched comparisons based on the estimated probabilities, we would also expect all our covariates to be balanced between the treated and the matched comparisons. The results

9 An advantage of using grades from compulsory school as an indicator of ability is that there are only minor curriculum differences at this school level, thus making grades from compulsory school more comparable than, e.g., grades at upper secondary level. The percentiles are defined within each cohort to account for possible grade inflation.

10 Probit estimates based on irrelevant variables may increase bias and/or variance of matching estimators. We therefore exclude variables if associated with p-values above 0.2 or if not essential for the balancing of the samples (de Luna et al. 2011).

from these balancing tests are reported in Table 5 in the Appendix.[11] The tests confirm that equality of means between treated and matched comparisons cannot be rejected.

5. Results

Table 2 present our main results, for the baseline model. These are illustrated in Figure 2 which shows trajectories of the estimated treatment effects for males (panel A) and females (panel B), starting from five years before the first year of registration and ending in the eleventh year after registration. Estimates are presented separately by age groups 19–29 and 30–54, which we will refer to as the younger and the older samples, respectively.[12] There are also two trajectories for each subsample, one where we have accounted for the potential earnings drop prior to enrolment (Ashenfelter's dip) and one where we have not. The latter estimates generally imply a smaller impact, particularly for the older part of our samples, and we focus most of our discussion on these estimates unless stated otherwise.

Table 2 DID propensity score matching estimates on annual earnings

	Males 19–29	%	Females 19–29	%	Males 30–54	%	Females 30–54	%
Yeart+1	-58,652	-34.6	-33,394	-23.0	-80,793	-29.5	-54,927	-26.1
	(623)		(627)		(1,246)		(837)	
Yeart+2	-106,319	-55.1	-70,622	-46.1	-129,791	-45.3	-90,619	-40.9
	(779)		(734)		(1,493)		(960)	
Yeart+3	-31,509	-15.0	6,349	4.0	-54,477	-18.4	-21,483	-9.3
	(871)		(816)		(1,505)		(994)	
Yeart+4	32,282	14.3	51,179	30.3	-3,328	-1.1	14,197	5.9
	(951)		(906)		(1,558)		(1,047)	
Yeart+5	40,821	17.0	43,408	24.2	7,799	2.5	15,822	6.4
	(1,043)		(1,012)		(1,733)		(1,147)	
Yeart+6	41,456	16.1	33,952	17.7	13,433	4.3	16,387	6.4
	(1,151)		(1,115)		(1,900)		(1,246)	
Yeart+7	39,644	14.4	23,082	11.2	14,799	4.6	16,675	6.4
	(1,256)		(1,227)		(2,078)		(1,360)	
Yeart+8	36,146	12.4	16,018	7.4	18,631	5.8	17,456	6.5
	(1,390)		(1,345)		(2,327)		(1,493)	
Yeart+9	33,337	10.8	11,610	5.1	18,167)	5.5	17,813	6.5
	(1,549)		(1,475)		(2,550)		(1,642)	
Yeart+10	29,192	9.0	8,059	3.4	20,047	6.0	16,111	5.8
	(1,758)		(1,610)		(2,902)		(1,848)	
Yeart+11	27,783	8.2	7,172	2.9	20,696	6.1	17,531	6.2
	(2,014)		(1,796)		(3,291)		(2,094)	

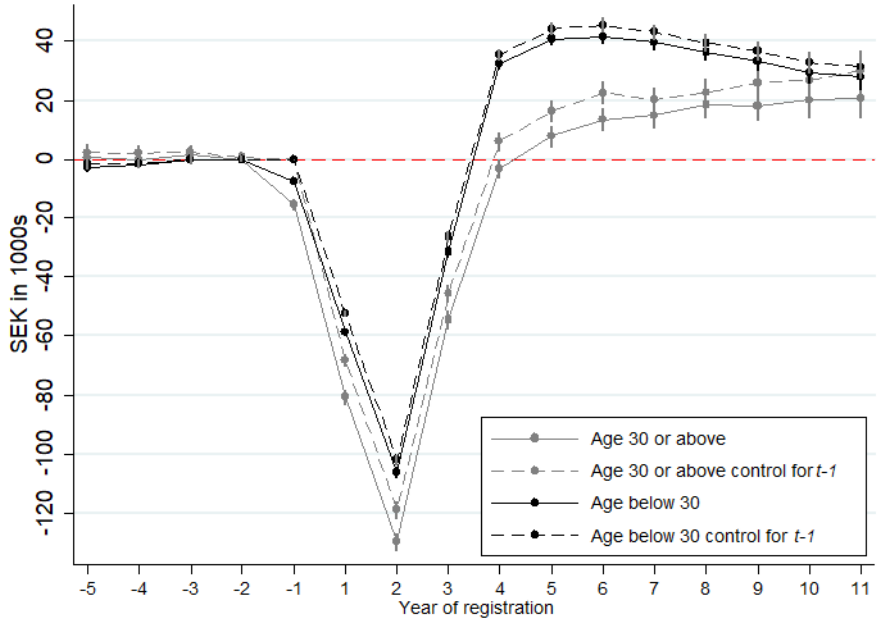
Note: Standard errors in parenthesis.

