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ABSTRACT

The Long-term Earnings Consequences of General vs. Specific Training of the Unemployed*

Training programs for the unemployed typically involve teaching specific skills in demand amongst employers. In 1997, Swedish unemployed could also choose general training at the upper secondary school level. Despite the dominance of programs offering specific training, long-term relative earnings effects of general vs. specific training are theoretically ambiguous. Analyzing detailed administrative data 1990-2010, we find specific training associated with higher earnings in the short run, but that earnings converge over time. Results also indicate that individuals act on their comparative advantages. Long-run earnings advantages of general training are found for females with limited prior education and among metropolitan residents.

JEL Classification: I21, J62, J68

Keywords: active labor market programs, adult education, vocational training

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1 Introduction

Governments in most OECD countries offer training programs for the unemployed, typically oriented toward vocational/specific skills. The consensus view seems to be that vocational/specific training is a more efficient measure for unemployed individuals than are courses providing general/theoretical skills. In the short run, learning a branch specific skill is presumed to better enhance re-entry into employment. General training, without an obvious connection to a labor market branch, may have less of an impact. However, in the long run, if general skills increase the ability to learn new tasks, this could make workers less sensitive to changes in the demand for skills. Earlier studies of adults in general education have reported average earnings returns which still increase eight to ten years after enrolment (Jacobson et al. 2003, 2005, Stenberg 2011; see Figures 1a and 1b). As program effects vary between individuals and over time, these estimates are not directly comparable with evaluations of vocational training programs, but they raise the question of whether the long-term effects of general training would catch up with or exceed the earnings effects of specific training. [1] Some economists have suggested that governments should stimulate adults to enroll in formal schooling during economic downturns (e.g., Heckman and Urzua 2008, Pissarides 2011), but there is an almost complete lack of empirical research on this topic. It is therefore unclear whether skill adjustments among the unemployed should involve a larger element of general training.[2]

[1] The results from evaluations of specific training for the unemployed in Sweden have differed across decades, with positive effects in the 1980s, zero or negative effects for participants at the start of the 1990s, and positive effects again in the late 1990s and early 2000s (e.g., Andrén and Gustafsson 2005, Calmfors et al. 2001, Axelsson and Westerlund 2005, Stenberg and Westerlund 2004, de Luna et al. 2008). The restrained results at the start of the 1990s have usually been ascribed to the economic recession's effect on employment prospects and/or the large scale of labor market training programs at the time.

[2] A few studies compare the economic efficiency between other training programs, job search assistance, public employment, and/or wage subsidies (Lechner et al. 2011, Kluve 2010, Card et al. 2010; for Sweden, see Forslund, Fredriksson and Vikström 2011 and Forslund, Liljeberg and von Trott zu Solz 2013).

The purpose of this article is to evaluate the relative earnings association of general versus specific training for the unemployed. In the spring of 1997, the Swedish government announced the Adult Education Initiative (AEI henceforth) which targeted the same groups of the unemployed as did the traditional vocational/specific training program. The AEI enabled unemployed adults aged 25-55 to attend a year of full-time schooling at the upper secondary level, with financial support equal to a maintenance of unemployment benefits. AEI started in August 1997 and attracted large numbers. We study a sample comprising the unemployed individuals who enrolled in 1997 in either the AEI or the largest vocational training program in Sweden (*Arbetsmarknadsutbildning*), which we will refer to as "Labor Market Training" (LMT).

We explore exceptionally rich population register data which includes annual earnings from 1990 until 2010, providing a follow-up period of 13 years. Our descriptive average earnings trajectories already represent an interesting contribution, as we are not aware of any analysis of this length of time for general vs. specific labor market programs. To move closer to a causal interpretation, the empirical strategy is based on difference-in-differences propensity score matching, which explicitly takes into account heterogeneous treatment effects and individual time invariant (fixed) unobserved characteristics. The evaluated samples are balanced on more than 100 covariates and our findings are overall robust, e.g., when we check for potential bias by including measures of cognitive and non-cognitive skills (males born 1953 or later) and for "parallel trends" by controlling for dynamic factors (changes) prior to program enrolment. The results obtained are, as expected, more sensitive to the length of the follow-up period. In addition, the expansion of the menu of programs may enhance efficiency to the extent that individuals act on their comparative advantages in practical/theoretical skills. This is possible to examine as propensity score matching accounts for individuals' heterogeneity,

and we find that results are also sensitive to the assumed counterfactual state, LMT versus the AEI. This point is discussed in Section 5.3 and the presented results include both cases.

Research comparing general and specific training for the unemployed is scant. Stenberg (2007) is a study similar to the present one, but it analyzes only the short-run annual earnings effects of the AEI and LMT (six years post-enrolment). The results were obtained with individual fixed effects estimates, i.e., basically relying on earnings and age as control variables. They corroborate the consensus view regarding short-term outcomes as the LMT individuals' earnings exceeded those of participants in the AEI by approximately € 3,500 for males and by €1,500 for females. The descriptive statistics in Figure 2(a) and 2(b) demonstrate the earnings trajectories from raw data for 1991-2003.

The main contribution of this study is the estimation of the long-term relative earnings impact of general versus specific training of the unemployed 13 years post enrolment. The length of the observation window makes it possible to examine if the earlier reported short-term earnings advantage of LMT remains over time, whether trends converge or whether the long-term earnings are more in favor of general training. Because general training is rarely provided for the unemployed, a long-term relative earnings advantage of the AEI would potentially support an expansion of active labor market programs, by allowing individuals to choose the program type in accordance with their comparative advantages. A second contribution of this study is that we allow estimates to vary according to individuals' comparative advantages. This is achieved by considering heterogeneous program effects and by interchangeably modeling the counterfactual state as LMT or the AEI. The results indicate that specific training outperforms general training in the short run (5-7 years). In the longer perspective, 7-13 years after program enrolment, the estimates tend to converge toward zero. The analyses indicate evidence consistent with individuals acting on their comparative

advantages. Results pertaining to subgroups also reveal substantial heterogeneity and imply scope for efficiency gains by expanding labor market programs to include general training of the unemployed. This is particularly true for females with limited education and may also apply to residents in a metropolitan labor market region (Stockholm). In separate analyses, there are indications that vocational training may be a way to compensate for low levels of non-cognitive skills or, conversely, that non-cognitive skills are an important complement to skills obtained in general training.

2 Earnings returns to specific and general human capital

The distinction between specific and general skills made by Becker (1964) has often been used to formulate hypotheses on differences in expected short-term and long-term labor market outcomes (e.g. Brunello 2003, Hanushek et al. 2011, Kreuger and Kumar 2004a, 2004b, Shavit and Müller 1998). In the short run, specific skills are assumed to be instantly in demand in the labor market, and to yield short-term average earnings returns which exceed those of general skills. General skills instead enhance the ability to learn, at the expense of a more sluggish transition from training into employment. While these are stylized characterizations, they fit with the trajectories presented in Stenberg (2007) and reproduced here as Figure 2.

In a longer perspective, business cycle fluctuations and technological changes may influence the relative payoff of the different types of human capital. First, by definition, the degree of transferability between employers is lower for specific skills. If the business cycle generates structural changes which force individuals to switch careers, there is a risk attached to investments in specific skills. Relatedly, technological changes could create an advantage for general skills if they enhance the ability to learn new skills. Employers could be more likely to offer further training to these individuals, who then become even less sensitive to changes. In

sum, the long-run relative earnings implications are ambiguous, and the time frame emerges as an important aspect to appropriately analyze the impact of general vs. specific skills.

We expect individuals' comparative advantages to affect the choice of investment in specific or general human capital. From this follows two crucial implications. On the one hand, labor market efficiency and societal benefit may be enhanced when program options are increased. On the other hand, it also implies that program types may attract individuals with different characteristics. The latter potentially (but not necessarily) constitutes a source of endogeneity bias in our estimates. The empirical approach to take this into account is explained in Section 5.3.

3 Institutional setting

In Sweden, compulsory (comprehensive) school is nine years, with very limited tracking. This is followed by two- or three-year programs at the upper secondary school. The two-year programs are mainly vocational, but also encompass business, social science and technology. The three-year programs are all theoretical and are intended to provide eligibility for higher studies.

A notable characteristic of the Swedish educational system is the prevalence of adults in formal education. Since 1969, Swedish municipalities have been obliged by law to offer schooling to adults who wish to re-enroll at the lower (compulsory) or upper secondary level. The courses offered are primarily theoretical, with only a limited supply of vocational courses, and are provided by institutes known as *Komvux*. Participants at Komvux are aged 20 years or older and may be those who dropped out of compulsory school or upper secondary programs. Compared with continental Europe, there is a relatively modest gap in the educational content

between the vocational and theoretical programs at the upper secondary level. Many individuals therefore enroll in Komvux to change the direction of their studies. Others enroll to complete a three-year upper secondary diploma and/or to improve grades, potentially to qualify for higher education. Those registered in Komvux are eligible to apply for study allowances that amount to about €1,000 per month (2010 values) of which two-thirds is a loan to be repaid over 25 years.

Vocational courses for adults are mainly offered as active labor market programs. The content of the LMT is typically highly varied, with the five largest sectors represented being technology and science, health care, administration, manufacturing and service (AMS 1999). Importantly, prior to the early 1990s, Komvux enrolment was rarely offered to unemployed individuals. This is partly explained by the fact that UI benefits are more generous than are study allowances (and do not require repayment) and that this would have generated incentives for individuals to register as unemployed before enrolling in Komvux.

Figure 3 shows historical data of the numbers unemployed who were registered in Komvux and LMT. At the start of the 1990s, following an extreme recession which saw unemployment increase from 2 percent to 11 percent, the unemployed were assigned to LMT, which then grew to its largest size to date. From 1993, as the levels of open unemployment did not decrease in any significant way, the government offered municipalities funding of slots in Komvux, reserved for the unemployed. These funds gradually increased, and the proportion of the unemployed in Komvux was approximately 10-20 percent in 1993-1996 (Stenberg 2011). The Adult Education Initiative (AEI) was launched in 1997. The government then more than doubled the number of slots in Komvux earmarked for the unemployed and offered one year of full time studies in Komvux with a special grant for education and training (UBS, *särskilt utbildningsbidrag*), equal to the level of the individual's UI benefits. The AEI instantly became

the largest active labor market program, with the participants representing 1.2 percent of the labor force.

The LMT and AEI partly targeted the same groups of the unemployed and prioritized those individuals in a weak position in the labor market. The choice of program was a joint decision between the individual and a case worker at the employment office, with the preferred program usually available if individuals met the formal criteria of being 25-55 years old and eligible for UI benefits. The financial support for the participants in each program was equal to the level of the individuals' UI benefits, and a six-month training period in either program qualified the individual for a new 300-day benefit period. The average program duration in the LMT was 141 days. AEI participants were offered one year of full time studies, but enrollees in 1997 were offered a prolonged special grant for education and training (equal to their UI) for the school year 1998-1999, which approximately 35 percent of the individuals accepted. The costs of each type of program were reported as SEK 85,000 (1 SEK≈.11 €) per year for the LMT and SEK 34,000 per year for the AEI. This would correspond to similar costs per participant. To simplify the analysis, we will disregard the direct program costs when assessing the relative payoff of the programs.[3]

4 Data

This study is based on annual population register data for 1990-2010, which encompasses all individuals residing in Sweden. To define our samples, the unemployment registers provide information on the day of enrolment in the LMT and the end date of this registration. We

[3] The average costs of the LMT would be SEK 33,300 [(141/360)*85000] compared with SEK 45,900 for the AEI if one assumes 1.35 years in Komvux on average. Our decision to disregard the differences is based on the fact that drop-outs complicate this calculation (completion rates at Komvux are below 60 percent), as does the fact that vocational programs vary greatly in their costs and we do not have access to information at the individual level. The implications of our estimates in the empirical section must be considered with this reservation about the costs in mind.

define the LMT participants as those enrolled in May or later in 1997, to make the timing of the programs reasonably similar. The courses at Komvux are usually ongoing from the end of August until December (autumn semester) and/or from January until the beginning of June (spring semester). For those enrolling in the AEI, we set the twofold condition that individuals were registered in Komvux in the autumn semester of 1997 and that they received the special grant for education and training (*Särskilt utbildningsbidrag*, UBS) that was introduced in 1997 specifically for the AEI. This helps us distinguish between participants in the AEI and participants in the regular Komvux program, who attended the same courses (and in the same classrooms). Excluding the individuals registered in both LMT and AEI in 1997, and those attending vocational courses within the AEI, the numbers registered in programs were 40,835 (LMT) and 46,227 (AEI). For our analyses, we exclude individuals who were registered in any of the two programs in 1996. We also set the condition that the individuals were aged 25-55 in 1997, received UI benefits and were registered as unemployed for at least one day between the 1st of January and the 30th of June. With these restrictions, the sample size is 15,129 (LMT) and 16,099 (AEI). This is our benchmark sample used in the analyses presented.

Figure 4 displays the trajectories of the AEI and LMT participants' annual earnings for 1990-2010. There is remarkable similarity in earnings between the two groups for 1990-1996, which is mainly an effect of conditioning on the incidence of the UI benefits in 1997. At face value, the earnings of males after enrolment indicate an advantage of the LMT, but the general training appears to be more beneficial for females. To the best of our knowledge, this kind of descriptive evidence has not been presented earlier.

Table 1 presents means of selected variables. Many of the characteristics are significantly different between the two groups (*p*-values <.05). Participants in AEI are on average about 2.5 years younger, have completed fewer years of schooling and are more often

employed in the public sector.[4] Among females, the AEI enrollees were more often on maternal leave and had more children at home than the LMT participants. Concerning unemployment history, the differences between the groups are relatively modest, although statistically significant. Table 2 describes the schooling completed until 2004 by participants in the AEI. For males born in 1953 or later, we also have information on test scores of cognitive and non-cognitive skills from the mandatory military enlistment, completed at age 18-19 (scaling 1-9, where 9 is the best). The conventional view is that general training attracts individuals with higher ability, but the difference in cognitive test scores is small (4.34 vs. 4.31) and not statistically significant (*p*-value .530). The average score for non-cognitive skills is marginally higher for the LMT sample (4.23 vs. 4.31, *p*-value .034).[5]

In the results section, we perform robustness checks based on "limited samples", restricted to those never registered in either program in 1991-1996 (our earliest record of LMT is 1991). This increases the comparability and decreases the risk that estimated program effects are diluted, but at the cost of external validity. The remaining number of observations is then 7,153 (LMT) and 8,324 (AEI). Table A.1 in the Appendix gives the descriptive statistics.

