

# **The long-term earnings consequences of general vs. specific training of the unemployed**

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# The long-term earnings consequences of general vs. specific training of the unemployed

by

Anders Stenberg<sup>a</sup> and Olle Westerlund<sup>b</sup>

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## Abstract

Training programs for the unemployed typically involve teaching a specific skill to ease the transition into employment. However, in 1997, the Swedish unemployed could choose general/theoretical training through enrollment in one year of full-time studies at the upper secondary school level. This study provides an empirical assessment of the relative earnings impact of general vs. specific training 13 years post-enrollment. In the long term, general training may compensate for the short-term relative earnings loss by enhancing the ability to adapt to changes in demand for skills. The analyses are based on population register data 1990-2010 and an unusually rich set of control variables. The results indicate that both programs are associated with earnings increases. Our relative program estimates reveal a short-term advantage of specific training that converges within 5-7 years. With a longer perspective, there is considerable heterogeneity in the relative earnings estimates. For females with short educations, the earnings increases following general training substantially exceed those following specific training.

Keywords: Active labor market programs, adult education, vocational training, general training.

JEL-codes: I21, J62, J68

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## Table of contents

|     |   |    |
|-----|---|----|
| 1   | Introduction .....  | 3  |
| 2   | Institutional setting .....                               | 7  |
| 3   | Theoretical considerations.....                           | 10 |
| 4   | Data .....  | 11 |
| 5   | Empirical strategy.....                                   | 18 |
| 5.1 | Difference-in-differences propensity score matching ..... | 18 |
| 5.2 | Relative program effects .....                            | 21 |
| 6   | Main results .....  | 23 |
| 7   | Heterogeneous effects .....                               | 31 |
| 8   | Summary .....   | 38 |
|     | References .....  | 40 |

## 1 Introduction

Training programs for the unemployed are offered by governments in most OECD countries. The course contents of these programs are typically oriented towards 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, say within a couple of years, specific skills are likely to better enhance re-entry into employment. However, in a longer perspective, general training may provide skills that are less sensitive to the changes in the labor market's demand for skills. Therefore, some economists suggest that governments should stimulate adults to enroll in formal schooling during economic downturns (e.g., Pissarides 2011). Studies of adults in general training programs have reported increasing average earnings returns eight to ten years after enrollment (Jacobson et al. 2003, 2005, Stenberg 2011, see Figure 1 and Figure 2).<sup>1</sup> As program effects vary across individuals and over time, these estimates are not directly comparable with evaluations of vocational training programs, but they do raise the question of whether the long term effects of general training would catch up with or exceed the earnings effects of specific training. There is an almost complete lack of empirical research on this topic, and so it is an open question whether skill adjustments among the unemployed should involve general training to a greater extent.<sup>2</sup> To address this gap in the literature, we explore data from a reform that saw large groups of the unemployed enroll in either formal schooling or a vocational/specific training program.

The purpose of this article is to evaluate the relative impact of general and specific training for the unemployed on their annual earnings over a follow-up period of 13 years. 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,

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<sup>1</sup> The results from short-run 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, but positive effects again in the late 1990s and early 2000s (e.g., Andrén and Gustafsson 2005, Calmfors et al. 2002, 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.

<sup>2</sup> A few studies deal with the economic efficiency of training programs relative to other labor market programs, e.g., job search assistance, public employment, and/or wage subsidies (Kluve 2010, Card et al. 2010, for Sweden see Forslund, Fredriksson and Vikström 2011 and Forslund, Liljeberg and von Trott zu Solz 2013).

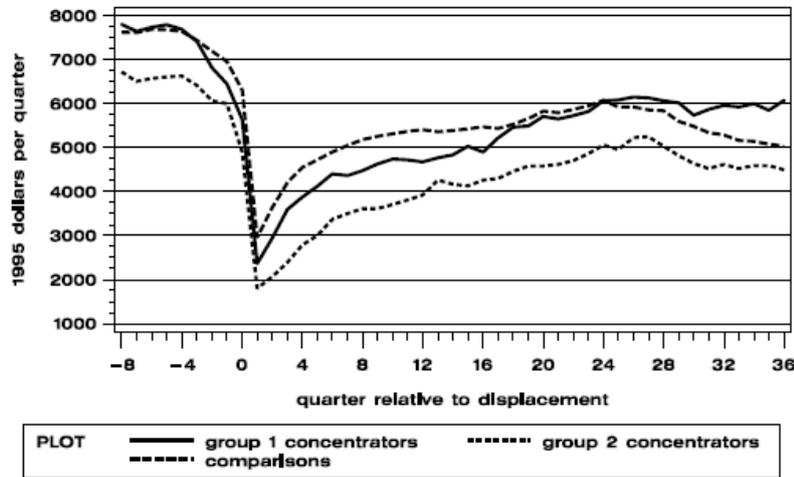
with a financial support equal to a maintained unemployment benefit. The adult schooling institutions were already in place, and the AEI attracted large numbers when the autumn semester of 1997 started in August. The sample studied concerns the unemployed individuals who enrolled in 1997 in either the AEI or the largest vocational training program in Sweden (*Arbetsmarknadsutbildning*), which we will refer to as “Labor Market Training” (LMT).

Using the population register data of annual earnings from 1990 until 2010, our empirical strategy is based on difference-in-differences propensity score matching, which explicitly takes into account heterogeneous treatment effects. The difference-in-differences outcome variable controls for individual time invariant (fixed) unobserved characteristics. The population register data are exceptionally rich, accounting for unemployment history and providing yearly information on various social insurance benefits. The estimates may still be biased if a confounding factor that is not captured by our empirical model influences both the decision to enroll in a program and future earnings. Technically, this can never be excluded. However, the robustness checks include controls for dynamic factors (changes) prior to program enrollment and, for males aged 25-44 in 1997 (born 1953 or later), measures of cognitive and non-cognitive skills allow us to check for potential ability bias. In addition, it is noteworthy that the empirical assessments of non-experimental estimators, both across meta-analyses (Card et al. 2010, Glazerman 2003, Greenberg et al. 2006) and when compared with the estimators derived from randomized experiments (Heckman et al. 1999, Heckman and Smith 1999, Smith and Todd 2005, Diaz and Handa 2006), have reported only a modest bias as long as the data used is of high quality. By most standards, the control variables in the present study are of very high quality. In this perspective, our empirical strategy is supported by the existing empirical evidence.<sup>3</sup>

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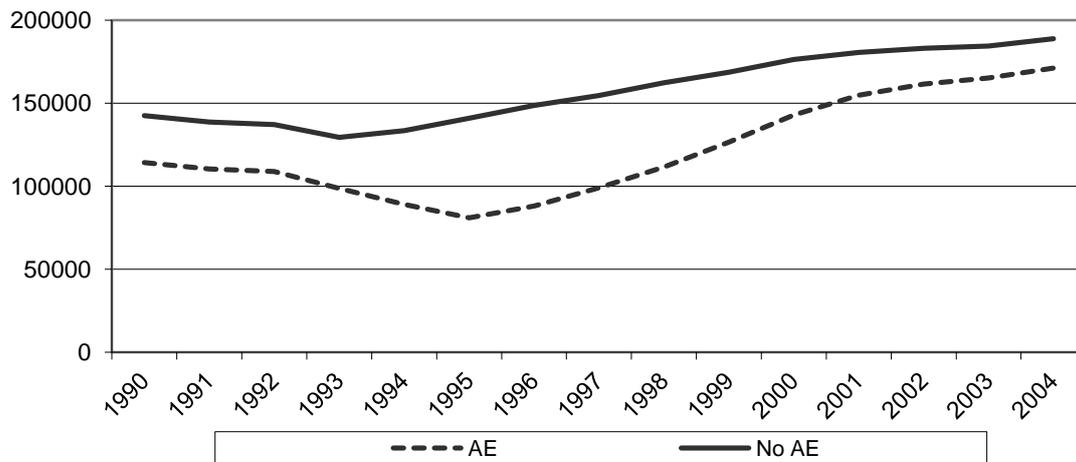
<sup>3</sup> In a seminal paper, Lalonde (1986) made rather pessimistic conclusions about non-experimental estimators. The above mentioned studies have shown that good data helps to avoid the main sources of bias.