11 Since we balance on more than 85 variables, we allow for one variable to be unbalanced, and in some rare cases we also allow dummy variables representing less than one percent of the treated to be unbalanced.

12 For completeness, Figure 6 in the Appendix present results separately for more finely grained age groups (reduced model only).

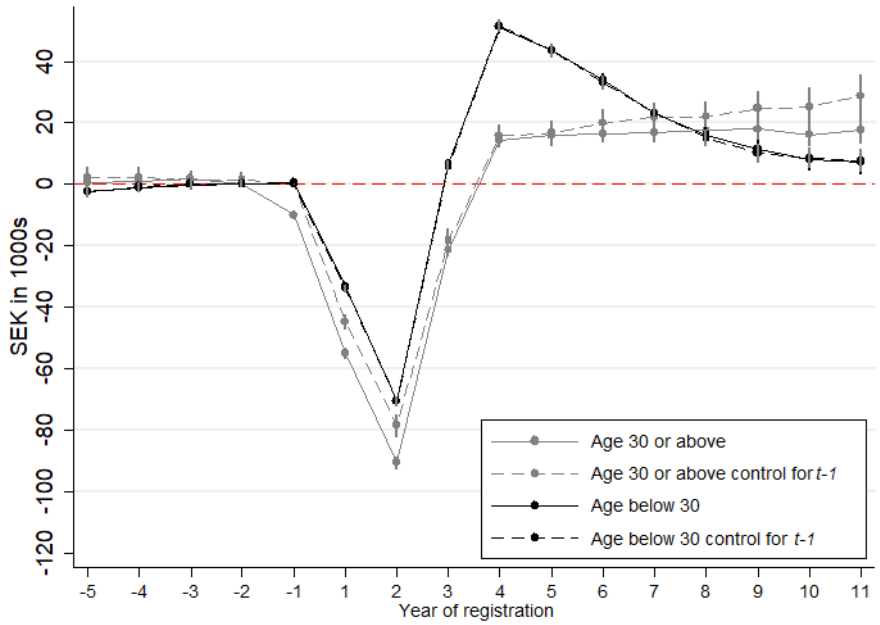
Figure 2 Annual labor earnings – estimated treatment effects

Panel A: Males



Notes: Estimated difference in annual earnings between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for males aged 19–29 is 47,715 treated and 93,333 (weighted) untreated, and for aged 30–54 it is 15,180 treated and 47,785 (weighted) untreated.

Panel B: Females



Notes: Estimated difference in annual earnings between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for females aged 19–29 is 40,756 treated and 79,280 (weighted) untreated, and for aged 30–54 it is 25,768 treated and 72,674 (weighted) untreated.

As expected, estimates before enrolment are balanced between treated and untreated. Then follows three years with negative estimates, i.e., where treated have lower earnings than the matched comparisons because they decrease their labor supply while completing HVE. From the fourth year, estimated treatment effects are typically positive. Below, we discuss these results in more detail.

5.1 Older samples

There are a few features of the results in Figure 2 which are specific to the older samples. First, there are more important differences in estimates when we account for the earnings drop prior to enrolment, reflecting that an earnings drop is a stronger predictor for enrolment among older samples. Once positive estimates have been established, the results are stable. For older males, estimates go from SEK 13,000 (4.2 percent) to SEK 20,000 (6.0 percent) in year eleven. For females, the last seven estimates are even more stable, only varying between SEK 14,200 (5.9 percent) and SEK 17,800 (6.5 percent). Figure 3 indicates that these estimates are not primarily driven by employment, since estimates for males are essentially zero and for females only display a small impact on employment (1 percent).