5 Empirical strategy

To assess the relative earnings impact of the AEI and LMT, we use difference-in-differences propensity score matching (PSM) to compare comparable individuals and take into account that treatment effects are heterogeneous. Below, we describe our relative average treatment

[4] About 14 percent of enrollees in AEI had completed at least a three-year upper secondary school program. As explained in Section 3, enrolment may be motivated by individuals' desires to redirect their studies or improve their grades. They may also have a diploma obtained in a foreign country.

[5] Cognitive skills are based on test scores of inductive, verbal, technical, and spatial skills. Non-cognitive scores are determined by a certified psychologist and measure social skills, leadership qualities, emotional stability and persistence. The scores are available for a subsample of 97,027 males born 1953 or later.

effect on the treated (ATT) of the AEI and LMT, taking a conventional ATT estimator as a point of departure. The interpretation of the relative ATT estimates is discussed in Section 5.3.

5.1 Difference-in-differences propensity score matching

In our empirical implementation, year *t* is 1997 and *t+* is (1998, 1999, …, 2010). If a program occurs at time *t*, the change in annual earnings ($Y_{t+} - Y_{t-}$) = ΔY is calculated for each individual. In a potential outcomes framework, we wish to compare ($\Delta Y_1 - \Delta Y_0$), where subscripts denote 1 if treated and 0 if untreated (for now). One of these is always missing. We therefore make the assumption that conditional on individuals' pre-program observable characteristics X, and denoting D = 1 for actual treatment and zero otherwise:

$$\left(\Delta \mathrm{Y}_1 - \Delta \mathrm{Y}_0\right) \perp \mathrm{D} \mid \mathrm{X}.$$

If this assumption holds, it also holds for some function of X, such that the matching is reduced to conditioning on a scalar (Rosenbaum and Rubin, 1983):

$$\left(\Delta \mathrm{Y}_1 - \Delta \mathrm{Y}_0\right) \perp \mathrm{D} \mid \mathrm{P(X)}$$

The function P(X) is the propensity score, in our case a probit estimate of the probability of enrolment in a program. Each treated is matched with an untreated who is the nearest neighbor in terms of the probit estimate. Because ΔY_0 cannot be observed for treated individuals (D = 1), it is estimated by the observed outcomes of the matched comparisons. Under assumptions *i*) – *iii*) given below, the ATT is then the average of ($\Delta Y_1 - \Delta Y_0$) for samples which have been balanced on the covariates. Formally:

$$\Delta \mathrm{Y}_{\mathrm{ATT}} = \left(\Delta \mathrm{Y}_1 \mid \mathrm{D} = 1, \mathrm{P(X)}\right) - \left(\Delta \mathrm{Y}_0 \mid \mathrm{D} = 0, \mathrm{P(X)}\right)$$

Program effects are likely to be heterogeneous. It means that separate estimates of ATT for two programs are not necessarily comparable (i.e. ATT may be different from the average treatment effect, ATE). To directly compare AEI and LMT, one may estimate a relative ATT by applying the same reasoning as in the case of the ATT discussed above, but consider D = 1 the treatment and D = 0 the alternative treatment (instead of "no treatment"). We thereby obtain an estimate of relative program effects for comparable program participants. To give a hypothetical example, if the program effects are correlated with say, age, separate estimates of ATT for the AEI and the LMT may differ only because of participants' different age structure. The relative ATT would correct this potential flaw by comparing ΔY of program participants of the same age, where the age variable has been balanced between the two groups. Table 3 provides an account of the probit model estimates of P(X), here the probability of AEI as treatment and LMT as the alternative treatment.[6]

To give estimates of the (relative) ATT a causal interpretation, one needs to assume: *i*) that $0 < P(X) < 1$; *ii*) that program participation does not affect the earnings of other individuals and; *iii*) conditional on the covariates, that the mechanisms behind enrolment decisions are independent of future earnings. The crucial assumption is *iii*. Even with a rich set of covariates, where our differenced outcome accounts for unobserved individual fixed effects affecting earnings, it is not possible to rule out that remaining unobserved factor(s) may correlate with both participation and future earnings. This will be discussed in the remainder of this section.[7]

[6] Unless essential for the balancing of the samples, covariates are discarded from the probit estimates if *p*-values exceed .2. This is because irrelevant covariates may increase bias and/or variance of matching estimators (e.g., Caliendo and Kopeinig 2008, de Luna et al. 2011).

[7] In the case under study, assumption *ii* can also be questioned because both training programs are large. However, Dahlberg and Forslund (2005) find no displacement effects of Swedish training programs in 1987-1996. One may note that they report substantial displacement effects of subsidized employment, as do Crépon et al. (2013) of job search assistance programs. Regarding positive externalities, Albrecht et al. (2009) argue that the returns to society of the AEI were higher than the individual earnings return by a factor of 1.5.

5.2 Application

In the Appendix, Tables A.2 and A.3 present balancing tests pertaining to matched samples where equality of means between the treated and the matched comparisons are not rejected.[8] This holds for all of the estimates discussed in the empirical section. The balancing tests encompass a rich set of covariates that include age, regional employment levels, dummies for region of residence (23 categories), employment sector (7 categories), prior education level (6 categories) and educational track (6 categories), number of children at home (6 categories), age of children (6 categories), indicators of marital status or divorce, pre-treatment annual earnings trajectories for 1990-1995 (1996 with our extended model, see below), and four different types of social insurance benefits in 1990-1995 (1996) related to unemployment insurance, parental leave, sick-leave and social welfare, applying both dummy variables (zero earnings, incidence of the various benefits) and continuous measures of amounts. We further balance on days registered as unemployed each year in 1992-1995 (1996) and on indicator variables if either zero days or the maximum number of days (365/366). In total, our balancing tests encompass at least 132 variables.

Our main concerns regarding sources of potential bias are differences in unobserved ability and in time-varying unobserved factors (see Biewen et al. 2014 for an extensive discussion on specification issues). As a check for ability bias in our estimates, for males born 1953 or later, we compare the results when including and excluding test scores relating to cognitive and non-cognitive skills. The estimation results then only display marginal changes, which on average correspond to .2 percentage points of the annual earnings (app. SEK 400).

[8] Balancing the samples was at times difficult with one-to-one matching without "trimming" the samples (excluding treated participants). Therefore, the results presented are based on four-to-one matching, overall similar to the one-to-one matching estimates, but avoiding trimming. For the balancing tests, we set the threshold at *p*-values of .05. As we balance on more than 120 variables, we allowed one variable to be unbalanced by accident. Our checks indicate that this has no bearing on the estimated results.

Regarding time-varying unobserved factors, changes in motivation or health may not be captured by our covariates.[9] A common critique of difference-in-difference estimators is that a temporary earnings drop in the year prior to program enrolment among the treated generates an upward bias because the earnings level does not reflect the individual's true productivity (Ashenfelter 1978). The baseline model we use in the results section, unless otherwise stated, does not consider covariates recorded in 1996, with pre-program earnings defined as the average of the annual earnings in 1993-1995. A contrasting approach is to assume that changes post-1995 imply changes with permanent effects which must be controlled for (e.g., Heckman and Smith 1999, Heckman et al. 1999). We applied extended versions of our estimation models to consider changes in transfers and earnings 1995-1996. If our estimates are affected by diverging parallel trends, or time-varying unobserved characteristics, one would expect results to systematically change by model specifications. Overall, the different specifications yield negligible differences in estimates. This is perhaps expected, as we compare participants in two programs rather than comparing with "non-participants". In Sections 6 and 7, the extended model results are reported when relevant.[10] Overall, the stability of our findings with respect to the extended model specification and the check for potential ability bias indicate support for our empirical strategy.[11]

[9] For some of the unemployed, program participation seems to be motivated primarily by avoidance of an active job search and/or to qualify for another period of UI benefits (Stenberg and Westerlund 2008, p63).

[10] For our extended model, the balancing concerns an additional 26 variables. We follow Heckman and Smith (1999) to control for nine different transitions in labor force status 1995-1996 between outside the labor force, employment and unemployment. Also included are levels 1996 and changes in the amounts of earnings and social insurance benefits in 1995-1996 and regarding sick-leave or social welfare also for 1996-1997 (we then assume that program choice does not cause transfers to change).

[11] This is consistent with findings from studies assessing non-experimental estimates based on data of high quality. Card et al. (2010) conclude that "*The absence of an 'experimental' effect suggests that the research designs used in recent non-experimental evaluations are not significantly biased relative to the benchmark of an experimental design*" (F475, their quotation marks). Of course, this is not to say that adequate experimental data is not preferred. Nevertheless, when good non-experimental data is available, it is unreasonable to abstain from studying important research questions while waiting for the uncertain event of future access to relevant experimental data.

5.3 Comparative advantages and relative program effects

A basic motivation for policy makers to expand the program types available is that it allows individuals to act on their personal abilities, which may generate comparative advantages. However, if these abilities affect labor market outcomes *independently* of program participation, this may yield bias in our estimates of the relative ATT.

Figure 5 shows the distribution of estimated probabilities of AEI enrolment based on estimates of Pr[AEI] or Pr[LMT] in the probit step. In the segment indicating a high probability of AEI enrolment, one would expect an overrepresentation of individuals with a comparative advantage in theoretical rather than vocational skills. This makes it important to carefully consider the implications of whether the probit step of the matching procedure is based on estimates of Pr[AEI] or Pr[LMT]. If propensity scores are symmetrical, the alternative set-ups will not affect our estimates. However, the distributions in Figure 5 are clearly tilted toward the probability of the program defined as "treatment", and away from the program defined as alternative treatment ("comparison"). [12]

The asymmetry arises because of matching and is exacerbated by that matching is performed "with replacement" (to minimize bias). Thus, a matched comparison is always re-inserted ("replaced") into the pool of potential comparisons. Consider the case where AEI is the treatment. The comparisons are LMT participants who, partly due to the replacement algorithm, are drawn to a greater extent from the side of the probability distribution where AEI participation is more likely. If individuals exploit their comparative advantages, one may then

[12] In contrast, a conventional OLS estimator is perfectly symmetrical and switching between AEI and LMT indicators just switches the sign of the coefficient.

expect estimates of the relative ATT to be more favorable for the AEI program, without necessarily indicating bias.

Assuming that all individuals in our sample have decided to enroll in a program, and that they choose freely between only two existing programs, the Pr[AEI] set-up tests whether the AEI is associated with higher earnings compared with the LMT for those choosing the AEI. However, estimates could hypothetically reflect that the comparative advantages affect earnings independently of the AEI. The results presented below will therefore concern both alternatives, Pr[AEI] and Pr[LMT].

Some rudimentary guidance to the question "what works and for whom?" may be conveyed by comparing the balancing tests of the alternative matching set-ups (Tables A.2 and A.3 in the Appendix). The balanced samples based on Pr[AEI] contain higher fractions of low-skilled and public sector employed. Among males, residing in the Stockholm region is also more prevalent. For females, small children are more common with the Pr[AEI] set up. Interestingly, comparing the balanced samples of Pr[AEI] and Pr[LMT] reveal only very small differences in terms of pre-program annual earnings and the number of days unemployed. In Section 7, we analyze heterogeneity in the relative estimates across subsamples.

6 Main results

Figure 6 displays the estimated impact of each program, i.e., ATT separately for the AEI and the LMT. The matched comparisons here were taken from the pool of individuals registered as unemployed in 1997 but *not* registered in either the LMT or the AEI.[13] The estimates pertaining

[13] This data was not described in Section 4. We refer interested readers to earlier published work that deals in more detail with issues related to evaluations of the respective types of programs, e.g., references given in footnote 1 for LMT and for Komvux Stenberg (2011) and Stenberg and Westerlund (2008).

to LMT are positive in the years immediately following program participation, whereas AEI is associated with an incremental earnings payoff which is only significantly positive from 2001 for females and from 2006 for males. Similar findings for professional training programs with long duration is reported in Lechner et al. (2011) for German labor market programs.

As explained in Section 5.1, the separate program estimates of the ATT are not necessarily comparable. Figure 7a (males) and 7b (females) show the difference-in-differences estimates of the relative ATT between participants of the AEI and the LMT. These are based only on comparable individuals, i.e., a subset of individuals from each program (matched on P(X)). The results indicate an initial and large drop in the relative earnings of the AEI participants, with estimates tending to converge thereafter. The estimates change in the expected direction (as described in Section 5.3), favoring the program chosen as the "treatment" indicator when we switch between matching on Pr[LMT] (the probability of enrolment in LMT) and Pr[AEI]. The relative treatment effects estimated from Pr[AEI] tend to converge for males, while the estimates for females are significantly above zero from 2003 and onward. These findings are shifted downward when the matching is based on estimates of Pr[LMT], positive but closer to zero for females and often significantly below zero for males. Robustness checks for parallel trends via the extended model, and/or basing the analyses on the limited sample (defined in Section 3), corroborate our results.

For males, the largest estimate of the relative impact of the AEI is SEK 8,700, obtained with the limited sample. Even if we extrapolate this result into future years, the present value of the estimated payoff would still not cover the initial relative earnings loss during 1998-2004 (recall that the direct costs are approximated as equal for the two programs). The extrapolation assumes a two percent discount rate and that everyone retires at age 65, accounting for the age structure of the samples (the cohorts retire gradually between 2007

and 2037). This simplified framework is used repeatedly below to assess what the estimates imply for the net benefits from society's point of view.[14]

For the sample of females, extrapolation of the estimates based on Pr[AEI] implies that the initial relative earnings losses (costs) in 1998-2002 are recovered by approximately 2020.[15] The youngest cohort in the sample is then 48 years old, and about half of the individuals are still below age 65. However, the estimates based on Pr[LMT] do not support a conclusion that the initial earnings drop for enrollees in AEI is recovered before the last cohort retires. Thus, to sum up so far, the results provide only weak support for the hypothesis that general training programs would be relatively more beneficial in the long term.

7 Heterogeneous effects

We now turn to analyses of subgroups.[16] Figure 8 present results for samples residing in the commuting areas (as defined by Statistics Sweden) of Stockholm, as well as Gothenburg and Malmö, the second and third largest cities in Sweden. The Stockholm local labor market is by far the biggest in Sweden, with more than one million employed (almost 25 percent of national employment). It is characterized by low unemployment and a high level of diversity. The results for the Stockholm samples contrast with the full sample results. For males, AEI is linked with relative earnings which exceed the LMT matched comparisons. The recorded earnings difference is large also when based on Pr[LMT], and statistically significant from 2007. In extrapolation, the estimated differences above SEK 20,000 imply a recovery of the initial

[14] We fully acknowledge that this may be developed, but leave it for future research. Our priority is to keep the discussion intelligible, and, because we are in relatively unexplored territory, to establish the qualitative results rather than to pin down the precise estimates.