Figure 1: Earnings trajectories of enrollees and non-enrollees in Community College, laid off workers aged 35 or over, Washington State



Source: Jacobson et al. (2003).

Figure 2: Earnings trajectories of enrollees and matched comparisons of non-enrollees in Komvux adult education (AE) 1994-1995



Source: Stenberg 2011.

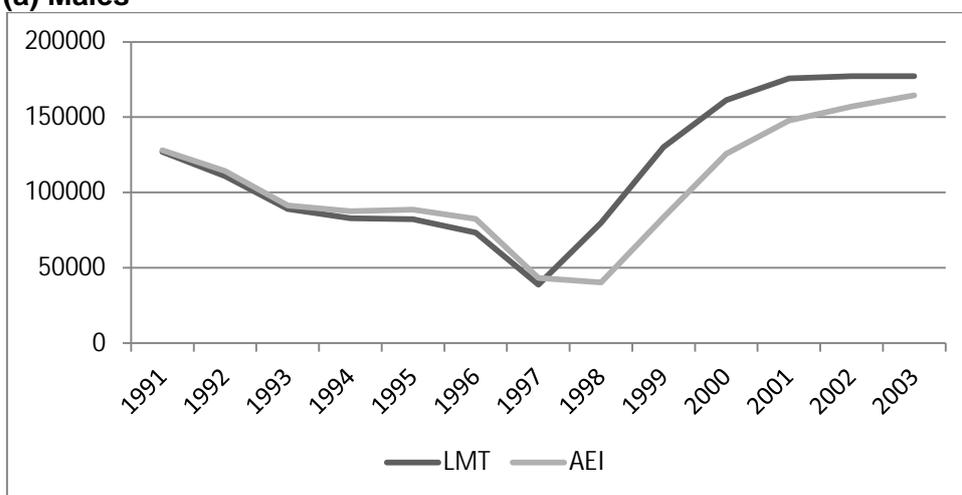
Research comparing general and specific training for the unemployed is scant. Stenberg (2007) is a study similar to the present one, but it analyzes only the annual earnings of the AEI and LMT participants from 1997 to 2003. The results were obtained with individual fixed effects estimates, i.e. basically relying on earnings and age as control variables. They confirmed 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 3(a)

and 3(b) demonstrate the earnings trajectories from raw data 1991-2003 (indicating somewhat smaller differences).

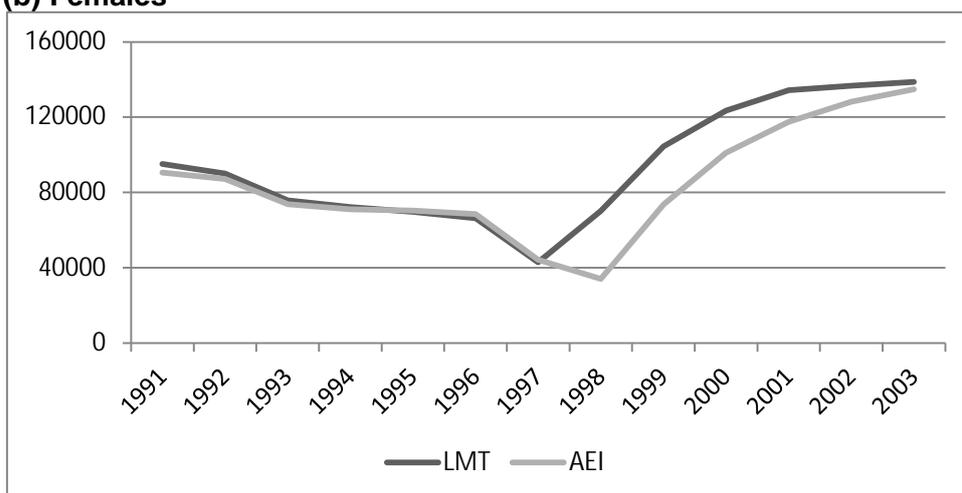
The contribution of the present study is to exploit data up to 13 years post enrollment, which is a follow up that is more than twice as long, to analyze the potential longer-term trends of the relative earnings impact of general versus specific training. Taking into account heterogeneous program effects, i.e. comparing comparable individuals of the two programs, the estimated relative average treatment effect on the treated indicates that specific training outperforms general training in the short run (5-7 years). In the longer perspective, 7-13 years after program enrollment, the estimates tend to become insignificantly different from zero. The analyses of subgroups reveal substantial heterogeneity in the estimated effects, which implies scope for potential efficiency gains by expanding the menu of training programs to include general training. This is particularly true for females with short educations. We also find 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 general skills.

Figure 3: Earnings trajectories of 1997 enrollees in AEI and LMT

**(a) Males**



**(b) Females**



Source: Stenberg (2007).

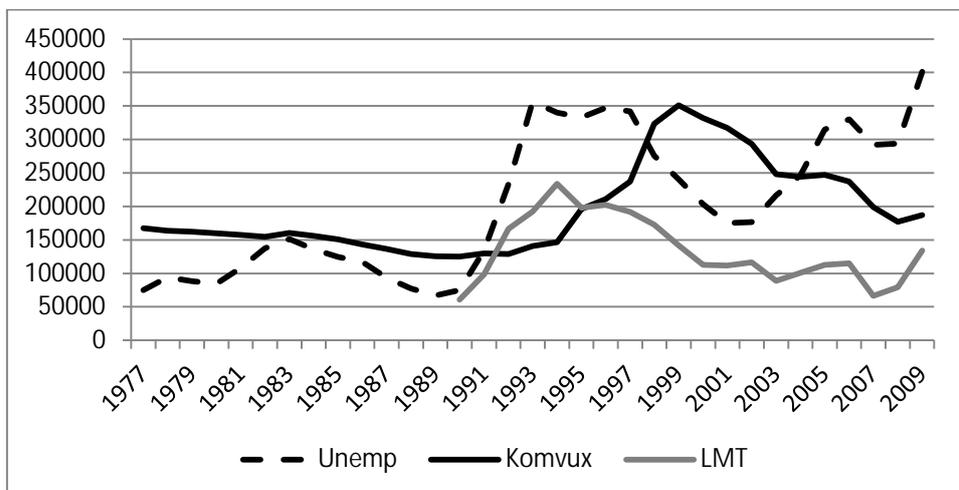
## 2 Institutional setting

In Sweden, young adults complete compulsory (comprehensive) school encompassing nine years with very limited tracking. This is followed by upper secondary school with one or two year programs, which are mainly vocational, and three year theoretical programs, which are intended as preparation for higher studies. Compared with continental Europe, there is a relatively modest gap in the educational contents between the vocational and theoretical programs. Since 1969, Swedish municipalities are 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*. The individuals at Komvux are aged 20 years or older and may be drop-outs who complete compulsory school or upper secondary programs. Others enroll to change the direction of their studies and/or to complete a three-year upper secondary diploma, 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. The numbers of individuals registered have been above 100,000 every year since the 1970s (including individuals only registered for one course). Importantly, prior to 1997, Komvux enrollment was rarely offered to unemployed individuals. This is partly explained by the fact that UI benefits are more generous than are study allowances (and not linked to repayment) and that this would generate incentives for individuals to register as unemployed before enrolling in Komvux. The vocational course 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).