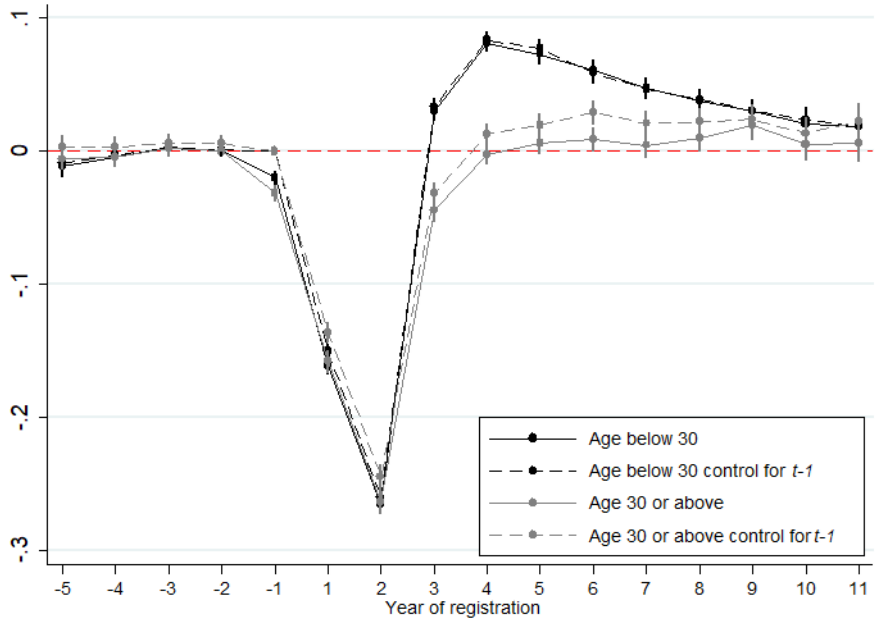
Figure 4 present estimates applying days in unemployment as the difference-in-difference outcome. One may note that days in unemployment is a slightly ambiguous outcome as it conditions on job-search, and therefore also represents an indication of participation in the labor force. For older males, there is some evidence of more days in unemployment among treated. Thus, while the likelihood of being employed is unaffected by HVE among older males, the stints of unemployment among treated are slightly longer (about a week) but these estimates taper off and are close to zero in the last few years. For older females, the estimates roughly line up with our employment estimates, with small differences to the advantage of the treated relative to the matched comparisons.

5.2 Younger samples

For the younger samples of males and females, estimation results indicate a relatively large short-term payoff to HVE. When estimates peak for males (year five) they correspond to 17 percent and for females (year four) to 30 percent of counterfactual annual earnings. These are large estimates, but partly reflect a temporary impact on the probability of employment, which is substantially higher among treated shortly after HVE completion. Figure 3 shows how the employment advantage diminishes over time, from 8 to 2 percent for males and from 10 percent to 1.5 percent in year eleven for females. This is only weakly reflected in the male annual earnings estimates which remain at a reasonably high level, 9 percent in year eleven. In contrast, female estimates drop markedly from the initial 30 percent in year four to 2.9 percent in year eleven.

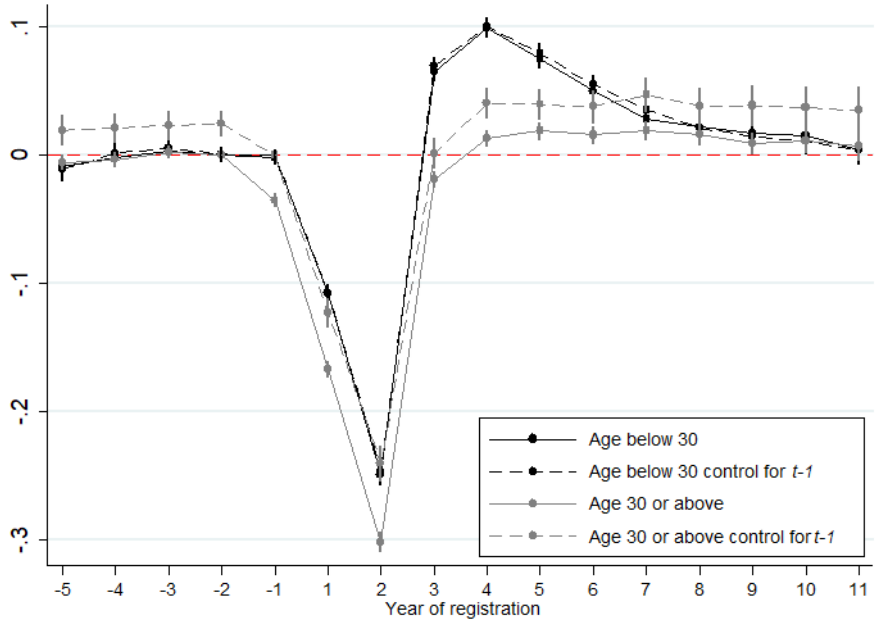
Figure 3 Employment status – estimated treatment effects

Panel A: Males



Notes: Employment status measured each year in November. Estimated difference in employment between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for males aged 19–29 is 47,715 treated and 93,333 (weighted) untreated, and for aged 30–54 it is 15,180 treated and 47,785 (weighted) untreated.