[15] The magnitude of the loss for females in 1998-2002 is about SEK 80,000, only about half the amount for the males.

[16] The results obtained when conditioning samples on age, 25-42 and 43-55, and on whether annual earnings in 1995 were above or below median, did not differ in any important way from the overall results just presented. The working paper version of this article contains details on these analyses (Stenberg and Westerlund 2014).

earnings disadvantage by 2015 and 2018, respectively. For females, all estimates are positive from 2004 but with a slight tendency to converge back toward zero. Nevertheless, the results imply a recovery of initial losses around year 2020, whether one uses AEI or LMT as the "treatment" indicator. For both males and females, the impression from the findings is corroborated if one uses the limited sample and/or the extended model specification (although imprecise in the case of females). For program participants residing in Gothenburg or Malmö, the second and third largest regional labor markets in Sweden (in total around 750,000 employed), the estimates are generally insignificant. The results are consistent with the idea that the relative program outcomes of different types of human capital are sensitive to local labor market characteristics, e.g. size, density, diversity and/or employment structure. The foremost difference in observable employment structures is that Stockholm has a lower share employed in the public sector and in manufacturing.

In Figure 9, the estimation results are displayed for groups with 1) a two-year upper secondary school diploma and 2) no completion of upper secondary school. One could argue that groups with limited education are of particular interest because the AEI offers education at the levels that were not completed by the individuals in those groups. For males, there is a tendency for estimates to be above zero only in the case of no upper secondary school, but this does not hold when the matching is based on Pr[LMT].

The results for females with prior completion of a two-year upper secondary program indicate positive relative earnings estimates of the AEI from 2003 to 2010, a result that also holds when the matching is based on Pr[LMT]. In both cases, the estimates imply that the initial relative earnings losses are recovered around 2020. This is also the case for the limited sample, but not when applying the extended model specification. Turning to females with no secondary education, at the bottom of Figure 9, the estimates are positive and statistically

significant almost throughout from 2003 and onward, regardless of specification and/or sample used. The accumulated net present values implied by the estimates indicate that the initial earnings disadvantage is already recovered within or just beyond our observation window. This result is very stable as it holds whether the matching is based on Pr[AEI] or Pr[LMT], and whether one employs the limited sample and/or uses the extended model specification (or both). We also checked if the results reflect fertility decisions by conditioning on samples to have two children, most often signaling completed fertility, or to have zero children, but the overall implications remain robust. Thus, for this particular subgroup, expanding the menu of labor market programs to include general training appears associated with substantial efficiency gains.

While this last result seems relatively compelling, it may be difficult to generalize because 1997 was the first year of a reform. One could imagine that an inherent demand made individuals with the highest gains from the AEI more likely to enroll. To check this, we estimated the corresponding relative program effects for participants without upper secondary school in 1998, 1999, 2000 and 2001, using data of the same quality as described in Section 4. The business cycle recovered quickly in 1997-2000 and further contributed to generating a different composition of the samples. In six cases out of eight, these estimates imply a similar recovery of initial earnings losses of AEI. The exceptions are when employing Pr[LMT] in the 1999 sample or the 2001 sample. We made a similar examination of the results for Stockholm residents, which hold only in five of 16 cases, with the Pr[AEI] set-up for males in 2000 and 2001 and with both set-ups for females in 1998 and 2001.

Finally, we use the information contained in the test scores relating to cognitive and non-cognitive skills, which are available for males born 1953 or later. We separate this sample based on whether the respective test scores are above or below the median values, resulting

in four groups in total (Figure 10). The findings are now less precise but still display two clear patterns. First, dividing the sample based on cognitive skills, above or below the median, has little impact on estimates. Perhaps surprisingly, cognitive skills do not seem to be important for the relative earnings impact of general vs. specific training. Second, the individuals with non-cognitive test scores below median appear to benefit more from specific training. For this group, the point estimates are statistically significant (negative) throughout. In contrast, those with above-median non-cognitive skills are associated with relatively stronger earning effects of general training. The magnitude of the positive estimates is overall modest (also with the limited sample or the extended model specification), but it is interesting that the pattern of results between the groups above and below median is relatively clear. A possible interpretation is that learning a specific skill is a way to compensate for a lower level of non-cognitive skills. Conversely, non-cognitive skills may be an important complement for benefiting from general training.

8 Summary

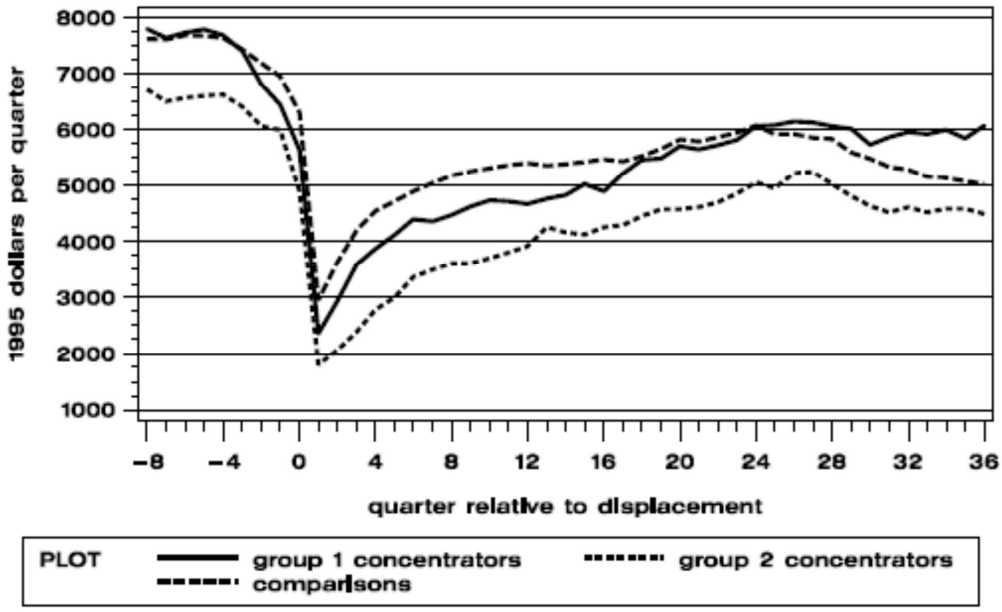
A principal contribution of this study is to provide empirical evidence on long term earnings associated with general training as an alternative to vocational/specific training. Heterogeneity among the unemployed, and in labor market demand for skills, implies that variety in the supply of training may allow individuals to capitalize on comparative advantages and improve the benefits of investments. With data on earnings 13 years post-enrolment showing differences between long-term and short-term outcomes, our analyses underscore the need for long follow-up periods to appropriately assess such programs. We also find strong indications that individuals tend to act on their comparative advantages. Characteristics predicting enrolment in general or specific training tend to be associated with estimated relative treatment effects that favor the chosen type of training. Methodologically, robustness

checks for ability bias and time-varying characteristics prior to the program confirm our main findings.

For females with limited prior schooling and for participants in the metropolitan labor market of Stockholm, we find that general training is associated with earnings that exceed those of specific training. These findings are in line with the hypothesis that general training better enhances labor market prospects in the long run, by providing skills which make individuals less sensitive to labor market-related changes. Nevertheless, most of our estimates imply that vocational/specific training is associated with more favorable earnings trajectories. Therefore, arguments in favor of theoretical/general training programs must be based on the heterogeneity of the unemployed. As has been suggested earlier, theoretical programs may be especially appropriate in periods of high unemployment when opportunity costs are low and high numbers in specific training programs may inflict lower marginal returns.

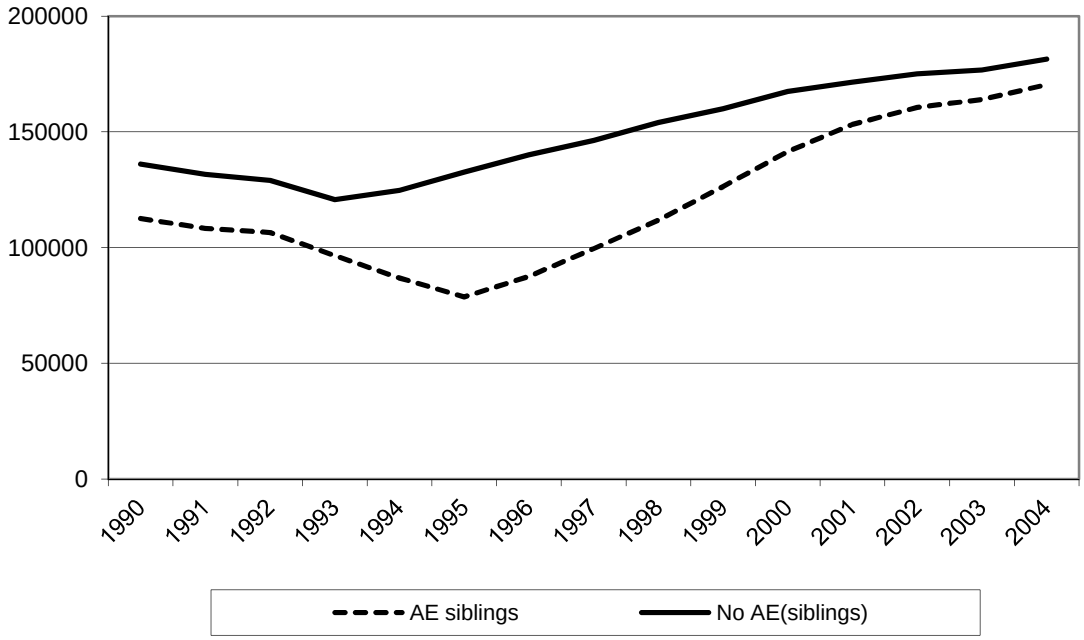
Our study makes a distinct contribution compared with previous research, but there are some important caveats and we would like to point out four of these. First, the program costs are based on rough approximations and are assessed as equal on average. Second, the comparison between the two programs disregards outside alternatives, e.g., other programs. Third, other goals for policy (equity, democracy, etc.) are not considered. Fourth, general equilibrium effects are not considered. One might think here of costs associated with general training because, in the presence of labor market frictions, firms have incentives to offer not only specific training but also general education (Acemoglu and Pischke 1999). As in the case of specific training, increased public supply of general training may be associated with a deadweight loss due to crowding out of firms' investments in general skills.

Figure 1a: Earnings trajectories of enrollees and non-enrollees in community college, laid off workers aged 35 or over, Washington State.



Source: Jacobson et al. (2003). Group 1 concentrators: Quantitative or technically oriented vocational courses. Group 2 concentrators : less quantitative courses.

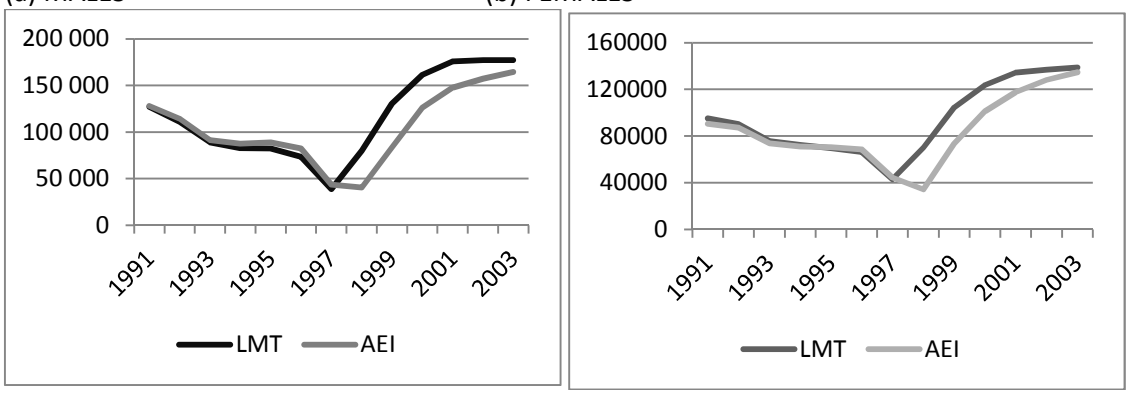
Figure 1b: Earnings trajectories of enrollees in Komvux adult education (AE) 1994-1995 and of their sibling non-enrollees.



Note: Data reproducing descriptive statistics from Stenberg (2011).

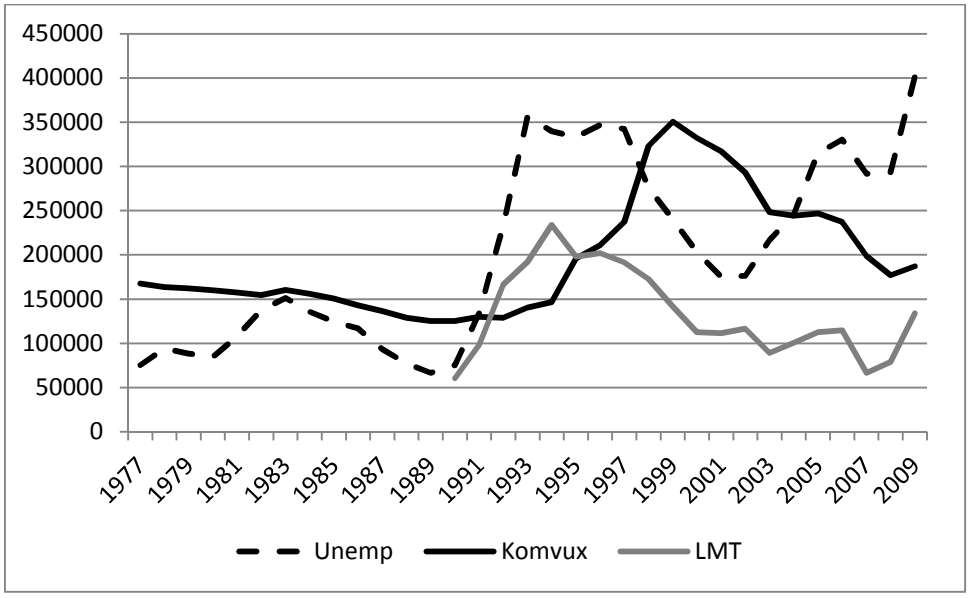
Figure 2: Earnings trajectories of 1997 enrollees in AEI and LMT.