Figure 4 shows that at the start of the 1990s, following an extreme recession which saw unemployment increase from 2 percent to 11 percent, the LMT grew to its largest size to date without reducing the open unemployment levels in any significant way. From 1993, 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 relatively generous financial support in the form of 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. Figure 4 illustrates how the AEI made the numbers in Komvux increase to unprecedented levels, exceeding 300,000 enrolled students.

Figure 4: Number of unemployed and enrollees in Labor Market Training and Komvux



Source: Statistics Sweden, various registers.

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 for individuals 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 per year for the LMT and SEK 34,000 per year for the AEI. This would correspond to similar costs to those of the AEI, and to simplify the analysis, we will disregard the direct program costs when assessing the relative payoff of the programs.<sup>4</sup>

<sup>4</sup> 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, as does the fact that vocational programs vary greatly in their costs, and we do not have access to information at the individual level. This is admittedly not ideal, and the main implications of our estimates in the empirical section must be considered with this reservation in mind.

### 3 Theoretical considerations

In his seminal work, Becker (1964) made a distinction between specific and general human capital. While this divide is often a subject of discussion, the concept has been at the basis of several hypotheses regarding the link between educational content and employment prospects. A commonly made assumption is that theoretical education enhances the ability to learn and provides skills that are of use in a more general sense. Employers may be more likely to offer further training to individuals with such skills, who may also become more flexible if confronted with technological changes, organizational changes or career changes (Brunello 2003, Shavit and Müller 1998).

In contrast, the individuals' comparative advantages speak in favor of offering various types of training. Vocational courses are easier to complete for individuals with low grades and/or who are less interested in theoretical subjects (Brunello och Checchi 2007). Vocational courses also have a more natural link to the labor market, which appears to be an intuitive explanation for the trajectories presented in Stenberg (2007) and reproduced here as Figure 3. In a longer perspective, there may be a risk attached to investments in specific skills, if unforeseen changes force individuals to switch careers, e.g., due to health reasons, or if there is a drop in demand for some professional skill such that job openings disappear and/or relative wages between professions change.<sup>5</sup> According to the arguments above, in times of, e.g., structural changes, theoretical education could enhance matching efficiency and work as an "insurance" against long spells of unemployment and/or against the need to re-enroll in schooling. In addition, theoretical education may also increase the individual's choice set by providing eligibility for further studies.

Time emerges as an important underlying aspect when discussing the future payoff of various types of training. If the degree of generality in education is negatively related to the short-term payoff and positively related to the long-term payoff, the net present value of general vs. specific training may differ for individuals depending on their individual discount rates and/or time preferences. To the extent that time preferences are positively correlated with cognitive ability or other relevant traits (Dohmen et al. 2010), one would perhaps expect individuals choosing general training to be associated with

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<sup>5</sup> It may also be that the individuals learn about the labor market or simply develop new preferences.

such characteristics.<sup>6</sup> In all, the heterogeneity of individuals and of the labor market demand for skills would favor variety in the supply of training, to capitalize on comparative advantages and improve the benefits of investments.

#### **4 Data**

This study is based on annual population register data 1990-2010 that encompasses all of the individuals residing in Sweden. To define our samples, the unemployment registers provide information on the day of enrollment in the LMT and the end date of this registration. We 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 January until the beginning of June (spring semester). For those enrolling in the AEI in the autumn of 1997, 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 (UBS) that was introduced in 1997 specifically for the AEI. This helps us distinguish between participants in the AEI and regular Komvux who attended the same courses (and in the same classrooms). Excluding the individuals registered in both programs in 1997 and those attending vocational courses within the AEI, the numbers registered in programs were 40,835 (LMT) and 46,227 (AEI); we refer to this as the *total sample*.

A large sample gains in external validity but at the cost of internal validity, because the estimated relative program effect may be diluted and/or strengthened by individuals who were registered in both Komvux and LMT during the 1990s. Therefore, for our benchmark analysis, we exclude individuals who were registered in any of the two programs in 1996. We also set the condition that in 1997, the individuals were registered as unemployed for at least one day between the 1<sup>st</sup> of January and the 30<sup>th</sup> of June and received UI benefits. These restrictions reduce the sample size to 17,149 (LMT) and 21,082 (AEI), i.e. about half of the total sample. This will be referred to as the *benchmark sample*.

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<sup>6</sup> It may be worth mentioning already at this stage that the results in Section 7 reveal that, when dividing a sample into two halves based on the cognitive test-scores above or below the median, the difference in the relative estimates 2003-2010 was very small, on average below SEK 300 (app. €30). The discussion on time preferences and other personality traits that are not directly observable is related to potential bias discussed further in Section 5.

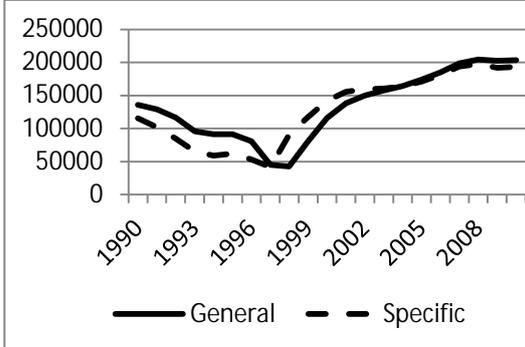
We also generated a third sample, limited to individuals who were never registered in either program 1991-1996 (our earliest record of LMT is 1991). This sample is “cleaner” but at the cost of external validity; the sample size is about one fifth of the total sample and approximately 15 percent of the total numbers enrolled. The number of observations is 8,576 (LMT) and 8,294 (AEI). We refer to this as the *limited sample*. In the empirical section, most of our analyses revolve around the benchmark sample, but results from limited samples are reported when relevant.

Figure 5 displays the trajectories of the AEI and LMT participants’ annual earnings from 1990-2010. The total sample trajectories demonstrate that, on average, the participants in the AEI had higher annual earnings from 1990-1996. The difference almost disappears if one conditions on the incidence of the UI benefits in 1997. Consequently, the figures pertaining to the benchmark samples show a remarkable similarity between the two programs. At face value, the earnings after enrollment among males indicate an advantage of the LMT, but the general training appears to be more beneficial for females.