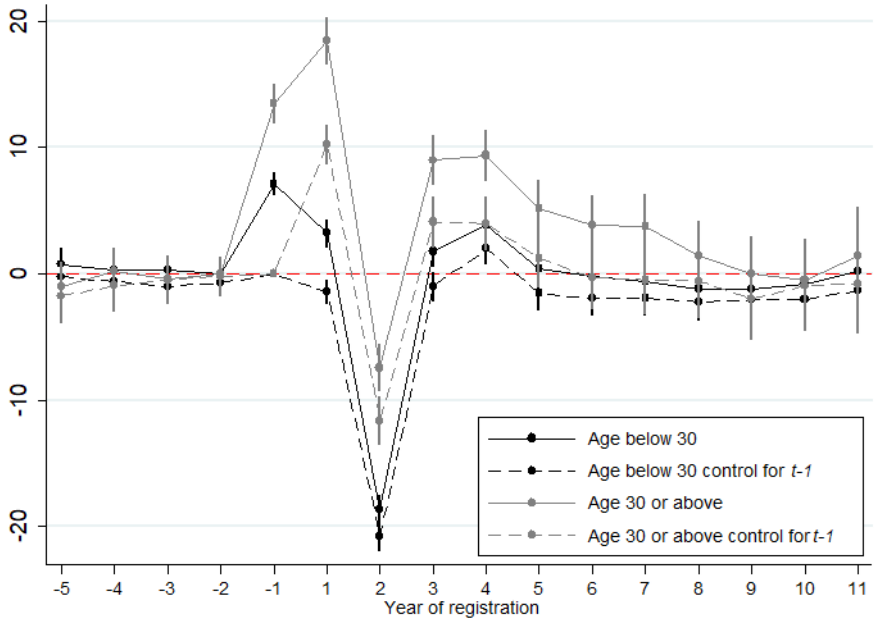
Panel B: Females



Notes: Employment status measured each year in November. Estimated difference in employment between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for females aged 19–29 is 40,756 treated and 79,280 (weighted) untreated, and for aged 30–54 it is 25,768 treated and 72,674 (weighted) untreated.

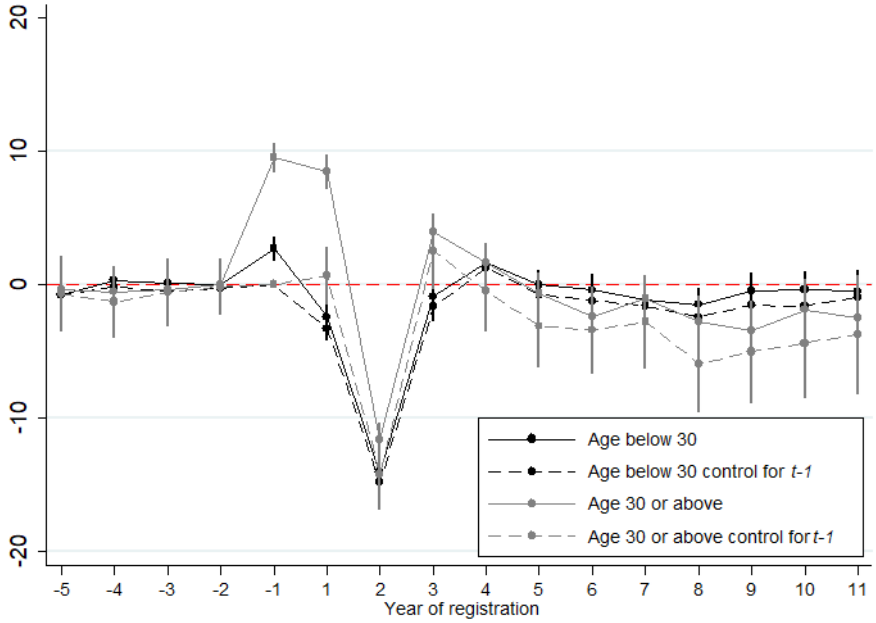
Figure 4 Days in unemployment – estimated treatment effects

Panel A: Males



Notes: Estimated difference in days in unemployment between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for males aged 19–29 is 47,715 treated and 93,333 (weighted) untreated, and for aged 30–54 it is 15,180 treated and 47,785 (weighted) untreated.

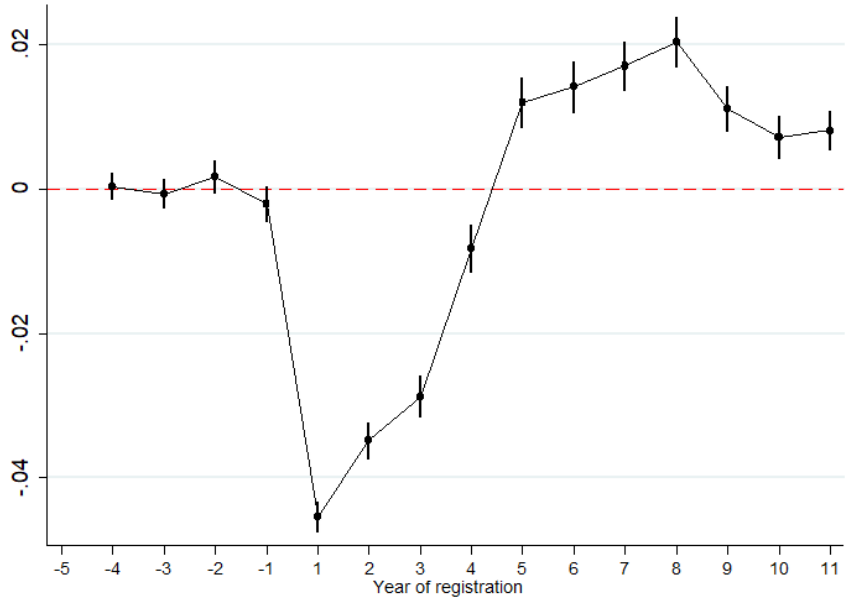
Panel B: Females



Notes: Estimated difference in days in unemployment between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for females aged 19–29 is 40,756 treated and 79,280 (weighted) untreated, and for aged 30–54 it is 25,768 treated and 72,674 (weighted) untreated.