(a) MALES (b) FEMALES



Source: Stenberg (2007).

Figure 3: Number of unemployed and enrollees in Labor Market Training and Komvux.



Source: Statistics Sweden, various registers.

Table 1: Descriptive mean statistics by program

	Males			Females		
	AEI	LMT	*p*-value	AEI	LMT	*p*-value
Age	35.053	37.397	0.000	35.080	37.882	0.000
Children	0.841	0.921	0.000	1.500	1.240	0.000
No children	0.551	0.506	0.000	0.246	0.337	0.000
One child	0.191	0.202	0.135	0.239	0.254	0.025
Two children	0.167	0.194	0.000	0.337	0.278	0.000
Child 0-3	0.153	0.160	0.323	0.266	0.181	0.000
Child 4-6	0.127	0.138	0.076	0.300	0.219	0.000
Married	0.265	0.319	0.000	0.410	0.410	0.925
Divorced	0.096	0.119	0.000	0.129	0.170	0.000
Years of sch	10.840	11.325	0.000	10.822	11.657	0.000
No upp sec sch	0.255	0.226	0.000	0.249	0.211	0.000
2-yr upp sec	0.605	0.493	0.000	0.634	0.422	0.000
Vocational 2 yrs	0.163	0.118	0.000	0.406	0.260	0.000
Business 2 yrs	0.072	0.030	0.000	0.170	0.140	0.000
Social sci 2 yrs	0.045	0.022	0.000	0.055	0.036	0.000
Technology 2 yrs	0.310	0.327	0.054	0.023	0.023	0.873
Business 3 yrs	0.037	0.072	0.000	0.035	0.114	0.000
Tertiary	0.060	0.162	0.000	0.053	0.215	0.000
Region emp.(gender)	0.725	0.722	0.000	0.692	0.695	0.000
Stockholm	0.145	0.123	0.000	0.107	0.159	0.000
Inland of Norrland	0.079	0.078	0.906	0.068	0.066	0.583
Farming/Mining	0.012	0.019	0.007	0.007	0.007	0.788
Construction	0.060	0.122	0.000	0.006	0.008	0.023
Manufacturing	0.090	0.140	0.000	0.043	0.072	0.000
Finance/insurance	0.076	0.089	0.012	0.054	0.093	0.000
Public sector	0.135	0.086	0.000	0.394	0.254	0.000
Other sector	0.205	0.191	0.065	0.174	0.215	0.000
Foreign born	0.168	0.190	0.003	0.136	0.193	0.000
Parent>0 1990	0.057	0.053	0.384	0.241	0.175	0.000
Parent>0 1995	0.057	0.060	0.631	0.295	0.205	0.000
Sick>0 1990	0.745	0.729	0.056	0.785	0.739	0.000
Sick>0 1995	0.197	0.178	0.008	0.280	0.264	0.019
Social welf.>0 1990	0.149	0.140	0.143	0.142	0.128	0.008
Social welf.>0 1995	0.157	0.156	0.969	0.141	0.150	0.103
UI>0 1990	0.168	0.157	0.123	0.196	0.174	0.000
UI>0 1995	0.688	0.706	0.041	0.695	0.680	0.031
Days unempl 1995	230.147	232.317	0.418	214.498	219.813	0.014
Max unempl 1995	0.159	0.145	0.040	0.163	0.150	0.022
No unempl 1995	0.187	0.163	0.001	0.193	0.175	0.002
No earn 1995	0.246	0.233	0.111	0.203	0.232	0.000
Observations	4245	8185		11854	6944	

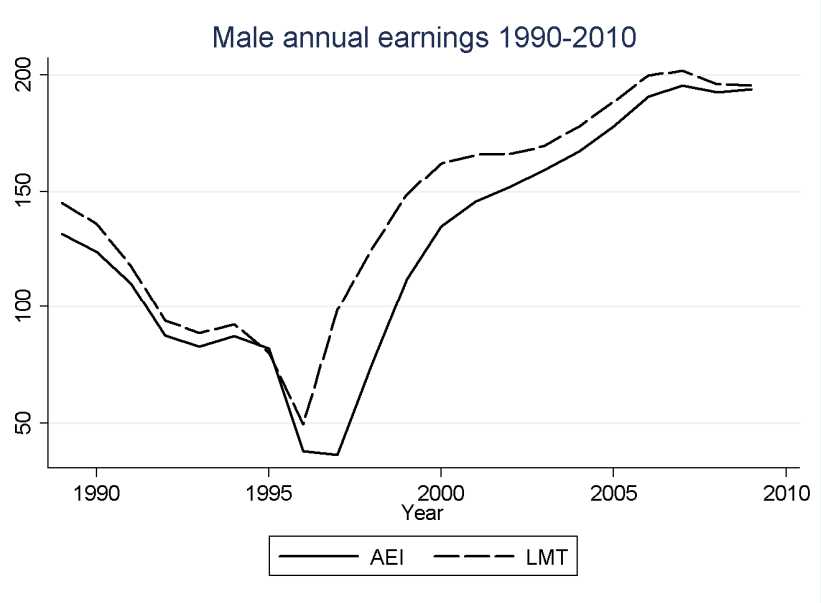
Note 1: Regional employment levels are gender specific. In 1990, sick leave benefits were paid from the first day of absence. This rule was changed in 1993 and only paid from the second day of sick leave absence.

Table 2. Content of general training within the AEI. Credits expressed in years of full-time studies.

	Males	Females
N	4,245	11,854
Total registered course credits at Komvux (years)	1.694	1.969
Total completed course credits at Komvux (years)	.883	1.112
Fraction completing zero credits	.150	.103
Fraction completing credits > 0 but < .25 years of AE	.082	.062
Fraction completing credits > .25 but < .5 years of AE	.115	.085
Fraction completing credits > .5 but < 1 year of AE	.278	.267
Fraction completing more than 1 year of AE credits	.376	.483
Proportion registered in compulsory level courses	.291	.278
Registered compulsory credits, average	.263	.217
Completed compulsory credits, average	.077	.073
Completed compulsory credits, if registered at level	.263	.263
Proportion registered in upper secondary level courses	.919	.951
Registered upper secondary credits, average	1.418	1.730
Completed upper secondary credits, average	.799	1.028
Completed upper secondary credits, if registered at level	.870	1.081
Proportions in type of upper secondary course registration		
- English	.749	.718
- Swedish	.739	.729
- Mathematics	.757	.711
- Social sciences	.810	.879
- Natural sciences	.368	.377
- Human sciences (e.g., foreign languages)	.160	.217
- Computer sciences	.719	.761
- Health-related subjects (e.g., nursing)	.220	.446
- Vocational courses	.000	.000
Proportion completing some tertiary level education	.139	.171
Completed tertiary education, average	.311	.383
Completed tertiary education, if registered at level	2.235	2.244
Total adult education completed (years)	**1.186**	**1.484**

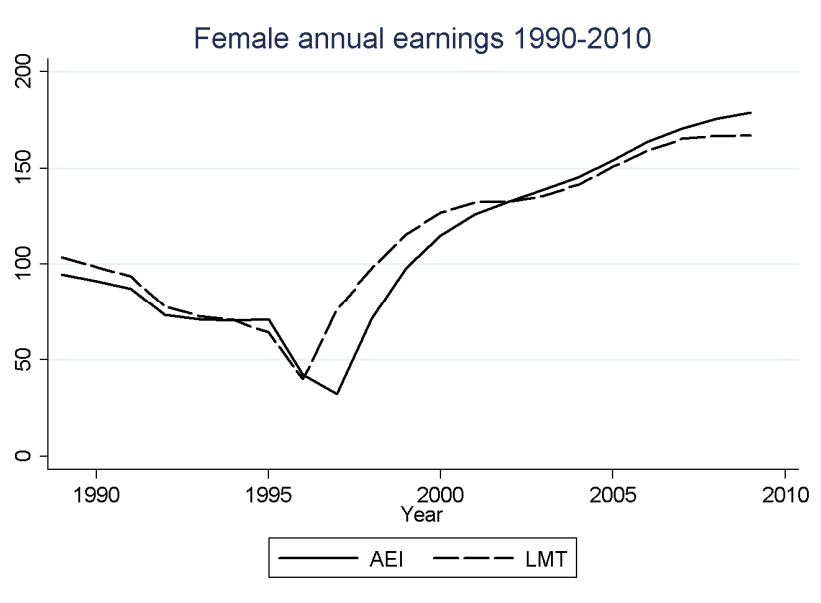
Figure 4: Earnings trajectories of AEI (general) and LMT (specific) participants.

MALES



N^{AEI} = 4,245 and N^{LMT} = 8,185

FEMALES



N^{AEI} = 11,854 and N^{LMT} = 6,944.

Table 3: Probit model estimates of the relative probability of enrolment in AEI.

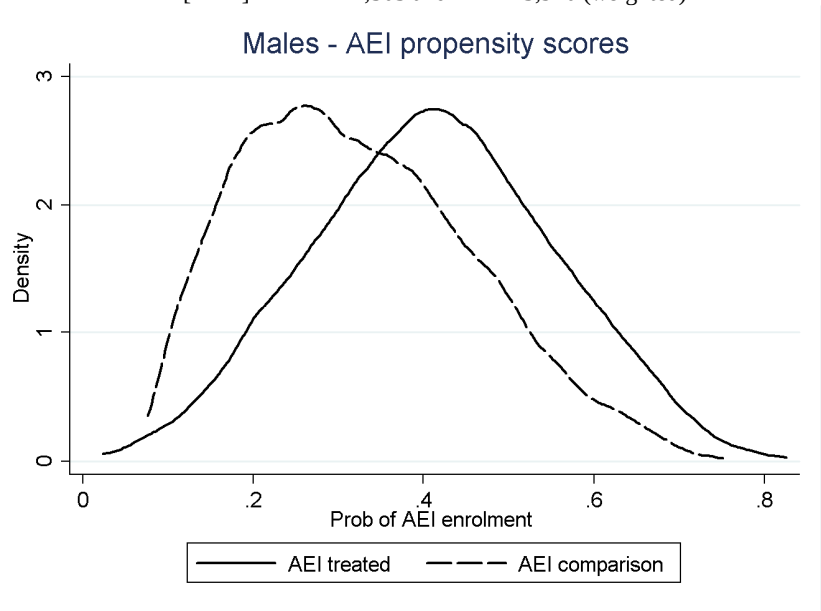
	Males [a]		Females [a]	
Age			-0.0331*	(0.0142)
9 yrs of sch	1.2290***	(0.0918)		
Less than 9 yrs	1.0332***	(0.1059)		
No upp sec sch			0.7603***	(0.0523)
2-yr upp sec	1.2842***	(0.0941)	0.5760***	(0.0473)
Social sci 2 yrs	0.2506**	(0.0790)	0.2434***	(0.0616)
Vocational 2 yrs	0.2281***	(0.0485)	0.2984***	(0.0403)
Technology 2 yrs	-0.1262**	(0.0459)	0.1017	(0.0763)
Business 2 yrs	0.3021***	(0.0680)	0.1989***	(0.0469)
12 yrs of sch	0.9293***	(0.0987)	0.1970***	(0.0462)
Business 3 yrs	-0.1538*	(0.0692)	-0.1662**	(0.0513)
15 yrs of scho	0.6587***	(0.0991)		
Regional emp.	2.5760***	(0.4302)		
Stockholm	0.0688	(0.0446)		
Malmö	0.1806***	(0.0377)	0.2278***	(0.0375)
Gothenburg			0.2679***	(0.0348)
Farming/Mining	-0.4766***	(0.1020)	-0.1532	(0.1205)
Construction	-0.5777***	(0.0484)	-0.1677	(0.1246)
Manufacturing	-0.4709***	(0.0429)	-0.3490***	(0.0474)
Finance/insurance	-0.2276***	(0.0481)	-0.2155***	(0.0435)
Public sector	0.1806***	(0.0436)	0.2572***	(0.0277)
Other sector	-0.1611***	(0.0349)	-0.1353***	(0.0304)
Divorced	-0.0663	(0.0421)	-0.0767*	(0.0304)
One child	-0.0695*	(0.0326)	0.1171***	(0.0347)
Two children	-0.1372***	(0.0369)	0.2153***	(0.0413)
Three children	-0.1111*	(0.0566)	0.2849***	(0.0541)
Four children	-0.1383	(0.0943)	0.2658***	(0.0792)
Foreign born	-0.0568	(0.0372)		
Child 0-3			0.1022*	(0.0451)
Child 7-10			0.0323	(0.0345)
Child 11-15	0.1145*	(0.0457)		
Child 18 or ol~r			-0.1191**	(0.0390)
Parental 1993			-0.0149*	(0.0064)
Parental 1995			-0.1756**	(0.0681)
Parent>0 1990	0.1020	(0.0605)		
Parent>0 1991	0.0425	(0.0579)	-0.0546	(0.0327)
Parent>0 1993			0.0560	(0.0417)
Parent>0 1994			0.0961**	(0.0369)
Parent>0 1995			0.0849	(0.0436)
Earnings 1990			-0.0304	(0.0201)
Earnings 1991	-0.0628*	(0.0244)		
Earnings 1992	0.0310	(0.0239)		
Earnings 1993	-0.0272	(0.0288)		
Earnings 1994	-0.0661**	(0.0254)	0.0352	(0.0226)
Earnings 1995			0.2295***	(0.0440)
No earn 1991	-0.0386	(0.0498)		
No earn 1993	-0.0576	(0.0367)	-0.0485	(0.0297)
No earn 1994	-0.0416	(0.0364)		
UI 1990	0.2157*	(0.0903)		
UI 1991	-0.0076	(0.0061)	-0.0170*	(0.0085)
UI 1993	-0.0129*	(0.0051)	-0.0093*	(0.0044)
UI>0 1991			0.0555	(0.0365)