Figure 5: Earnings trajectories of AEI (general) and LMT (specific) participant

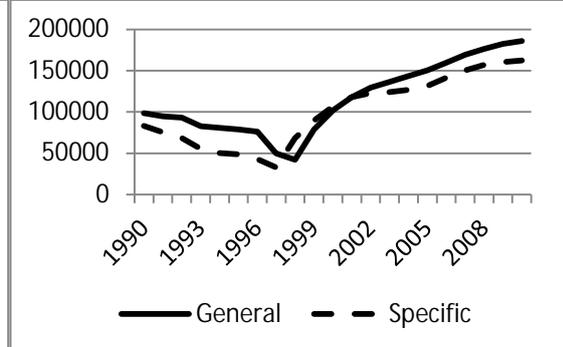
Total sample:

**Males**



$N^{AEI} = 11,245$  and  $N^{LMT} = 21,680$

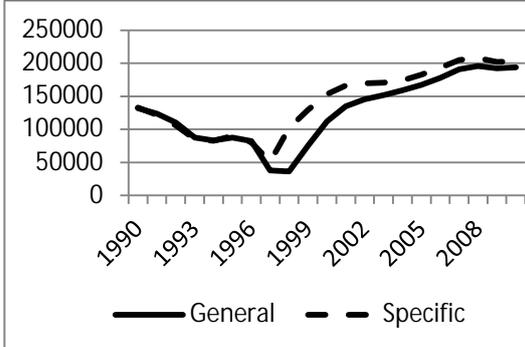
**Females**



$N^{AEI} = 34,982$  and  $N^{LMT} = 19,155$

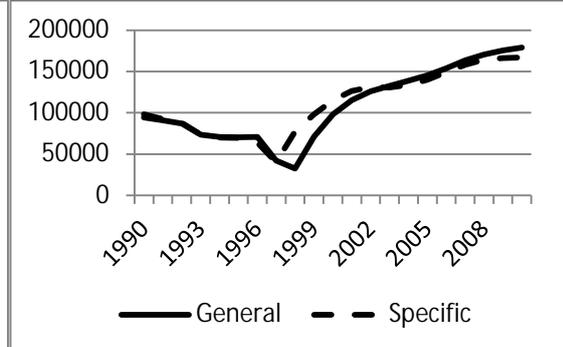
Benchmark sample

**Males**



$N^{AEI} = 4,245$  and  $N^{LMT} = 9,524$

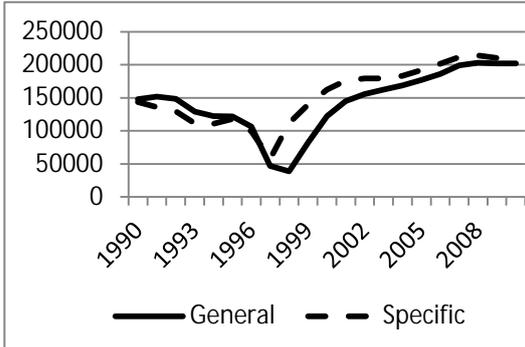
**Females**



$N^{AEI} = 11,854$  and  $N^{LMT} = 7,625$

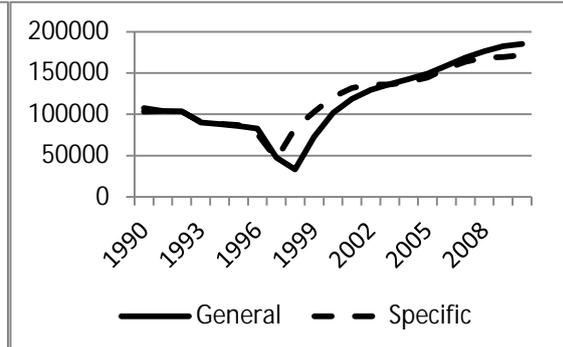
Limited sample:

**Males**



$N^{AEI} = 1,916$  and  $N^{LMT} = 5,747$

**Females**



$N^{AEI} = 6,378$  and  $N^{LMT} = 3,898$

Table 1 and Table 2 present the descriptive statistics on selected average characteristics of the male and female program participants, respectively (a more complete account of the available variables is given in Section 5). Most of the characteristics are significantly different between the two groups of program participants. The individuals in the AEI are about one year younger and slightly less associated with unemployment in 1996 than are those in the LMT. Among females, AEI participants were more often employed in the public sector, were more often on maternal leave and had more children at home than the LMT participants.

Table 1 also gives the descriptive statistics from military enlistment tests of cognitive and non-cognitive skills that are available for a subsample of males born in 1953 or later. The conventional view is that general training attracts individuals with higher ability, but the cognitive test scores are only barely significantly higher among the AEI individuals and the difference for non-cognitive skills is not statistically significant.

Table 3 describes the schooling completed by participants in the AEI, at lower secondary (compulsory) level, upper secondary level and tertiary (higher) education. This includes education completed until 2004.