The rather steep drop in earnings estimates of young women merits closer attention.[13] One not too far-fetched hypothesis is that females in HVE might postpone childbearing until after they have completed their education and have established a stable income. There are financial incentives for this behavior since the support received while on parental leave is largely based on the annual earnings of the preceding year. If this is reflected in our annual earnings estimates, we would expect days in unemployment, which condition individuals to be job-seeking, to be relatively unaffected post-treatment. Figure 4 (panel B) shows, in line with our hypothesis, estimates which are close to zero from year four and later years despite that earnings estimates fall noticeably in these years. Furthermore, Figure 5 shows the incidence of a new child aged 0–3 in the woman's household. Although we do not have access to data on actual births, we use this variable as a proxy for fertility decisions. Again, in line with the outlined hypothesis, we find that treated females are less likely to give birth to a child in the first three years after registration, but in the following five years (i.e., coinciding with fall in earnings estimates) the probability of a new child 0–3 years old is higher among treated females. As a rudimentary mediation analysis, we also checked the estimated returns to HVE controlling for the number of children 0–3 years old in the household also post-treatment. Earnings estimates for young females then display less of a drop, and in year eleven the estimate is 95 percent higher compared to Figure 2, increasing from SEK 8,059 to SEK 15,697. This indicates that fertility patterns are likely to explain part of the dynamics in annual earnings estimates for young females in Figure 2.

Figure 5 Females 19–29 – estimated treatment effects on the incidence of new child 0–3 years old



Notes: Estimated first difference in the incidence of a new child aged 0–3 in the woman's household between treated and matched comparisons, base year is t-2 prior to first year of registration (1). Estimates presented starting from four years prior to first registration (-4) to year 11 after first registration. N is 40,756 treated and 79,280 (weighted) untreated.

13 As for males, there is a bit of a paradox in the results shown in Figure 3 and Figure 4, implying no difference in the number of days in unemployment despite a higher probability of employment for treated young males. This may be explained by that matched untreated from year four to year seven have more often applied for and received study allowances following registering in education. This likely makes some of them neither employed nor unemployed.

6. Cost-Benefit-Analysis

A key question in any evaluation study is whether the estimated results imply that the costs to society are covered by the benefits. Unfortunately, cost-benefit-analysis (CBA) involve formidable empirical challenges and relies on a number of untestable assumptions. Although this makes CBA calculations easy to criticize, they are still necessary for any sort of wider discussion about the assessment of HVE and its cost-effectiveness and, ultimately, its viability as an educational policy. For this reason, we calculate the internal rate of return by setting a number of baseline assumptions that we believe are conservative. This is followed by complementary calculations where we introduce more optimistic assumptions.

The internal rate of return is defined as the discount rate which sets to the present value of costs and future benefits equal. Our baseline assumptions are the following; *i*) the benefits are equal to baseline model point estimates, and the average estimate in years 9 to 11 is extrapolated to all years until the average age of the sample reaches retirement age; *ii*) the estimated earnings return reflects an increase in production, with no crowding out effects (Dahlberg and Forslund, 2005); *iii*) the retirement age is assumed at 63; *iv*) the deadweight loss is 30 percent, i.e., program costs are multiplied by 1.3; *v*) public insurance payments such as UI, sick-leave or social welfare are not affected by HVE; *vi*) foregone earnings are calculated as the negative gap between earnings trajectories observed from year 1 in Figure 2; *vii*) there are no positive externalities of HVE; *viii*) both foregone earnings and positive earnings returns are multiplied by 1.4 to account for payroll taxes (approximately 40 percent) and to provide a proxy measure of a production value; and *ix*) the cost per student for one year of HVE is set to the average reported government pay per student, which is SEK 64,700.[14]

Each of these assumptions may be debated, but we would argue that the calculations are overall on the conservative side. First, the foregone production value (assumption *vii*) is the largest cost incurred for society by the HVE. However, it is likely to be lower for society than the foregone production of the individual. If colleagues work extra hours to compensate for the vacancy of the treated individual, or if a non-employed individual fills the vacant slot, the loss in production is partly reduced.[15] As an alternative, we therefore assume that 35 percent of forgone production is compensated for by colleagues or non-employed individuals. Second, because firm-owners and employees negotiate wages, an increase in productivity is unlikely to be fully captured by increases in annual earnings. In Sweden in the 2000s, about 60 percent of the yearly value added has fallen to wage earners and the remaining part to firm-owners. To get closer to an appropriate estimate of the value added, it would imply that our earnings measure should be multiplied by a factor 1.66 (we apply 1.5). Third, no external effects of HVE (assumption *vii*) may underestimate the social returns to HVE. The economics literature on