UI>0 1994	-0.0882*	(0.0391)		
UI>0 1995			0.0735*	(0.0324)
Days unempl 1992			0.0002	(0.0001)
Days unempl 1993	0.0002	(0.0002)		
Days unempl 1994			0.0003***	(0.0001)
Days unempl 1995	-0.0001	(0.0002)		
Max unempl 1992			0.1018*	(0.0436)
Max unempl 1993	0.0645	(0.0397)		
Max unempl 1994	0.0570	(0.0384)		
Max unempl 1995	0.0819*	(0.0378)		
No unempl 1992			0.0430	(0.0360)
No unempl 1993	0.0954	(0.0505)		
No unempl 1995	0.1335**	(0.0513)		
Sick leave 1990			-0.1102	(0.0692)
Sick leave 1992	-0.0166**	(0.0056)		
Sick leave 1994	-0.0077	(0.0057)	-0.0149**	(0.0051)
Sick leave 1995	-0.1144	(0.0595)		
Sick>0 1990			0.0589*	(0.0300)
Sick>0 1991	0.0641*	(0.0302)	0.0385	(0.0275)
Sick>0 1993	0.0500	(0.0300)	0.0499*	(0.0234)
Sick>0 1995	0.0831*	(0.0353)		
Social welf 1992	0.0523*	(0.0206)		
Social welf 1993	-0.0505*	(0.0212)	-0.0308	(0.0254)
Social welf 1994			-0.0529*	(0.0256)
Social welf 1995	-0.4684	(0.2573)		
Social welf.>0 1990			0.0616	(0.0338)
Social welf.>0 1995			-0.0284	(0.0343)
Constant	-3.3261***	(0.3346)	0.1317	(0.2662)
Observations	12098		17509	
Pseudo R-squared	0.0863		0.1066	

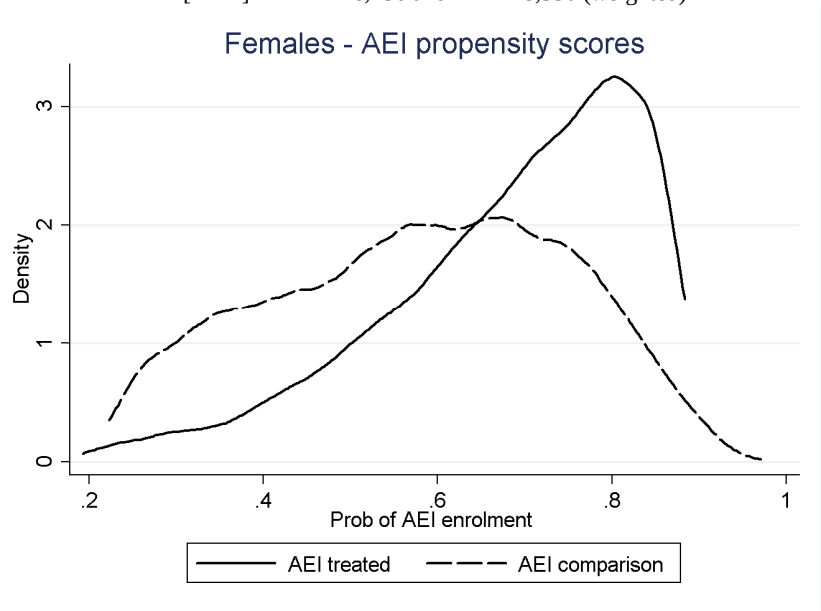
[a)] Earnings and transfers expressed in SEK 100,000 (2010 values). For reasons of space, coefficients not displayed include age-dummies (males) and 13 additional regional dummies. Estimates are also based on interaction variables which for males only include (Social welf.>0 1990*UI 1995). For females, the indicator variable of 9 years of schooling is interacted with “no unemployment 1995”; five interaction variables involve “no upper secondary school” (age at immigration, sick leave 1992, social welfare 1990 and 1995 and earnings 1995); two interaction variables involve two year upper secondary school (no unemployment 1995, and age at immigration); Stockholm is interacted with sick leave benefits 1991; and finally earnings 1995 squared is also included.

Figure 5a: AEI participants weighted distribution of propensity score estimates, using AEI as treatment Pr[AEI] and as comparison Pr[LMT] respectively.

Males: Pr[AEI] N^{AEI} = 4,138 and N^{LMT} = 5,893 (weighted)
Pr[LMT] N^{LMT} = 7,503 and N^{AEI} = 3,970 (weighted)

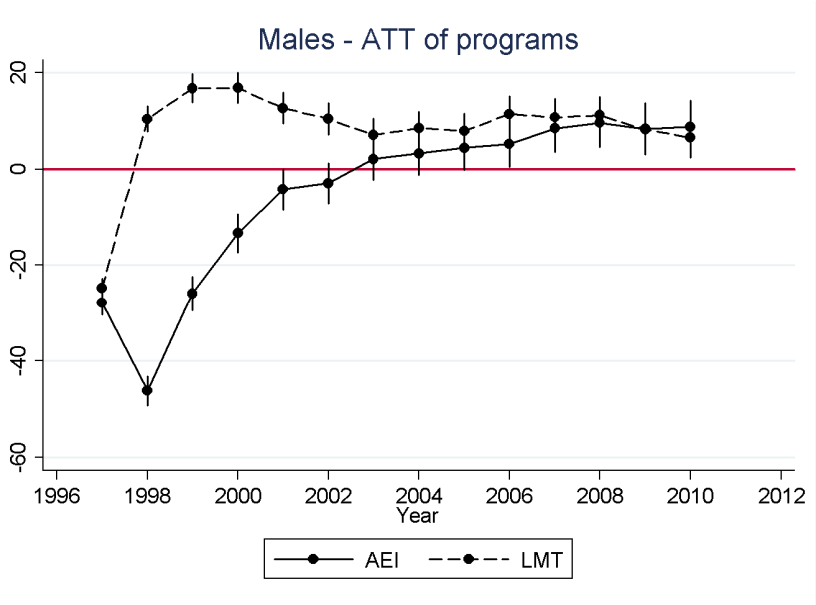


Females Pr[AEI] N^{AEI} = 11,478 and N^{LMT} = 5,809 (weighted)
Pr[LMT] N^{LMT} = 6,156 and N^{AEI} = 8,530 (weighted)

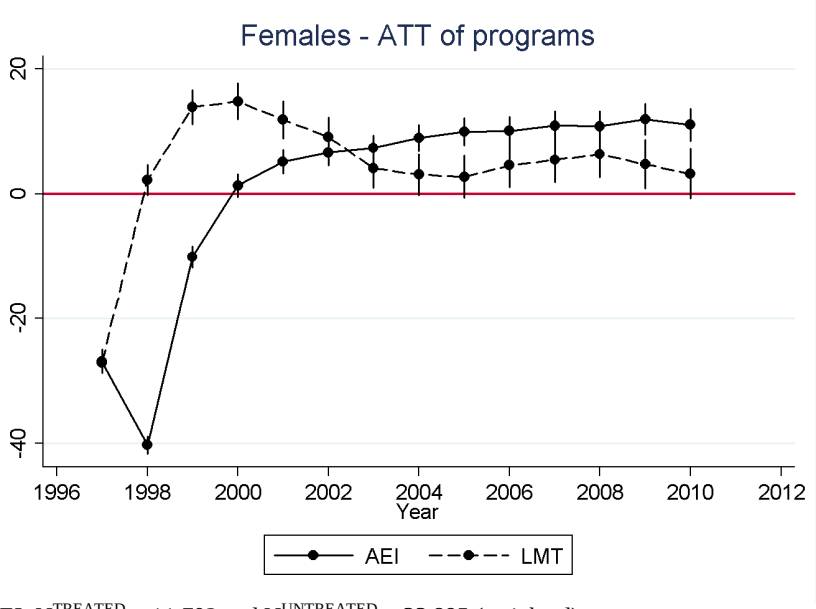


Note: For comparability, the distributions pertaining to Pr[LMT] are presented as probabilities of AEI enrolment (absolute value of 1-Pr[LMT]).

Figure 6: Difference in difference estimates (SEK in 1000s) of program effects on annual earnings, separately estimated for the AEI and LMT.

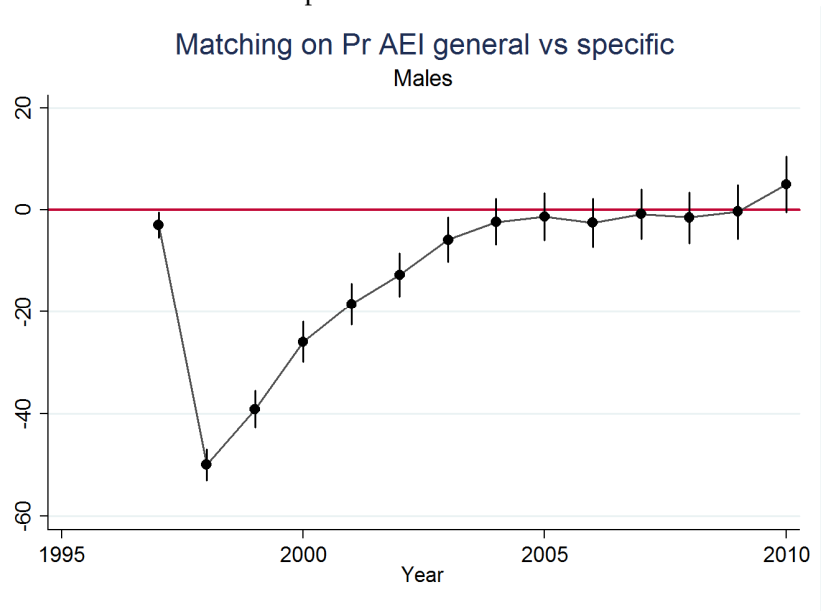


AEI: $N^{TREATED}$ = 4,089 and $N^{UNTREATED}$ = 14,289 (weighted).
LMT: $N^{TREATED}$ = 7,852 and $N^{UNTREATED}$ = 25,189 (weighted).

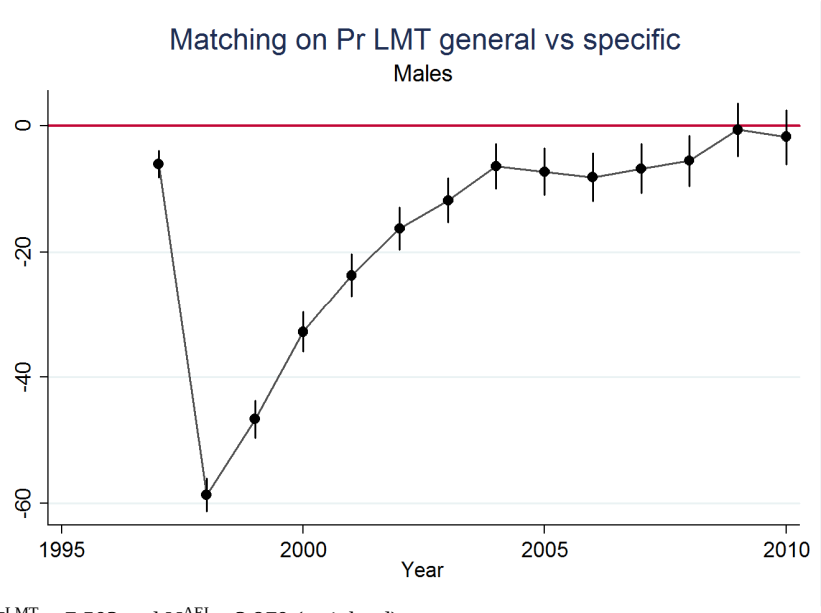


AEI: $N^{TREATED}$ = 11,702 and $N^{UNTREATED}$ = 33,895 (weighted).
LMT: $N^{TREATED}$ = 6,698 and $N^{UNTREATED}$ = 21,745 (weighted).

Figure 7a: Male difference-in-differences (SEK in 1000s) propensity score matching estimates, benchmark samples.

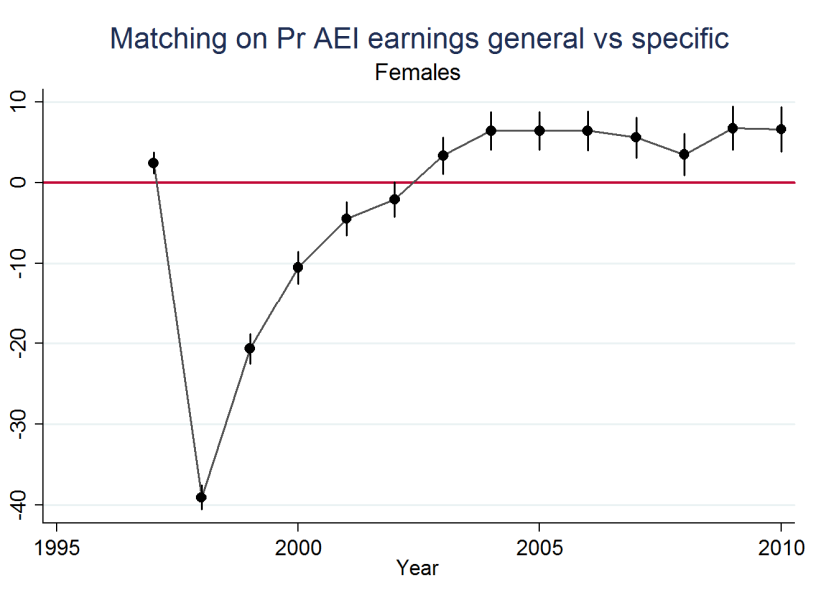


N^{AEI} = 4,138 and N^{LMT} = 5,893 (weighted)

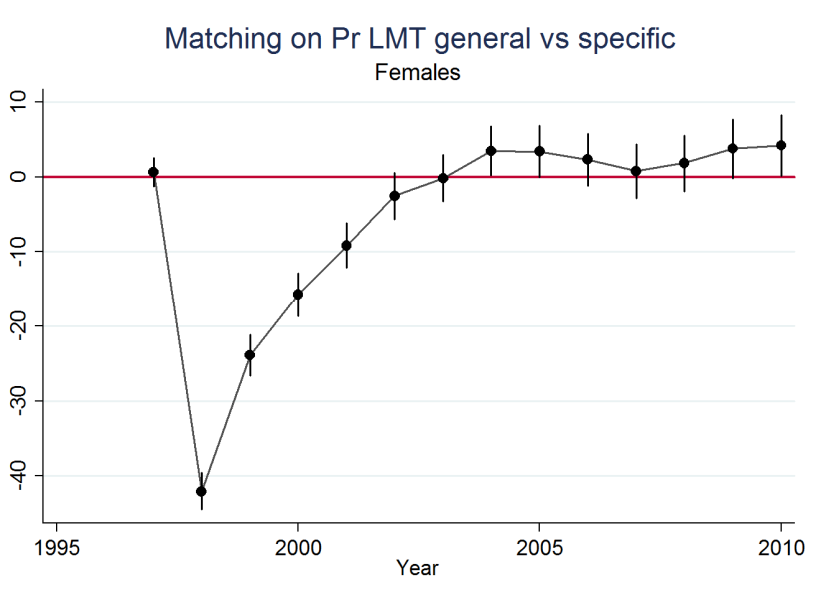


N^{LMT} = 7,503 and N^{AEI} = 3,970 (weighted)

Figure 7b: Female difference-in-differences (SEK in 1000s) propensity score matching estimates, benchmark samples.