Table 1: Males, descriptive averages of program participants

|  | Total sample |        | Benchmark sample |        | Limited sample |        | Matched comparisons;<br>benchmark sample <sup>a)</sup> |        |
|--|--------------|--------|------------------|--------|----------------|--------|--|--------|
|  | AEI          | LMT    | AEI              | LMT    | AEI            | LMT    | AEI  | LMT    |
| Age                                      | 34.65        | 34.19* | 35.05            | 35.35  | 35.23          | 34.86  | 35.08  | 35.17  |
| Married                                  | 0.27         | 0.30*  | 0.27             | 0.28   | 0.28           | 0.28   | 0.26   | 0.07   |
| No. of children at home                  | 0.83         | 0.94*  | 0.84             | 0.89*  | 0.84           | 0.88   | 0.83   | 0.01   |
| No upp. secondary school                 | 0.26         | 0.23*  | 0.26             | 0.22*  | 0.27           | 0.21*  | 0.26   | 0.24   |
| Public sector                            | 0.15         | 0.08*  | 0.13             | 0.08*  | 0.15           | 0.08*  | 0.13   | 0.14   |
| Stockholm area                           | 0.16         | 0.14*  | 0.15             | 0.11*  | 0.17           | 0.12*  | 0.14   | 0.14   |
| Inland of Norrland                       | 0.07         | 0.07   | 0.08             | 0.08   | 0.08           | 0.08   | 0.08   | 0.09   |
| Foreign born                             | 0.19         | 0.29*  | 0.17             | 0.17   | 0.13           | 0.12   | 0.15   | 0.15   |
| Cognitive skill test score <sup>b)</sup> | 4.34         | 4.29*  | 4.34             | 4.25*  | 4.41           | 4.29*  | 4.34   | 4.14*  |
| Non-cognitive skills <sup>b)</sup>       | 4.24         | 4.22   | 4.23             | 4.30   | 4.35           | 4.41   | 4.22   | 4.19   |
| Parental leave 1996                      | 0.08         | 0.05*  | 0.07             | 0.06*  | 0.08           | 0.06*  | 0.08   | 0.06*  |
| Parental leave 1995                      | 0.07         | 0.04*  | 0.06             | 0.05   | 0.07           | 0.05*  | 0.06   | 0.06   |
| Parental leave 1990                      | 0.06         | 0.03*  | 0.06             | 0.05*  | 0.06           | 0.04*  | 0.06   | 0.05   |
| Sick-leave 1996                          | 0.18         | 0.14*  | 0.19             | 0.16*  | 0.19           | 0.16*  | 0.20   | 0.18   |
| Sick-leave 1995                          | 0.19         | 0.14*  | 0.20             | 0.16*  | 0.20           | 0.16*  | 0.20   | 0.20   |
| Sick-leave 1990                          | 0.74         | 0.49*  | 0.74             | 0.64*  | 0.73           | 0.59*  | 0.76   | 0.76   |
| Social welfare 1996                      | 0.18         | 0.37*  | 0.17             | 0.17   | 0.14           | 0.14   | 0.16   | 0.17   |
| Social welfare 1995                      | 0.17         | 0.33*  | 0.16             | 0.16   | 0.13           | 0.13   | 0.15   | 0.15   |
| Social welfare 1990                      | 0.14         | 0.14*  | 0.15             | 0.13*  | 0.10           | 0.08*  | 0.15   | 0.15   |
| UI benefits 1996                         | 0.70         | 0.56*  | 0.80             | 0.85*  | 0.72           | 0.80   | 0.81   | 0.87*  |
| UI benefits 1995                         | 0.61         | 0.46*  | 0.69             | 0.68   | 0.54           | 0.54   | 0.70   | 0.70   |
| UI benefits 1990                         | 0.14         | 0.09*  | 0.17             | 0.14*  | 0.12           | 0.09*  | 0.17   | 0.18   |
| Days unemp. 1996                         | 222.9        | 265.6* | 255.6            | 267.9* | 215.7          | 241.3* | 256.2  | 276.4* |
| Days unemp. 1995                         | 206.1        | 213.4  | 230.1            | 226.8  | 172.3          | 176.8  | 231.2  | 232.3  |
| Days unemp. 1990                         | 137.4        | 113.5* | 156.1            | 139.8* | 87.5           | 78.8*  | 159.4  | 162.0  |
| Max days 1996                            | 0.17         | 0.20*  | 0.22             | 0.23   | 0.16           | 0.17   | 0.22   | 0.25*  |
| Max days 1995                            | 0.13         | 0.12*  | 0.16             | 0.14*  | 0.12           | 0.10*  | 0.16   | 0.16   |
| Max days 1990                            | 0.09         | 0.07*  | 0.11             | 0.09*  | 0.05           | 0.04   | 0.11   | 0.11   |
| Zero labor earnings 1996                 | 0.29         | 0.39*  | 0.26             | 0.25   | 0.18           | 0.18   | 0.27   | 0.29*  |
| Zero labor earnings 1995                 | 0.24         | 0.36*  | 0.25             | 0.22*  | 0.14           | 0.14   | 0.25   | 0.24   |
| Zero labor earnings 1990                 | 0.05         | 0.09*  | 0.06             | 0.06   | 0.04           | 0.05   | 0.06   | 0.06   |
| Registered Komvux 1991                   | 0.06         | 0.04*  | 0.06             | 0.04*  | 0              | 0      | 0.06   | 0.05   |
| Registered Komvux 1992                   | 0.07         | 0.06*  | 0.07             | 0.06*  | 0              | 0      | 0.07   | 0.07   |
| Registered Komvux 1993                   | 0.07         | 0.06*  | 0.05             | 0.04*  | 0              | 0      | 0.05   | 0.05   |
| Registered Komvux 1994                   | 0.10         | 0.09*  | 0.07             | 0.06*  | 0              | 0      | 0.07   | 0.06   |
| Registered Komvux 1995                   | 0.13         | 0.10*  | 0.06             | 0.04*  | 0              | 0      | 0.06   | 0.04*  |
| Registered Komvux 1996                   | 0.23         | 0.10*  | 0                | 0      | 0              | 0      | 0.00   | 0.00   |
| Komvux 1991-1996                         | 0.36         | 0.23*  | 0.17             | 0.13*  | 0              | 0      | 0.17   | 0.14*  |
| Registered LMT 1991                      | 0.10         | 0.08*  | 0.12             | 0.09*  | 0              | 0      | 0.12   | 0.12   |
| Registered LMT 1992                      | 0.15         | 0.13*  | 0.17             | 0.16*  | 0              | 0      | 0.18   | 0.18   |
| Registered LMT 1993                      | 0.09         | 0.08*  | 0.10             | 0.10   | 0              | 0      | 0.10   | 0.10   |
| Registered LMT 1994                      | 0.15         | 0.15*  | 0.17             | 0.16   | 0              | 0      | 0.17   | 0.17   |
| Registered LMT 1995                      | 0.16         | 0.18*  | 0.16             | 0.17   | 0              | 0      | 0.16   | 0.17   |
| Registered LMT 1996                      | 0.14         | 0.20*  | 0                | 0      | 0              | 0      | 0.00   | 0.00   |
| LMT 1991-1996                            | 0.49         | 0.52*  | 0.47             | 0.46*  | 0              | 0      | 0.48   | 0.43   |
| N  | 11,245       | 21680  | 4,245            | 9,524  | 1,916          | 4,678  | 4,138  | 5,893  |

Note: \* Indicates difference compared with untreated is significant at a 5 per cent level.

a) Variables recorded in 1996 are balanced when extended model is applied. Participation in program 1991-1995 is balanced when the limited sample is applied. See text for further details.

b) Measures of cognitive and non-cognitive skills are collected from military enlistment test scores, available for a subsample of 97,027 males born 1953 or later. The analyses in Figure 15 are based on 2,705 participants in the AEI and 5,747 in the LMT.