14 HVE homepage https://www.myh.se/Statistik/Yrkeshogskoleutbildningar/Utbetalda-statliga-medel-/

15 The stable unit treatment value assumption (SUTVA) is that non-participants are completely unaffected by the HVE. This is a very strong assumption, but it implies that the earnings loss of the individual is the appropriate measure of foregone earnings to society. The opposite is to assume that all vacancies are replaced by non-employed, resulting in zero foregone production (Johnson and Layard 1986). Stenberg and Westerlund (2016) set the upper bound of the probability of a non-employed filling the vacancy equal to the employment rate (0.70).

externalities suggests multiple channels, e.g., positive effects on productivity of colleagues, on economic growth and on non-pecuniary factors such as health, criminality, equity or democracy. The suggested multipliers for some of these factors in isolation have roughly been 1.3, sometimes lower and sometimes higher and there is overall no consensus in the literature. [16] Fourth, we also allow the estimated benefits of HVE to be based on models where we account for earnings in the year before first registration (see discussion in Section 4).

In Table 3, we present the calculated internal rate of returns. These are positive throughout when applying our most conservative "baseline" assumptions, and even for the older sample in isolation. As we add some of the more optimistic assumptions, these are one-by-one not too influential, but collectively they make a large difference to the calculations. For the overall sample, the calculated internal rate of return increases from 6.7 percent to 17.0 percent. The younger sample estimates are more influential for the overall assessment, both because they represent the majority of participants (70 percent) and because they remain in the labor market for about another thirty years outside our observation window.

Table 3 Cost-benefit-calculations of the internal rate of return under different assumptions

	Total	Aged 19–29	Aged 30–54
Baseline	**6.7%**	**8.8%**	**2.2%**
Add assumptions:			
Value added profits 50%	8.0%	10.5%	3.1%
Account for t-1 earnings	8.5%	9.9%	5.8%
Opportunity costs 0.65	9.2%	11.5%	4.4%
External effects 30%	9.4%	11.9%	4.3%
All of the above	17.0%	19.5%	12.5%

7. Concluding discussion

The main contribution of this paper is that we have analyzed the impact on labor earnings of Higher Vocational Education (*Yrkeshögskola*) and find strong indications of positive effects. Positive outcomes of such a large-scale program could serve to encourage countries to gradually increase human capital investments in adult workers. The emphasis on vocational skills may also make HVE interesting for policy makers in other countries as it potentially has short term gains, and thereby constitute less of a political risk if participants' payoff is possible to observe a year or two after program completion.

16 Some studies report a productivity multiplier of 1.5 (Moretti 2004, Kirby and Riley 2008) whereas others report no or small effects (Acemoglu and Angrist, Ciccone and Peri 2005, Isacsson 2005). There are also studies arguing in favor of non-pecuniary outcomes and economic growth (Dee 2004, Gradstein and Justman 2002, Green et al. 2006, Putnam et al. 2001, Milligan et al. 2004) whereas the economics literature on health outcomes is mixed, with some studies claiming that education has spillovers on health (Buckles et al. 2013, Lager and Torssander 2012, Lleras-Muney 2005, Arendt 2005, Ricci and Zachariadis 2013) while others claim no or small effects on health (Clark and Royer 2013, Mazumder 2012, Meghir et al. 2018). Results are more coherent concerning effects on reduced criminality (Lochner and Moretti 2004, Machin et al. 2011, Hjalmarson et al. 2011).

The main reservation we have is related to that the earnings returns decrease for the younger samples aged 19–29 at the time of program entry. While estimates peak at 30 percent for females and 17 percent for males but drop to 2.9 and 9 percent respectively. Regarding the modest results for young females, our conjecture is that the pattern is related to that young women, following HVE, have incentives to establish themselves on the labor market before childbirth. Nevertheless, the relatively modest estimates for the youngest group of females are a little surprising since most earlier studies of adult education have reported larger estimates for females than for males.