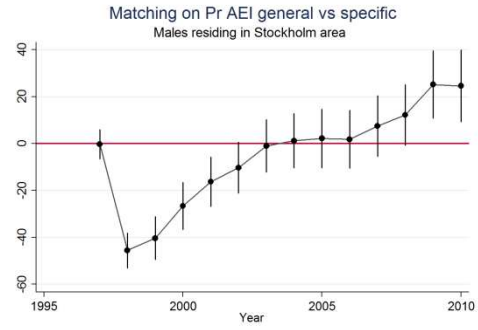
N^{AEI} = 11,405 and N^{LMT} = 5,711 (weighted)



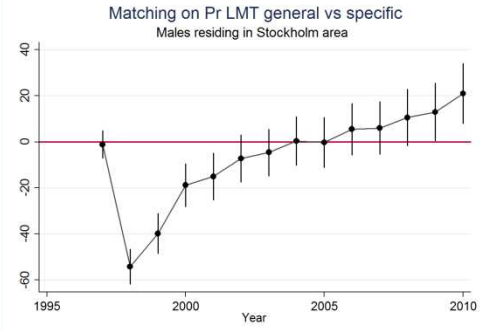
N^{LMT} = 6,156 and N^{AEI} = 8,530 (weighted)

Figure 8: Difference in differences matching, benchmark sample estimates by area of residence.

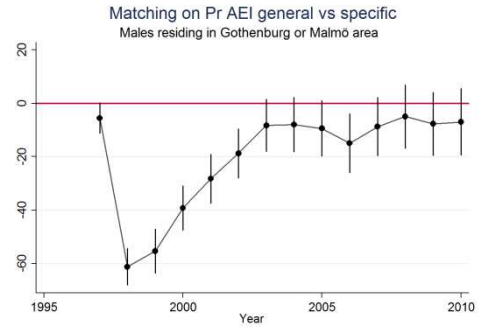
MALES residing in a) the Stockholm or b) Gothenburg & Malmö area



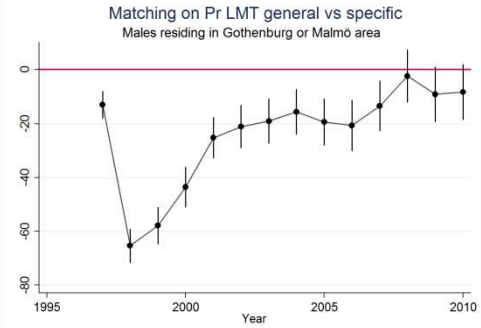
N^{AEI} = 734 and N^{LMT} = 885 (weighted)



N^{LMT} = 1,022 and N^{AEI} = 653 (weighted)

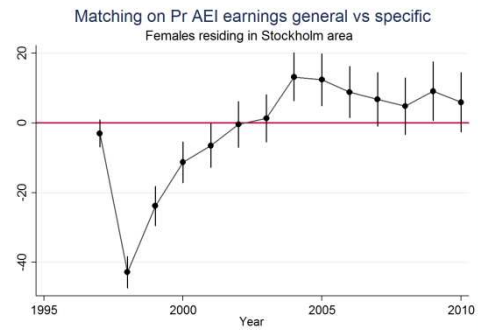


N^{AEI} = 831 and N^{LMT} = 1,659 (weighted)

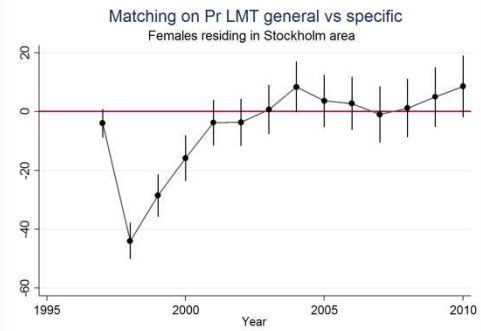


N^{LMT} = 1,376 and N^{AEI} = 769 (weighted)

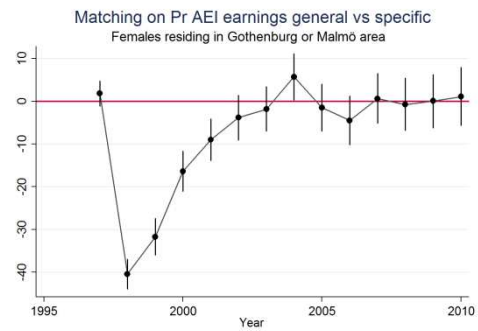
FEMALES residing in a) the Stockholm or b) Gothenburg & Malmö area



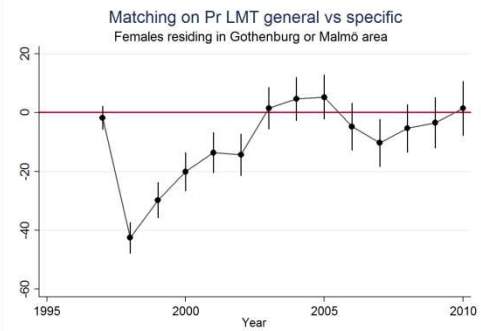
N^{AEI} = 1,396 and N^{LMT} = 1,016 (weighted)



N^{LMT} = 1,125 and N^{AEI} = 1,551 (weighted)



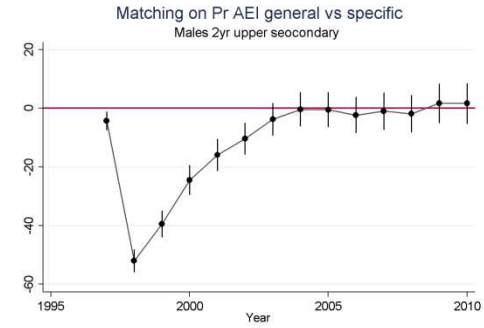
N^{AEI} = 1,899 and N^{LMT} = 1,022 (weighted)



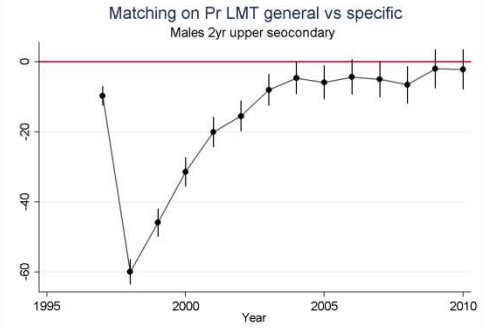
N^{LMT} = 1,207 and N^{AEI} = 1,488 (weighted)

Figure 9: Difference in differences matching, benchmark sample estimates by prior level of education.

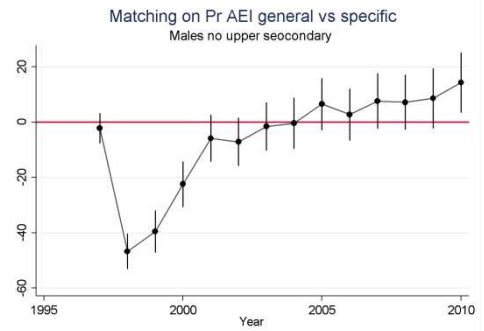
MALES by schooling



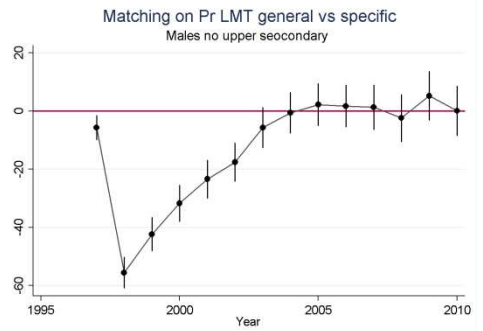
N^{AEI} = 2,534 and N^{LMT} = 3,375 (weighted)



N^{LMT} = 3,992 and N^{AEI} = 2,432 (weighted)

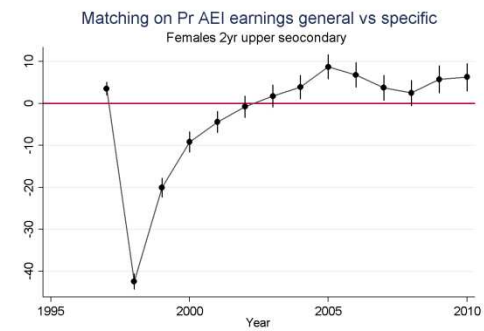


N^{AEI} = 978 and N^{LMT} = 1,341 (weighted)

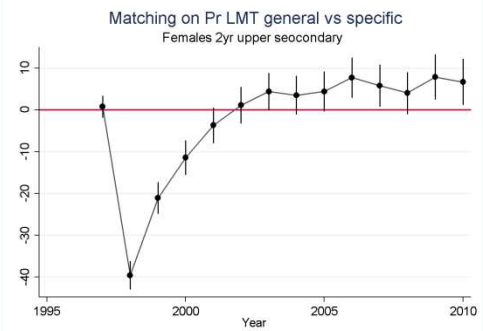


N^{LMT} = 1,973 and N^{AEI} = 970 (weighted)

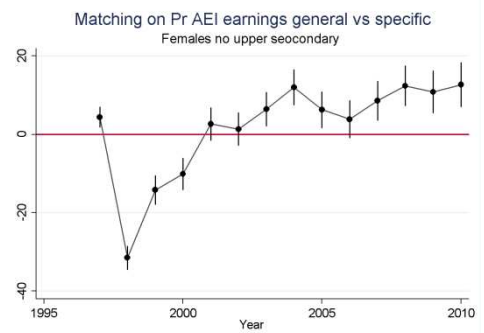
FEMALES by schooling



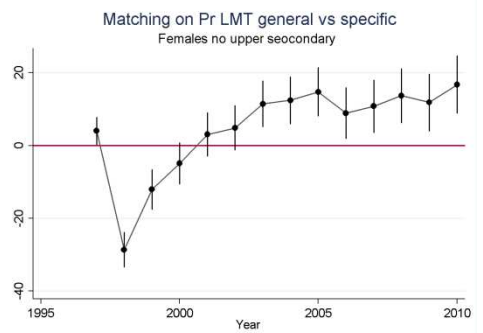
N^{AEI} = 7,340 and N^{LMT} = 2,831 (weighted)



N^{LMT} = 3,080 and N^{AEI} = 5,207 (weighted)



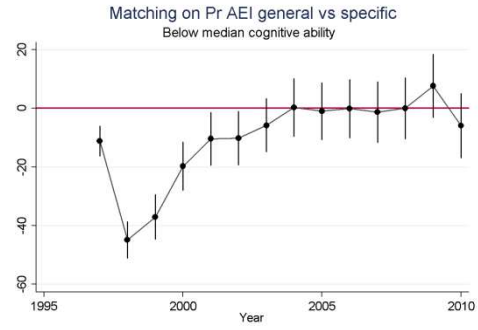
N^{AEI} = 2,625 and N^{LMT} = 1,400 (weighted)



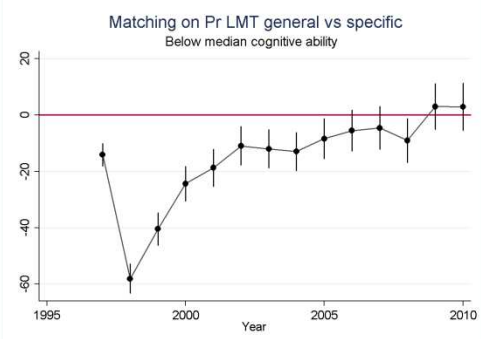
N^{LMT} = 1,464 and N^{AEI} = 2,051 (weighted)

Figure 10: Difference in differences matching, benchmark sample estimates separately for above and below median of cognitive and non-cognitive skills.

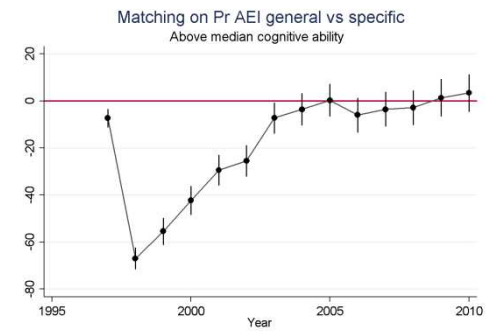
MALES by cognitive skills



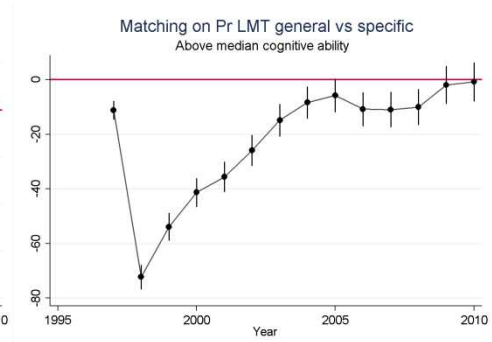
N^{AEI} = 987 and N^{LMT} = 1,065 (weighted)



N^{LMT} = 1,402 and N^{AEI} = 763 (weighted)

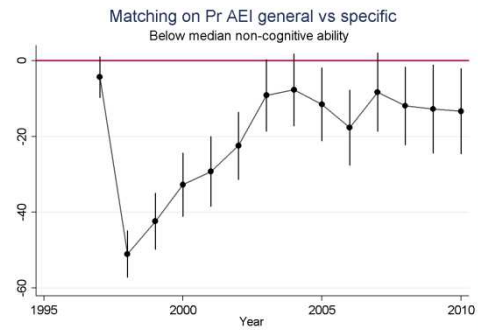


N^{AEI} = 1,795 and N^{LMT} = 2,156 (weighted)

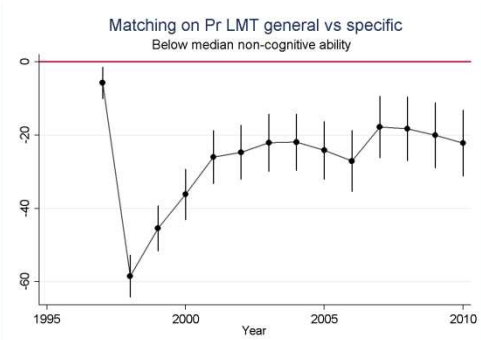


N^{LMT} = 2,655 and N^{AEI} = 1,653 (weighted)