Table 2: Females, descriptive averages of program participants

|                          | Total sample |        | Benchmark sample |               | Limited sample |        | Matched comparisons;<br>benchmark sample <sup>a)</sup> |        |
|--------------------------|--------------|--------|------------------|---------------|----------------|--------|--|--------|
|                          | AEI          | LMT    | AEI              | LMT           | AEI            | LMT    | AEI  | LMT    |
| Age                      | 35.34        | 34.88* | <b>35.08</b>     | <b>36.54*</b> | 35.07          | 36.06* | 35.15  | 35.17  |
| Married                  | 0.42         | 0.39*  | <b>0.41</b>      | <b>0.38*</b>  | 0.43           | 0.38*  | 0.40   | 0.40   |
| No. of children at home  | 1.50         | 1.22*  | <b>1.50</b>      | <b>1.17*</b>  | 1.56           | 1.15*  | 1.49   | 1.50   |
| No upp. secondary school | 0.25         | 0.23*  | <b>0.25</b>      | <b>0.21*</b>  | 0.23           | 0.19*  | 0.25   | 0.24   |
| Public sector            | 0.44         | 0.22*  | <b>0.39</b>      | <b>0.26*</b>  | 0.43           | 0.27*  | 0.38   | 0.38   |
| Stockholm area           | 0.13         | 0.16*  | <b>0.11</b>      | <b>0.15*</b>  | 0.11           | 0.15*  | 0.11   | 0.11   |
| Inland of Norrland       | 0.07         | 0.06   | <b>0.07</b>      | <b>0.07</b>   | 0.07           | 0.07   | 0.07   | 0.07   |
| Foreign born             | 0.15         | 0.30*  | <b>0.14</b>      | <b>0.18*</b>  | 0.10           | 0.13*  | 0.13   | 0.14   |
| Parental leave 1996      | 0.24         | 0.15*  | <b>0.28</b>      | <b>0.17*</b>  | 0.30           | 0.20*  | 0.27   | 0.24*  |
| Parental leave 1995      | 0.27         | 0.19*  | <b>0.29</b>      | <b>0.20*</b>  | 0.34           | 0.24*  | 0.29   | 0.28   |
| Parental leave 1990      | 0.25         | 0.14*  | <b>0.24</b>      | <b>0.16*</b>  | 0.24           | 0.14*  | 0.24   | 0.24   |
| Sick-leave 1996          | 0.23         | 0.19*  | <b>0.26</b>      | <b>0.23*</b>  | 0.26           | 0.23*  | 0.26   | 0.24*  |
| Sick-leave 1995          | 0.26         | 0.20*  | <b>0.28</b>      | <b>0.25*</b>  | 0.29           | 0.26*  | 0.28   | 0.27   |
| Sick-leave 1990          | 0.78         | 0.52*  | <b>0.78</b>      | <b>0.68*</b>  | 0.79           | 0.66*  | 0.79   | 0.79   |
| Social welfare 1996      | 0.15         | 0.33*  | <b>0.15</b>      | <b>0.17*</b>  | 0.12           | 0.15*  | 0.15   | 0.17*  |
| Social welfare 1995      | 0.14         | 0.29*  | <b>0.14</b>      | <b>0.16*</b>  | 0.11           | 0.13*  | 0.14   | 0.15   |
| Social welfare 1990      | 0.13         | 0.14   | <b>0.14</b>      | <b>0.12*</b>  | 0.10           | 0.08*  | 0.14   | 0.15   |
| UI benefits 1996         | 0.62         | 0.58*  | <b>0.80</b>      | <b>0.85*</b>  | 0.75           | 0.81*  | 0.81   | 0.87*  |
| UI benefits 1995         | 0.54         | 0.46*  | <b>0.69</b>      | <b>0.67*</b>  | 0.58           | 0.54*  | 0.70   | 0.70   |
| UI benefits 1990         | 0.16         | 0.11*  | <b>0.20</b>      | <b>0.16*</b>  | 0.16           | 0.12*  | 0.20   | 0.19   |
| Days unemp. 1996         | 190.8        | 262.0* | <b>244.5</b>     | <b>272.3*</b> | 222.8          | 252.4* | 246.2  | 270.6* |
| Days unemp. 1995         | 170.4        | 203.2* | <b>214.5</b>     | <b>220.0*</b> | 177.3          | 176.8  | 215.9  | 217.7  |
| Days unemp. 1990         | 107.5        | 101.0* | <b>135.0</b>     | <b>122.7*</b> | 92.1           | 75.9*  | 136.0  | 134.7  |
| Max days 1996            | 0.15         | 0.20*  | <b>0.23</b>      | <b>0.25</b>   | 0.21           | 0.20   | 0.24   | 0.26*  |
| Max days 1995            | 0.12         | 0.12   | <b>0.16</b>      | <b>0.15*</b>  | 0.15           | 0.12*  | 0.16   | 0.16   |
| Max days 1990            | 0.07         | 0.06*  | <b>0.09</b>      | <b>0.07*</b>  | 0.06           | 0.04*  | 0.09   | 0.09   |
| Zero labor earnings 1996 | 0.22         | 0.38*  | <b>0.22</b>      | <b>0.25*</b>  | 0.17           | 0.19*  | 0.23   | 0.24*  |
| Zero labor earnings 1995 | 0.19         | 0.35*  | <b>0.20</b>      | <b>0.22*</b>  | 0.14           | 0.16*  | 0.21   | 0.21   |
| Zero labor earnings 1990 | 0.06         | 0.10*  | <b>0.07</b>      | <b>0.08*</b>  | 0.05           | 0.06*  | 0.07   | 0.06   |
| Registered Komvux 1991   | 0.09         | 0.08*  | <b>0.09</b>      | <b>0.08*</b>  | 0              | 0      | 0.09   | 0.08*  |
| Registered Komvux 1992   | 0.09         | 0.09*  | <b>0.09</b>      | <b>0.09*</b>  | 0              | 0      | 0.09   | 0.09   |
| Registered Komvux 1993   | 0.07         | 0.09*  | <b>0.07</b>      | <b>0.08*</b>  | 0              | 0      | 0.07   | 0.07   |
| Registered Komvux 1994   | 0.12         | 0.13*  | <b>0.09</b>      | <b>0.10*</b>  | 0              | 0      | 0.09   | 0.09   |
| Registered Komvux 1995   | 0.14         | 0.14*  | <b>0.07</b>      | <b>0.07*</b>  | 0              | 0      | 0.07   | 0.06*  |
| Registered Komvux 1996   | 0.23         | 0.16*  | <b>0.00</b>      | <b>0.00*</b>  | 0              | 0      | 0.00   | 0.00   |
| Komvux 1991-1996         | 0.39         | 0.34*  | <b>0.22</b>      | <b>0.22*</b>  | 0              | 0      | 0.23   | 0.21*  |
| Registered LMT 1991      | 0.07         | 0.06*  | <b>0.08</b>      | <b>0.07*</b>  | 0              | 0      | 0.09   | 0.08   |
| Registered LMT 1992      | 0.08         | 0.10*  | <b>0.10</b>      | <b>0.11*</b>  | 0              | 0      | 0.10   | 0.12*  |
| Registered LMT 1993      | 0.05         | 0.07*  | <b>0.06</b>      | <b>0.08*</b>  | 0              | 0      | 0.07   | 0.07   |
| Registered LMT 1994      | 0.10         | 0.12*  | <b>0.12</b>      | <b>0.13*</b>  | 0              | 0      | 0.12   | 0.12   |
| Registered LMT 1995      | 0.11         | 0.16*  | <b>0.12</b>      | <b>0.15*</b>  | 0              | 0      | 0.12   | 0.14*  |
| Registered LMT 1996      | 0.12         | 0.20*  | <b>0.00</b>      | <b>0.00*</b>  | 0              | 0      | 0.00   | 0.00   |
| LMT 1991-1996            | 0.35         | 0.47*  | <b>0.34</b>      | <b>0.38</b>   | 0              | 0      | 0.35   | 0.37*  |
|                          | N34,982      | 19,155 | 11,854           | 7,625         | 6,378          | 3,898  | 11,478   | 5,809  |

Note: \* Indicates difference compared with untreated is significant at a 5 per cent level.

<sup>a)</sup> Variables recorded in 1996 are balanced when extended model is applied. Participation in program 1991-1995 is balanced when the limited sample is applied. See text for further details.