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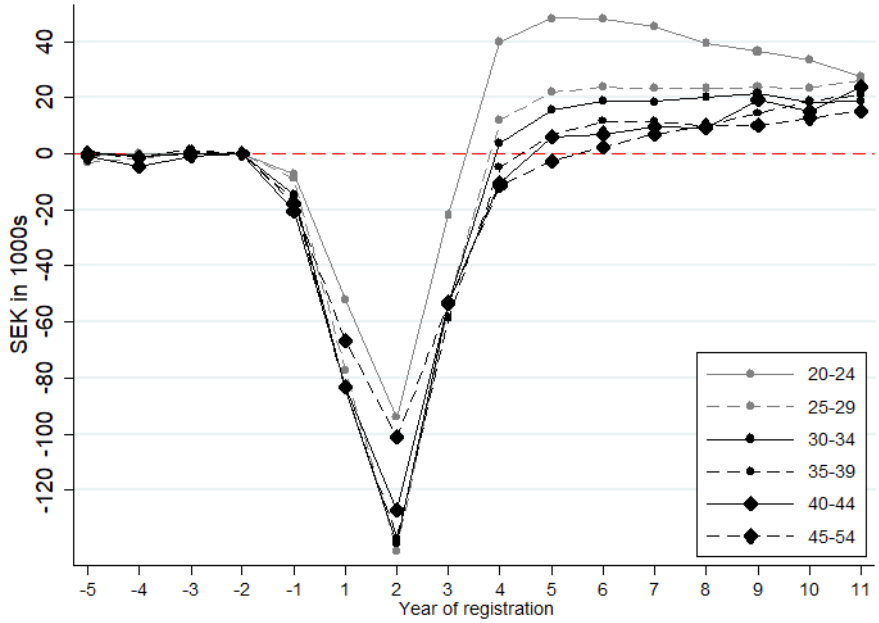
Appendix

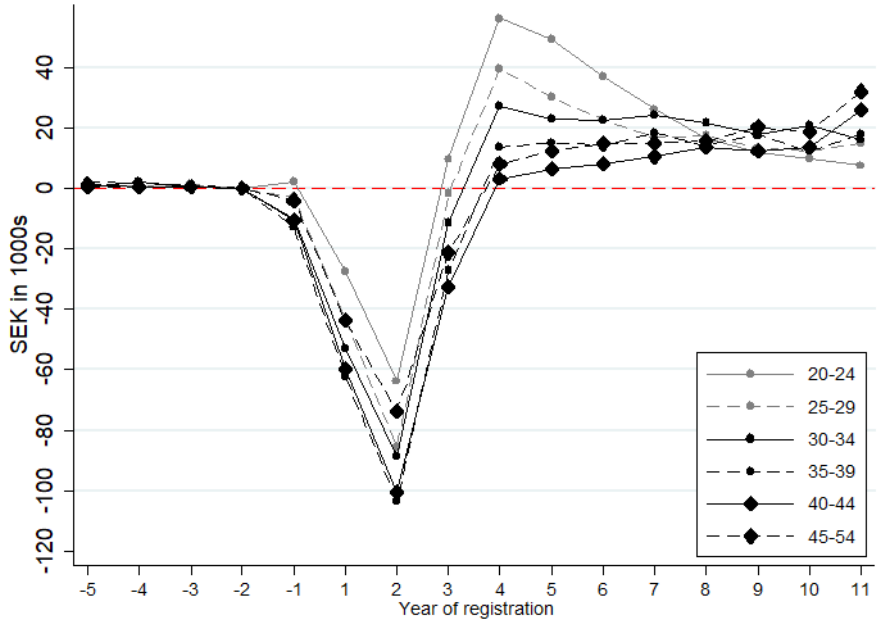
Figure 6 Annual labor earnings – estimated treatment effects for different age groups

Panel A: Males



Notes: Estimated difference in annual earnings between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for males 34,429 treated (aged 20–24); 13,306 (aged 25–29); 6,257 (30–34); 3,922 (35–39); 2,711 (40–44) and 2,290 (45–54).

Panel B: Females



Notes: Estimated difference in annual earnings between treated and matched comparisons, base year is t-1 or t-2 prior to first year of registration (1). Estimates presented starting from five years prior to first registration (-5) to year 11 after first registration. N for females 27,904 treated (aged 20–24); 12,865 (aged 25–29); 7,879 (30–34); 6,650 (35–39); 5,610 (40–44) and 5,614 (45–54).