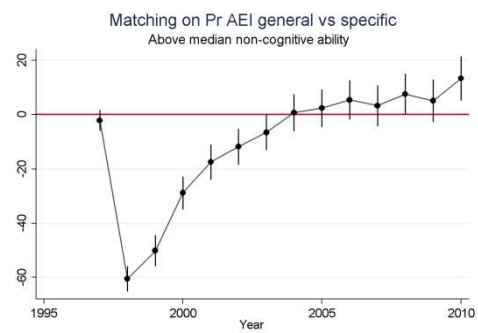
MALES by non-cognitive skills



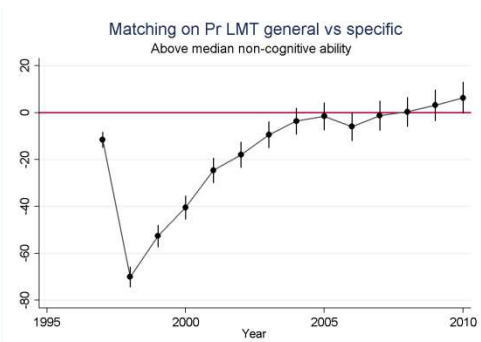
N^{AEI} = 839 and N^{LMT} = 1,039 (weighted)



N^{LMT} = 1,292 and N^{AEI} = 803 (weighted)



N^{AEI} = 1,736 and N^{LMT} = 2,186 (weighted)



N^{LMT} = 2,766 and N^{AEI} = 1,624 (weighted)

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ONLINE APPENDIX

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NOT INTENDED FOR PUBLICATION

Table A.1: Limited sample, descriptive mean statistics by program

	Males			Females		
	AEI	LMT	*p*-value	AEI	LMT	*p*-value
Age	35.234	37.978	0.000	35.069	37.924	0.000
Children	0.841	0.923	0.010	1.555	1.250	0.000
No children	0.552	0.504	0.001	0.233	0.336	0.000
One child	0.178	0.199	0.065	0.223	0.242	0.032
Two children	0.185	0.201	0.137	0.356	0.292	0.000
Child 0-3	0.160	0.157	0.721	0.293	0.214	0.000
Child 4-6	0.125	0.136	0.263	0.332	0.254	0.000
Married	0.278	0.342	0.000	0.428	0.429	0.937
Divorced	0.082	0.112	0.001	0.113	0.149	0.000
Yrs of sch	10.781	11.297	0.000	10.843	11.753	0.000
No upp sec sch	0.269	0.235	0.005	0.230	0.193	0.000
2-yr upp sec	0.612	0.488	0.000	0.671	0.438	0.000
Vocational 2 yrs	0.174	0.117	0.000	0.449	0.279	0.000
Business 2 yrs	0.090	0.033	0.000	0.168	0.134	0.000
Social sci 2 yrs	0.045	0.023	0.000	0.055	0.039	0.000
Technology 2 yrs	0.294	0.321	0.044	0.019	0.021	0.622
Business 3 yrs	0.035	0.070	0.000	0.025	0.108	0.000
Tertiary	0.053	0.164	0.000	0.052	0.230	0.000
Regional empl.	0.728	0.724	0.000	0.693	0.696	0.000
Stockholm	0.168	0.132	0.000	0.111	0.163	0.000
Inland of Norrland	0.078	0.076	0.774	0.067	0.065	0.735
Farming/Mining	0.014	0.022	0.041	0.008	0.007	0.539
Construction	0.075	0.133	0.000	0.005	0.010	0.008
Manufacturing	0.101	0.144	0.000	0.046	0.069	0.000
Finance/insurance	0.081	0.090	0.260	0.053	0.098	0.000
Public sector	0.150	0.085	0.000	0.429	0.272	0.000
Other sector	0.233	0.208	0.033	0.183	0.233	0.000
Foreign born	0.127	0.144	0.082	0.102	0.141	0.000
Parent>0 1990	0.057	0.051	0.326	0.239	0.160	0.000
Parent>0 1995	0.067	0.066	0.814	0.337	0.256	0.000
Sick>0 1990	0.734	0.720	0.257	0.789	0.735	0.000
Sick>0 1995	0.199	0.182	0.120	0.288	0.277	0.260
Social welf.>0 1990	0.104	0.092	0.141	0.101	0.089	0.044
Social welf.>0 1995	0.126	0.117	0.361	0.109	0.119	0.126
UI>0 1990	0.121	0.107	0.125	0.160	0.139	0.005
UI>0 1995	0.537	0.564	0.049	0.585	0.552	0.002
Days unempl 1995	172.263	179.031	0.111	177.254	173.242	0.205
Max unempl 1995	0.118	0.107	0.202	0.149	0.122	0.000
No unempl 1995	0.330	0.280	0.000	0.293	0.283	0.323
No earn 1995	0.139	0.146	0.444	0.136	0.169	0.000
Observations	1916	3730		6378	3423	

Note: Regional employment levels are gender specific. In 1990, sick-leave benefits were paid from the first day of absence. This rule was changed in 1993 and only paid from the second day of sick leave absence.

Table A.2. Males, balancing tests, matched samples descriptive statistics. Two versions based on matching of probability estimates of AEI (left) or LMT (right).

	Pr[AEI]			Pr[LMT]		
	AEI	LMT	*p*-value	AEI	LMT	*p*-value
Age	35.072	35.118	*0.794*	36.926	36.967	*0.775*
Children	0.831	0.830	*0.983*	0.905	0.932	*0.163*
No child	0.554	0.557	*0.782*	0.514	0.510	*0.582*
1 child	0.192	0.182	*0.268*	0.202	0.198	*0.606*
2 children	0.166	0.172	*0.482*	0.186	0.186	*0.983*
3 children	0.062	0.061	*0.982*	0.070	0.075	*0.311*
4 children	0.019	0.022	*0.304*	0.022	0.023	*0.528*
> 4 children	0.008	0.006	*0.188*	0.006	0.008	*0.160*
Child aged 0-3	0.150	0.152	*0.736*	0.157	0.161	*0.558*
Child aged 4-6	0.125	0.128	*0.668*	0.137	0.138	*0.928*
Child aged 7-10	0.131	0.135	*0.576*	0.145	0.151	*0.313*
Child aged 11-15	0.114	0.111	*0.761*	0.129	0.128	*0.770*
Child aged 16-17	0.039	0.038	*0.831*	0.050	0.047	*0.363*
Child aged > 18	0.136	0.131	*0.534*	0.143	0.148	*0.327*
Married	0.257	0.265	*0.409*	0.296	0.293	*0.698*
Divorced	0.095	0.096	*0.881*	0.117	0.113	*0.501*
Years of sch	10.804	10.830	*0.426*	10.965	10.986	*0.460*
Less than 9 yrs	0.034	0.032	*0.666*	0.051	0.053	*0.453*
9 yrs	0.206	0.199	*0.460*	0.189	0.185	*0.601*
2-yr upp sec	0.614	0.620	*0.584*	0.541	0.541	*0.983*
Vocational	0.160	0.157	*0.781*	0.117	0.115	*0.649*
Social sci	0.045	0.047	*0.646*	0.023	0.023	*0.890*
Buisness	0.073	0.076	*0.624*	0.034	0.034	*0.863*
Technology	0.317	0.318	*0.878*	0.360	0.357	*0.764*
3-yr upp sec	0.093	0.093	*0.970*	0.128	0.121	*0.246*
Buisness	0.037	0.038	*0.873*	0.072	0.076	*0.381*
Tertiary < 3yrs	0.045	0.046	*0.782*	0.092	0.099	*0.142*
Tertiary 3 yrs	0.009	0.009	*0.840*	0.000	0.000	.
Regional employm.	0.725	0.725	*0.892*	0.721	0.721	*0.772*
Inland of Norrland	0.080	0.079	*0.903*	0.083	0.082	*0.958*
<u>County dummies</u>						
Stockholm	0.141	0.134	*0.368*	0.111	0.106	*0.278*
Uppsala	0.028	0.029	*0.730*	0.030	0.025	*0.056*
Södermanland	0.027	0.025	*0.627*	0.027	0.027	*0.969*
Östergötland	0.044	0.037	*0.097*	0.045	0.049	*0.334*
Jönköping	0.032	0.030	*0.681*	0.026	0.028	*0.373*
Kronoberg	0.012	0.015	*0.341*	0.013	0.011	*0.371*
Kalmar	0.030	0.039	*0.019*	0.037	0.034	*0.314*
Gotland	0.012	0.009	*0.214*	0.009	0.012	*0.062*

Blekinge	0.026	0.025	*0.702*	0.014	0.015	*0.767*
Skåne	0.140	0.140	*0.994*	0.122	0.120	*0.701*
Halland	0.027	0.027	*0.892*	0.026	0.026	*0.927*
Västra Götaland	0.157	0.162	*0.553*	0.159	0.167	*0.199*
Värmland	0.026	0.025	*0.958*	0.050	0.049	*0.766*
Örebro	0.026	0.028	*0.551*	0.029	0.028	*0.634*
Västmanland	0.030	0.033	*0.499*	0.032	0.030	*0.367*
Dalarna	0.037	0.038	*0.806*	0.055	0.061	*0.100*
Gävleborg	0.047	0.039	*0.089*	0.043	0.045	*0.627*
Västernorrland	0.043	0.041	*0.643*	0.031	0.033	*0.484*
Jämtland	0.029	0.029	*0.896*	0.017	0.017	*0.847*
Västerbotten	0.038	0.045	*0.125*	0.050	0.044	*0.110*
Norrbotten	0.048	0.048	*0.990*	0.073	0.074	*0.756*
No sector	0.421	0.434	*0.211*	0.356	0.372	*0.052*
Farming/Mining	0.012	0.010	*0.258*	0.017	0.016	*0.485*
Construction	0.061	0.057	*0.429*	0.120	0.116	*0.527*
Manufacturing	0.090	0.094	*0.537*	0.139	0.139	*0.914*
Finance/Insurance	0.076	0.074	*0.754*	0.089	0.087	*0.763*
Public sector	0.134	0.126	*0.253*	0.081	0.078	*0.488*
Other sector	0.206	0.205	*0.951*	0.198	0.192	*0.406*
Foreign born	0.149	0.150	*0.908*	0.157	0.144	*0.019*
Age at immigration	3.437	3.488	*0.794*	3.796	3.604	*0.217*
Parent>0 1990	0.058	0.064	*0.308*	0.056	0.061	*0.183*
Amount	0.698	0.860	*0.165*	0.678	0.688	*0.905*
Parent>0 1991	0.064	0.065	*0.876*	0.065	0.068	*0.446*
Amount	0.889	0.836	*0.667*	0.909	0.933	*0.815*
Parent>0 1992	0.060	0.063	*0.584*	0.066	0.060	*0.149*
Amount	0.953	0.918	*0.801*	1.048	0.985	*0.578*
Parent>0 1993	0.057	0.059	*0.751*	0.061	0.058	*0.543*
Amount	0.911	1.097	*0.208*	1.069	0.938	*0.234*
Parent>0 1994	0.055	0.057	*0.721*	0.057	0.058	*0.865*
Amount	1.058	1.107	*0.765*	1.083	1.124	*0.746*
Parent>0 1995	0.059	0.058	*0.897*	0.061	0.066	*0.146*
Amount	0.895	1.049	*0.333*	1.022	0.946	*0.519*
Sick leave>0 1990	0.762	0.769	*0.488*	0.765	0.759	*0.423*
Amount	9.568	10.031	*0.250*	10.317	10.113	*0.534*
Sick leave>0 1991	0.754	0.759	*0.547*	0.740	0.740	*0.989*
Amount	8.620	8.950	*0.451*	9.393	9.382	*0.977*
Sick leave>0 1992	0.377	0.377	*0.986*	0.371	0.365	*0.468*
Amount	5.204	5.399	*0.684*	6.607	7.316	*0.133*
Sick leave>0 1993	0.291	0.297	*0.511*	0.269	0.276	*0.340*
Amount	5.146	5.290	*0.772*	5.815	5.954	*0.751*
Sick leave>0 1994	0.197	0.192	*0.613*	0.189	0.186	*0.665*
Amount	5.154	5.410	*0.640*	6.135	5.498	*0.166*

Sick leave>0 1995	0.201	0.203	*0.843*	0.186	0.189	*0.677*
Amount	5.199	5.250	*0.923*	5.939	6.020	*0.863*
Social welfare>0 1990	0.153	0.148	*0.565*	0.146	0.150	*0.489*
Amount	1.497	1.436	*0.662*	1.354	1.370	*0.865*
Social welfare>0 1991	0.167	0.167	*0.982*	0.152	0.157	*0.381*
Amount	2.014	2.057	*0.801*	1.714	1.789	*0.533*
Social welfare>0 1992	0.179	0.183	*0.578*	0.159	0.165	*0.285*
Amount	2.124	2.403	*0.145*	1.740	1.852	*0.346*
Social welfare>0 1993	0.173	0.183	*0.236*	0.167	0.166	*0.775*
Amount	2.028	2.146	*0.500*	1.963	1.996	*0.800*
Social welfare>0 1994	0.168	0.172	*0.661*	0.156	0.163	*0.303*
Amount	1.756	1.818	*0.672*	1.596	1.625	*0.784*
Social welfare>0 1995	0.146	0.154	*0.309*	0.145	0.140	*0.456*
Amount	1.279	1.345	*0.574*	1.301	1.317	*0.861*
Unemp. ins.>0 1990	0.172	0.172	*0.959*	0.163	0.169	*0.285*
Amount	5.494	5.597	*0.769*	5.077	5.125	*0.848*
Unemp. ins.>0 1991	0.278	0.269	*0.388*	0.280	0.277	*0.649*
Amount	12.118	11.858	*0.640*	12.785	12.698	*0.844*
Unemp. ins.>0 1992	0.446	0.439	*0.547*	0.462	0.471	*0.289*
Amount	25.194	25.165	*0.971*	27.293	26.655	*0.304*
Unemp. ins.>0 1993	0.589	0.583	*0.569*	0.614	0.623	*0.293*
Amount	36.739	36.450	*0.743*	39.849	40.600	*0.276*
Unemp. ins.>0 1994	0.661	0.661	*0.935*	0.694	0.694	*0.982*
Amount	38.108	37.982	*0.877*	40.608	39.697	*0.143*
Unemp. ins.>0 1995	0.699	0.695	*0.654*	0.720	0.718	*0.792*
Amount	42.822	43.345	*0.554*	44.910	44.807	*0.878*
Earnings 1990	131.740	131.630	*0.954*	143.390	141.850	*0.282*
Earnings 1991	123.970	124.050	*0.968*	134.920	133.410	*0.313*
Earnings 1992	110.850	111.780	*0.642*	117.340	116.990	*0.822*
Earnings 1993	88.697	88.427	*0.891*	93.587	90.668	*0.061*
Earnings 1994	84.135	83.614	*0.783*	87.821	87.116	*0.637*
Earnings 1995	88.180	88.415	*0.903*	92.021	91.081	*0.534*
Zero earnings 1990	0.058	0.056	*0.731*	0.056	0.060	*0.358*
Zero earnings 1991	0.098	0.102	*0.545*	0.097	0.101	*0.422*
Zero earnings 1992	0.162	0.160	*0.788*	0.158	0.157	*0.932*
Zero earnings 1993	0.237	0.237	*0.995*	0.242	0.246	*0.557*
Zero earnings 1994	0.250	0.252	*0.854*	0.254	0.258	*0.626*
Zero earnings 1995	0.246	0.240	*0.473*	0.235	0.240	*0.554*
Days unemp 1992	159.320	159.730	*0.900*	157.230	159.220	*0.423*
Days unemp 1993	204.370	205.580	*0.716*	204.780	207.620	*0.253*
Days unemp 1994	213.120	215.100	*0.531*	216.090	213.420	*0.260*
Days unemp 1995	231.020	232.240	*0.700*	233.130	232.390	*0.754*
Max days unemp 1992	0.112	0.109	*0.611*	0.108	0.110	*0.805*
Max days unemp 1993	0.150	0.151	*0.994*	0.143	0.146	*0.619*