Table 3: 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)                  | 0.883        | 1.112        |
| Fraction completing zero credits                                  | 0.150        | 0.103        |
| Fraction completing credits > 0 but < .25 years of AE             | 0.082        | 0.062        |
| Fraction completing credits > .25 but < .5 years of AE            | 0.115        | 0.085        |
| Fraction completing credits > .5 but < 1 year of AE               | 0.278        | 0.267        |
| Fraction completing more than 1 year of AE credits                | 0.376        | 0.483        |
| <b>Proportion registered in compulsory level courses</b>          | 0.291        | 0.278        |
| Registered compulsory credits, average                            | 0.263        | 0.217        |
| Completed compulsory credits, average                             | 0.077        | 0.073        |
| Completed compulsory credits, if registered at level              | 0.263        | 0.263        |
| <b>Proportion registered in upper secondary level courses</b>     | 0.919        | 0.951        |
| Registered upper secondary credits, average                       | 1.418        | 1.730        |
| Completed upper secondary credits, average                        | 0.799        | 1.028        |
| Completed upper secondary credits, if registered at level         | 0.870        | 1.081        |
| <i>Proportions in type of upper secondary course registration</i> |              |              |
| - English   | 0.749        | 0.718        |
| - Swedish   | 0.739        | 0.729        |
| - Mathematics   | 0.757        | 0.711        |
| - Social sciences   | 0.810        | 0.879        |
| - Natural sciences  | 0.368        | 0.377        |
| - Human sciences (e.g. foreign languages)                         | 0.160        | 0.217        |
| - Computer sciences   | 0.719        | 0.761        |
| - Health-related subjects (e.g. nursing)                          | 0.220        | 0.446        |
| - Vocational courses  | 0.000        | 0.000        |
| <b>Proportion registered in supplementary level courses</b>       | 0.024        | 0.027        |
| Registered upper supplementary credits, average                   | 0.013        | 0.022        |
| Completed upper supplementary credits, average                    | 0.007        | 0.011        |
| Completed upper supplementary credits, if registered at level     | 0.284        | 0.419        |
| <b>Proportion completing some tertiary level education</b>        | 0.139        | 0.171        |
| Completed tertiary education, average                             | 0.311        | 0.383        |
| Completed tertiary education, if registered at level              | 2.235        | 2.244        |
| <b>Total adult education completed (years)</b>                    | <b>1.186</b> | <b>1.484</b> |

## 5 Empirical strategy

In this section, we present our empirical strategy designed to assess the relative program effects of the AEI and LMT on annual earnings. In Section 5.1, we describe a conventional estimator of the average program treatment effects on the treated (ATT), using difference-in-differences propensity score matching (PSM). In this framework, the “untreated” are individuals in “no program,” and the estimates reflect the ATT of the AEI and LMT separately. The assumptions necessary for the identification of causality are discussed after the formal account of the PSM. In Section 5.2, we define the relative ATT estimator by interpreting the counterfactual state as “another program”. Because program effects are likely to be heterogeneous across individuals, separate estimates of the ATT for the programs are not necessarily comparable.

### 5.1 Difference-in-differences propensity score matching

The major advantage with the PSM is that the researcher explicitly controls the weights attached to the treated and untreated observations. It serves to compare comparable individuals and derive the ATT even if the treatment effects are heterogeneous across individuals. Formally, if a program occurs at time  $t$ , we compare the change in annual earnings  $(Y_{it+} - Y_{it-}) = \Delta Y$  of individuals in a program (treated = 1) with individuals not enrolled (untreated = 0). In our empirical implementation,  $t = 1997$ ,  $t^- = 1995$  and  $t+$  is 1998, 1999 and each year up to and including 2010. The difference-in-difference estimator may be written as  $(\Delta Y_1 - \Delta Y_0)$ , where subscript 1 denotes the program enrollment and 0 denotes no program enrollment. This set-up controls for the unobserved time invariant (fixed) characteristics affecting earnings.

Conditional on the observable characteristics  $X$ , we assume that the outcome is independent of the mechanisms determining program assignment  $D = 1$ , i.e.  $(DY_1 - DY_0) \perp D | X$ . The “curse of dimensionality” makes it difficult to find appropriate matches on more than a few  $X$  variables. An important result from Rosenbaum and Rubin (1983) is that if the above assumption holds, it also holds for some function of  $X$ , such that the matching is reduced to conditioning on a scalar:

$$(DY_1 - DY_0) \perp D | P(X)$$

The function  $P(X)$  is the propensity score, in our case a probit estimate of the probability of enrollment in a program. Each treated is matched with an untreated who is the nearest

neighbor in terms of the probit estimate.<sup>7</sup> One-to-one matching with replacement minimizes bias. Increasing the number of matches improves the precision at the cost of potential bias. Given that the treated and their matched comparisons are balanced on all variables in  $X$ , the ATT is given by the average treated-untreated difference in  $\Delta Y$  for the balanced samples.<sup>8</sup>

A common critique against difference-in-difference estimators is that a temporary earnings drop in the year prior to program enrollment among the treated generates an upward bias because the earnings level does not reflect the individual's true productivity (Ashenfelter 1978). Therefore, our outcome variable does not consider earnings in 1996, with pre-program earnings defined as the average of the annual earnings 1993-1995. Our control variables also disregard observations on earnings and transfers post-1995. This is the baseline model we use in the results section unless otherwise stated. A contrasting approach is to view changes in earnings or transfers 1995-1996 as implying changes with permanent effects for which it is necessary to control (e.g., Heckman and Smith 1999, Heckman et al. 1999). As a robustness check, we also estimated our models using extended versions where earnings in 1996 and changes in transfers are considered.<sup>9</sup> These results are discussed when relevant in the empirical section.

Our balancing tests of the explanatory variables cannot reject equality of means between the treated and the matched comparisons. This holds throughout, for all of the estimates discussed in the empirical section.<sup>10</sup> For a selection of variables, the rightmost columns in Table 1 (males) and in Table 2 (females) present balancing tests pertaining to the benchmark sample estimates, baseline model specification (the extended model also balances samples on variables recorded in 1996). In all, these balancing tests encompass an unusually rich set of covariates that include age cohort (30 categories), prior education (6), employment sector (7), residing in rural or metropolitan area (3), number of children at home (6), age of children (6), indicators of marital status or

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<sup>7</sup> In practice, irrelevant covariates are excluded from the probit estimates because they may increase bias and/or variance of matching estimators (e.g. de Luna et al. 2011). Variables are discarded if  $p$ -values are above .2 unless they are essential for the balancing of the samples.

<sup>8</sup> Balancing the samples was at times difficult with one-to-one matching without "trimming" the samples (frequently excluding approximately 20 percent of a program). Therefore, the results presented in the empirical section are based on four-to-one matching, in general similar to the one-to-one matching estimates but avoiding trimming.

<sup>9</sup> In line with Heckman and Smith (1999), we then also balance on transitions in labor force status between 1995 and 1996. This concerns nine different transitions between outside the labor force, employment and unemployment. Also included are indicator variables of newly married or divorced in 1996 and 1997, changes in the amounts of social insurance benefits in 1995-1996 and regarding sick-leave, early retirement or social welfare also for 1996-1997. With benefit payments in 1997 among the covariates, we must assume that the AE does not cause them to increase.

<sup>10</sup> The complete balancing tests are available from the authors on request.

divorce, pre-treatment annual earnings trajectories for 1990-1995 (1996), and five different types of social insurance benefits in 1990-1995 (1996) related to unemployment insurance, parental leave, sick-leave, social welfare and early retirement, applying both dummy variables (zero earnings, incidence of the various benefits) and continuous measures of amounts. We further balance the treated and matched comparisons on days registered as unemployed each year 1992-1995 (1996) or on indicator variables if either zero days or the maximum (365/366) number of days. In total, our balancing tests encompass 137 variables.

To give our estimates of the 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, the mechanisms behind enrollment decisions are independent of future earnings.