Table 4 Probit results applied for propensity score

	Males 19–29	Females 19–29	Males 30–54	Females 30–54
Income t-2	-.000***	.001***	-.000***	.000
	(.000)	(.000)	(.000)	(.000)
Unemp t-2	.048***	.080***	.188***	.169***
	(.016)	(.014)	(.016)	(.013)
Study allowance t-3	.105***	-.021*	.256***	.204***
	(.011)	(.011)	(.028)	(.016)
Study allowance t-2	.119***	.001	.139***	
	(.013)	(.011)	(.031)	
Study allowance t-1	.530***	.465***	-.057	-.012
	(.016)	(.018)	(.149)	(.094)
Unemployed t-3	.047***		.040**	.032**
	(.016)		(.018)	(.014)
Labor market prog t-2	-.025	-.032		
	(.018)	(.020)		
Parental leave t-3	-.044	.129***	.057***	.083***
	(.036)	(.020)	(.013)	(.011)
Days unemp t-2	.000**	.000**		
	(.000)	(.000)		
Social welfare t-3	-.081***	-.111***	-.158***	-.147***
	(.018)	(.018)	(.027)	(.022)
Social welfare t-2	-.147***	-.115***	-.083***	-.093***
	(.018)	(.019)	(.027)	(.023)
GPA at age 16	.034***	.014***		
	(.001)	(.000)		
9 years of schooling	-.669***	-.945***	-.691***	-.821***
	(.019)	(.021)	(.017)	(.017)
11 years of schooling	-.505***	-.673***	-.271***	-.308***
	(.030)	(.033)	(.019)	(.014)
12 years of schooling	-.507***	-.713***	-.190***	-.249***
	(.022)	(.026)	(.011)	(.009)
No field stated	.056**	.061**	.225***	.224***
	(.022)	(.025)	(.022)	(.021)
Pedagogics	.632	-.235	-.033	-.392***
	(.486)	(.384)	(.144)	(.111)
Humanities	.106***	.088***	.181***	.222***
	(.024)	(.026)	(.030)	(.026)
Social science	.079***	.112***	.074***	.042**
	(.027)	(.028)	(.023)	(.021)
Natural science	.471***	.314***	.282***	.077*
	(.053)	(.107)	(.043)	(.041)
Engineering	.033	.036	.025	.089***
	(.022)	(.031)	(.020)	(.026)
Farming/forestry	-.070**	.034	-.091***	.198***
	(.033)	(.030)	(.033)	(.031)
Health	-.097***	-.214***	.159***	.183***
	(.029)	(.027)	(.030)	(.021)
Services	-.264***	-.108***	-.043*	.061***
	(.026)	(.028)	(.025)	(.022)
Age		-.008***	-.039***	
		(.002)	(.001)	
Married/cohabit	-.151***	-.370***		-.041***
	(.032)	(.028)		(.009)

Parental leave t-1	.070**	.102***	.063***	.078***
	(.030)	(.025)	(.014)	(.012)
Child 0-3	-.185***	-.229***	-.076***	.027**
	(.038)	(.034)	(.012)	(.013)
Parent with high school	.178***	.127***	.115***	.105***
	(.013)	(.016)	(.015)	(.012)
Parent with some college	.250***	.169***	.186***	.153***
	(.013)	(.017)	(.015)	(.014)
Income t-3		.000***		
		(.000)		
Child 7-15		.056	-.012	.021**
		(.038)	(.010)	(.008)
Child 16 or older		-.280	-.017	.059***
		(.202)	(.014)	(.009)
Income t-5			-.000	.000*
			(.000)	(.000)
Days unemp t-4			.000	
			(.000)	
Unemp t-5			.102***	.076***
			(.014)	(.011)
Days unemp t-3			-.000***	-.000*
			(.000)	(.000)
Labor market prog t-3			.081***	.052***
			(.023)	(.020)
Parental leave t-2			.044***	.070***
			(.014)	(.013)
Child 4-6			-.042***	.042***
			(.013)	(.011)
Days unemp t-5				-.000**
				(.000)
N	205,603	172,609	342,258	292,991

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. See text for choice of explanatory variables. All regressions include a constant term and (when relevant) age-dummies, annual earnings of selected years from t-5 or later, dummies for region of residence (22), and cohort (9). Complete results are available on request.

Table 5 Balancing tests, descriptive mean statistics between treated and matched comparisons (selected variables)

	Females				**Males**			
	Below 30 years old		30 years or older		Below 30 years old		30 years or older	
	Treated	Matched comp.	Treated	Matched comp.	Treated	Matched comp.	Treated	Matched comp.
Age	23.19	23.20	38.98	38.95	22.71	22.73	37.36	37.32
Married/cohabit	0.09	0.09	0.64	0.64	0.03	0.03	0.49	0.49
Child at home	0.10	0.11	0.77	0.77	0.02	0.02	0.48	0.48
Years of schooling	11.61	11.61	11.50	11.51	11.36	11.36	11.39	11.40
GPA at age 16	53.89	53.90	-	-	43.44	43.62	-	-
Days unemp t-3	21.18	20.68	30.65	30.98	23.10	23.11	38.13	38.82
Days unemp t-2	24.78	24.43	31.46	31.43	28.49	28.82	39.49	39.79
Days unemp t-1	30.24	27.18	39.08	29.49	39.36	32.55	49.45	36.23
Income t-2	115.46	114.80	188.48	190.07	114.61	114.45	251.49	250.73
Study allowance t-1	0.38	0.39	0.12	0.12	0.44	0.45	0.08	0.08
Born in Sweden	0.93	0.93	0.88	0.88	0.92	0.92	0.86	0.86
Parent years of schooling	12.92	12.93	11.65	11.67	13.25	13.27	12.10	12.11
Metropolitan area	0.56	0.56	0.54	0.53	0.54	0.54	0.56	0.55
Observations	40,769	79,280	25,775	72,674	47,735	93,333	15,186	47,785

På vilket sätt statens insatser bidrar till svensk tillväxt och näringslivsutveckling står i fokus för våra rapporter.

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