Max days unemp 1994	0.138	0.125	*0.076*	0.128	0.123	*0.403*
Max days unemp 1995	0.161	0.166	*0.518*	0.147	0.144	*0.692*
Zero days unemp 1992	0.358	0.353	*0.618*	0.359	0.352	*0.379*
Zero days unemp 1993	0.260	0.259	*0.871*	0.245	0.243	*0.749*
Zero days unemp 1994	0.214	0.206	*0.384*	0.191	0.197	*0.343*
Zero days unemp 1995	0.186	0.184	*0.865*	0.165	0.165	*0.987*

Note: Regional employment levels are gender specific. In 1990, sick leave benefits were paid from the first day of absence. This rule was changed in 1993 and only paid from the second day of sick leave absence. Variables recorded in 1996 are balanced when an extended model is applied. See text for further details.

Table A.3. Females, balancing tests, matched samples descriptive statistics. Two versions based on matching of probability estimates of AEI (left) or LMT (right).

	Pr[AEI]			Pr[LMT]		
	AEI	LMT	*p*-value	AEI	LMT	*p*-value
Age	35.393	35.417	*0.823*	37.347	37.556	*0.184*
Children	1.421	1.434	*0.423*	1.280	1.292	*0.579*
No child	0.262	0.254	*0.194*	0.321	0.320	*0.921*
1 child	0.248	0.256	*0.183*	0.256	0.257	*0.890*
2 children	0.329	0.324	*0.480*	0.286	0.282	*0.688*
3 children	0.130	0.135	*0.349*	0.106	0.104	*0.710*
4 children	0.031	0.031	*0.984*	0.026	0.029	*0.366*
> 4 children	0.000	0.000	.	0.006	0.008	*0.115*
Child aged 0-3	0.245	0.246	*0.867*	0.192	0.184	*0.247*
Child aged 4-6	0.282	0.293	*0.081*	0.233	0.239	*0.419*
Child aged 7-10	0.290	0.289	*0.923*	0.242	0.244	*0.741*
Child aged 11-15	0.223	0.217	*0.318*	0.211	0.216	*0.533*
Child aged 16-17	0.079	0.081	*0.604*	0.086	0.085	*0.907*
Child aged > 18	0.118	0.117	*0.853*	0.146	0.150	*0.570*
Married	0.395	0.388	*0.309*	0.401	0.407	*0.469*
Divorced	0.135	0.137	*0.717*	0.166	0.167	*0.950*
Years of sch	10.746	10.773	*0.165*	11.087	11.042	*0.162*
Less than 9 yrs	0.034	0.032	*0.454*	0.049	0.047	*0.632*
9 yrs	0.209	0.205	*0.508*	0.183	0.189	*0.365*
2-yr upp sec	0.624	0.629	*0.462*	0.496	0.498	*0.799*
Vocational	0.375	0.379	*0.527*	0.291	0.294	*0.710*
Social sci	0.057	0.057	*0.904*	0.042	0.043	*0.746*
Buisness	0.184	0.179	*0.349*	0.166	0.166	*0.970*
Technology	0.025	0.027	*0.407*	0.026	0.030	*0.261*
3-yr upp sec	0.089	0.085	*0.266*	0.162	0.166	*0.573*
Buisness	0.036	0.040	*0.138*	0.099	0.092	*0.243*
Tertiary < 3yrs	0.044	0.049	*0.122*	0.109	0.099	*0.068*
Tertiary 3 yrs	0.000	0.000	.	0.000	0.000	.
Regional employm.	0.692	0.692	*0.582*	0.693	0.693	*0.687*
Inland of Norrland	0.073	0.077	*0.277*	0.069	0.073	*0.458*
<u>County dummies</u>						
Stockholm	0.113	0.105	*0.087*	0.131	0.127	*0.502*
Uppsala	0.028	0.030	*0.418*	0.043	0.045	*0.597*
Södermanland	0.023	0.023	*0.916*	0.015	0.011	*0.054*
Östergötland	0.053	0.052	*0.643*	0.049	0.047	*0.580*
Jönköping	0.036	0.037	*0.955*	0.031	0.032	*0.669*
Kronoberg	0.015	0.013	*0.263*	0.011	0.011	*0.947*
Kalmar	0.033	0.033	*0.860*	0.038	0.038	*0.952*
Gotland	0.012	0.012	*0.923*	0.011	0.013	*0.247*

Blekinge	0.025	0.027	*0.597*	0.018	0.021	*0.331*
Skåne	0.130	0.132	*0.618*	0.115	0.118	*0.598*
Halland	0.023	0.021	*0.381*	0.017	0.015	*0.283*
Västra Götaland	0.168	0.167	*0.862*	0.137	0.135	*0.813*
Värmland	0.045	0.042	*0.302*	0.053	0.050	*0.536*
Örebro	0.035	0.037	*0.492*	0.038	0.038	*0.961*
Västmanland	0.032	0.033	*0.743*	0.046	0.047	*0.774*
Dalarna	0.041	0.042	*0.759*	0.049	0.054	*0.244*
Gävleborg	0.049	0.049	*0.801*	0.041	0.042	*0.709*
Västernorrland	0.042	0.048	*0.064*	0.035	0.036	*0.811*
Jämtland	0.018	0.019	*0.357*	0.016	0.016	*0.825*
Västerbotten	0.030	0.033	*0.216*	0.045	0.042	*0.482*
Norrbotten	0.049	0.046	*0.350*	0.061	0.060	*0.930*
No sector	0.343	0.346	*0.709*	0.359	0.365	*0.502*
Farming/Mining	0.007	0.007	*0.984*	0.008	0.008	*0.832*
Construction	0.006	0.006	*0.965*	0.008	0.007	*0.305*
Manufacturing	0.048	0.047	*0.710*	0.067	0.069	*0.672*
Finance/Insurance	0.060	0.057	*0.434*	0.081	0.080	*0.752*
Public sector	0.342	0.345	*0.639*	0.257	0.253	*0.636*
Other sector	0.194	0.192	*0.739*	0.219	0.219	*0.942*
Foreign born	0.126	0.129	*0.514*	0.160	0.166	*0.455*
Age at immigration	2.627	2.618	*0.933*	3.683	3.859	*0.301*
Parent>0 1990	0.229	0.233	*0.555*	0.191	0.193	*0.728*
Amount	8.529	8.997	*0.111*	7.259	7.307	*0.896*
Parent>0 1991	0.251	0.256	*0.418*	0.211	0.214	*0.695*
Amount	10.315	10.257	*0.862*	8.437	8.675	*0.557*
Parent>0 1992	0.274	0.272	*0.741*	0.224	0.229	*0.502*
Amount	11.521	11.842	*0.369*	9.520	9.724	*0.639*
Parent>0 1993	0.280	0.284	*0.500*	0.228	0.231	*0.757*
Amount	12.477	12.442	*0.928*	10.511	10.659	*0.759*
Parent>0 1994	0.271	0.275	*0.582*	0.217	0.211	*0.387*
Amount	11.697	11.648	*0.894*	9.300	9.087	*0.633*
Parent>0 1995	0.272	0.273	*0.875*	0.218	0.213	*0.516*
Amount	12.657	12.594	*0.872*	10.731	9.644	*0.024*
Sick leave>0 1990	0.797	0.798	*0.862*	0.775	0.778	*0.781*
Amount	9.499	9.670	*0.462*	9.700	9.887	*0.569*
Sick leave>0 1991	0.787	0.792	*0.404*	0.758	0.761	*0.745*
Amount	8.125	8.349	*0.331*	8.490	8.807	*0.345*
Sick leave>0 1992	0.454	0.458	*0.597*	0.427	0.427	*0.966*
Amount	5.351	5.724	*0.144*	6.264	6.285	*0.958*
Sick leave>0 1993	0.350	0.358	*0.187*	0.333	0.329	*0.654*
Amount	4.413	4.751	*0.151*	5.233	5.346	*0.759*
Sick leave>0 1994	0.276	0.275	*0.934*	0.270	0.272	*0.766*
Amount	4.709	5.157	*0.106*	6.139	6.126	*0.978*

Sick leave>0 1995	0.282	0.279	*0.662*	0.275	0.286	*0.195*
Amount	5.023	5.327	*0.270*	6.194	6.479	*0.517*
Social welfare>0 1990	0.144	0.144	*0.944*	0.138	0.135	*0.710*
Amount	1.046	1.028	*0.789*	1.178	1.152	*0.787*
Social welfare>0 1991	0.139	0.148	*0.098*	0.137	0.140	*0.624*
Amount	1.034	1.076	*0.494*	1.106	1.096	*0.906*
Social welfare>0 1992	0.146	0.155	*0.075*	0.145	0.145	*0.984*
Amount	1.064	1.113	*0.436*	1.163	1.153	*0.906*
Social welfare>0 1993	0.160	0.170	*0.059*	0.162	0.155	*0.322*
Amount	1.189	1.266	*0.247*	1.377	1.314	*0.535*
Social welfare>0 1994	0.148	0.147	*0.878*	0.150	0.158	*0.205*
Amount	1.018	1.062	*0.468*	1.298	1.315	*0.863*
Social welfare>0 1995	0.138	0.146	*0.137*	0.146	0.152	*0.312*
Amount	0.928	0.937	*0.876*	1.023	1.119	*0.234*
Unemp. ins.>0 1990	0.198	0.193	*0.338*	0.184	0.194	*0.171*
Amount	4.368	4.238	*0.449*	4.233	4.336	*0.654*
Unemp. ins.>0 1991	0.277	0.282	*0.379*	0.263	0.266	*0.705*
Amount	8.139	8.392	*0.318*	8.203	8.282	*0.820*
Unemp. ins.>0 1992	0.422	0.422	*0.952*	0.397	0.407	*0.250*
Amount	15.945	15.552	*0.274*	15.539	16.064	*0.281*
Unemp. ins.>0 1993	0.567	0.569	*0.689*	0.546	0.543	*0.689*
Amount	24.450	24.547	*0.818*	24.997	24.745	*0.665*
Unemp. ins.>0 1994	0.656	0.657	*0.915*	0.646	0.643	*0.793*
Amount	27.140	26.934	*0.613*	27.482	27.631	*0.789*
Unemp. ins.>0 1995	0.708	0.701	*0.309*	0.701	0.699	*0.816*
Amount	31.604	31.689	*0.849*	33.178	33.508	*0.593*
Earnings 1990	95.405	94.684	*0.433*	100.260	99.264	*0.439*
Earnings 1991	91.952	91.641	*0.735*	96.329	94.883	*0.263*
Earnings 1992	87.885	86.767	*0.258*	91.247	90.404	*0.539*
Earnings 1993	73.476	73.061	*0.674*	75.815	75.071	*0.586*
Earnings 1994	70.307	70.264	*0.965*	70.988	69.903	*0.419*
Earnings 1995	69.169	68.554	*0.509*	68.217	67.593	*0.632*
Max earnings rank	0.856	0.850	*0.515*	0.829	0.809	*0.139*
Zero earnings 1990	0.069	0.070	*0.901*	0.075	0.078	*0.536*
Zero earnings 1991	0.088	0.086	*0.664*	0.094	0.099	*0.319*
Zero earnings 1992	0.132	0.143	*0.025*	0.142	0.139	*0.640*
Zero earnings 1993	0.193	0.193	*0.975*	0.206	0.203	*0.717*
Zero earnings 1994	0.209	0.216	*0.215*	0.226	0.229	*0.694*
Zero earnings 1995	0.213	0.213	*0.891*	0.232	0.238	*0.508*
Days unemp 1992	136.060	137.780	*0.396*	131.990	132.880	*0.739*
Days unemp 1993	174.590	175.900	*0.526*	172.510	171.140	*0.620*
Days unemp 1994	195.010	196.070	*0.603*	193.550	192.600	*0.724*
Days unemp 1995	217.370	217.660	*0.883*	222.130	222.020	*0.966*
Max days unemp 1992	0.089	0.091	*0.695*	0.082	0.087	*0.300*

Max days unemp 1993	0.117	0.110	*0.141*	0.110	0.116	*0.302*
Max days unemp 1994	0.133	0.125	*0.119*	0.124	0.122	*0.682*
Max days unemp 1995	0.164	0.157	*0.211*	0.153	0.152	*0.892*
Zero days unemp 1992	0.405	0.396	*0.221*	0.414	0.413	*0.951*
Zero days unemp 1993	0.304	0.299	*0.386*	0.306	0.317	*0.208*
Zero days unemp 1994	0.234	0.226	*0.197*	0.233	0.231	*0.805*
Zero days unemp 1995	0.185	0.181	*0.446*	0.169	0.175	*0.373*

Note: Regional employment levels are gender specific. In 1990, sick leave benefits were paid from the first day of absence. This rule was changed in 1993 and only paid from the second day of sick leave absence. Variables recorded in 1996 are balanced when an extended model is applied. See text for further details.