In the present case, assumption *ii* can be questioned because both programs are large. In the short run, competition for vacant slots was likely reduced when the AEI absorbed a large number of potential job-seekers. However, for the long-term overall implications of our results, which are our primary interest, it appears reasonable to assume that this is of negligible importance.<sup>11</sup>

The crucial assumption is *iii*. Even with our rich set of covariates, it is not possible to rule out that some unobserved factor(s) may correlate with both participation and future earnings. As the difference-in-differences outcome takes into account the time-invariant unobservable factors, the main threat to our identification is time-varying unobservable characteristics. For example, individuals who have lost motivation or (re)gained motivation may be over- or under-represented in a group of program participants.<sup>12</sup> Our extended model specification described above, which adds controls for pre-program changes in earnings and transfers, provides one robustness check as to whether this is a small or large problem. We generally find negligible differences in our estimates when altering between model specifications. Another concern is the unobserved ability differences between the two groups of program participants. As an assessment of

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<sup>11</sup> For active labor market programs in Sweden in 1987-1996, Dahlberg and Forslund (2005) find no displacement effects of the training programs for the unemployed (but substantial displacement effects for 65 percent-subsidized employment). Related to program effects on the untreated, Albrecht et al. (2009) argue that there were positive general equilibrium effects of the AEI, which increase the returns to society of the program by a factor of 1.5 compared with the earnings return of the participating individuals.

<sup>12</sup> 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).

potential ability bias in our estimates, we use a sample of males born 1953 or later, where we compare the results when including and excluding test scores relating to cognitive and non-cognitive skills. There are then only minor changes in our estimates, which correspond to .2 percentage points of the annual earnings. The changes are even smaller when using the extended model. In more general terms, studies comparing estimates based on experimental and high-quality nonexperimental data, referred to in the introduction, render some comfort as they do not report any systematic bias.<sup>13</sup>

## 5.2 Relative program effects

For the relative ATT, one may apply the same reasoning as in the case of the ATT discussed above, but consider  $D = 1$  the “default program” and  $D = 0$  as an alternative labor market program (instead of “no program”). We thereby compare 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  $\Delta Y$  of program participants of the same age, where the age variable has been balanced between the two groups.

The distinction between treatments merits some attention, i.e. the difference between matching based on estimates of  $\Pr[\text{AEI}]$  or  $\Pr[\text{LMT}]$  in the probit step. Figure 6 displays the distribution of propensity scores when using the different set-ups. These are not symmetric probabilities and therefore represent different weighting algorithms that address slightly different hypotheses.<sup>14</sup>

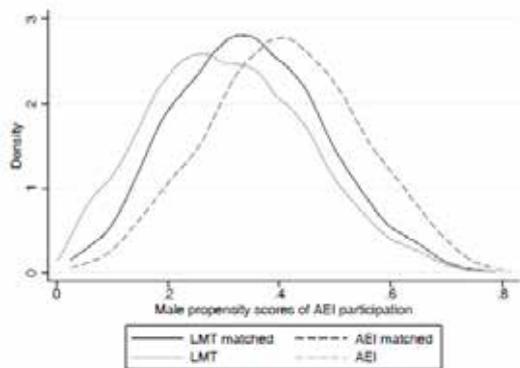
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<sup>13</sup> Card et al. (2010) conclude that “*The absence of an “experimental” effect suggests that the research designs used in recent non-experimental evaluations are not significantly biased relative to the benchmark of an experimental design*” (F475, their quotation marks).

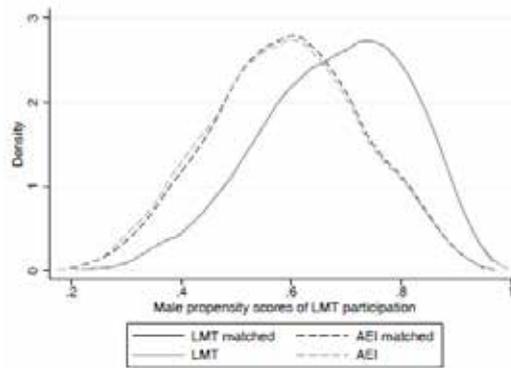
<sup>14</sup> An OLS estimator is perfectly symmetrical and switching between AEI and LMT indicators just switches the sign of the coefficient. To minimize the sum of squared error terms, the OLS weighting system is positively related to how often a value of an X variable occurs and to the variation in treatment for this value (Angrist 1998).

Figure 6: Distribution of propensity score estimates, using Pr[AEI] and Pr[LMT] respectively (note; curves largely overlap for “treatment” (default) programs)

### Males

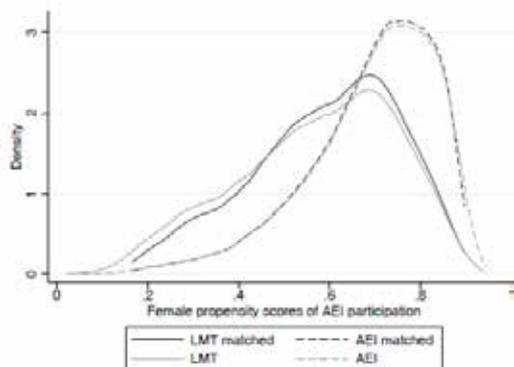


$N^{AEI} = 4,138$  and  $N^{LMT} = 5,893$  (weighted)

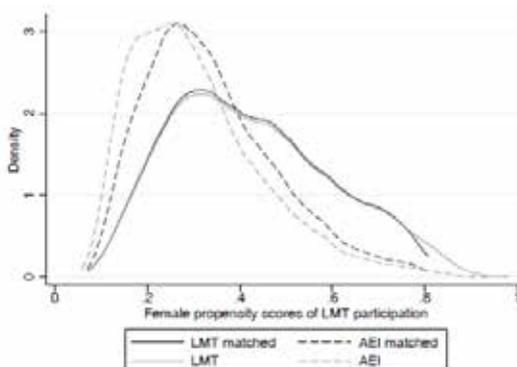


$N^{LMT} = 7,503$  and  $N^{AEI} = 3,970$  (weighted)

### Females



$N^{AEI} = 11,478$  and  $N^{LMT} = 5,809$  (weighted)



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

To see this, let us assume that all individuals have decided to enroll in a program and that they choose freely between only two existing programs; the AEI and the LMT. If we are interested in evaluating a counterfactual world where only the LMT exists, the Pr[AEI] set up tests whether the AEI is associated with higher earnings compared with the LMT for those choosing the AEI. Conversely, the Pr[LMT] set up evaluates the earnings of the LMT relative to the AEI for those enrolled in the LMT. These would give the same estimates if the assignment to programs were symmetric. To the extent that there are asymmetries, greater weights are given to those with a relatively high probability of participation in the “treatment program” (according to the probit estimates). If there are heterogeneous relative program effects where individuals act on the expected returns of the program, one would expect estimates to reflect the comparative advantage of the observed treatment, i.e. to favor the “default” program. In the empirical section, the results presented concern both of these alternatives.

There are two ways to interpret a difference in unobserved ability between the respective program participants. First, a main argument in favor of AEI-like programs is that the unemployed may have some ability (comparative advantage) that makes them more suitable for theoretical programs. This could be reflected in our relative program estimates and would be the effect we are interested in. However, if this ability influences the outcome *independently* of program participation, and is not captured by the covariates, it may yield bias in our estimates of the relative ATT. This would be related to the discussion above concerning selection on unobservables.

Given that policies across OECD countries primarily offer LMT-like programs, with AEI types of programs barely existing, one would perhaps either expect results to be in favor of the LMT regardless of the set up or claim that there are other obstacles explaining why AEI-type programs do not exist.

## 6 Main results

The results presented in this section primarily concern the benchmark sample estimates (see Section 4), using the baseline empirical model specification as defined above. For each result, there are also estimates pertaining to the extended model specification and/or the limited samples. These results are considered as robustness checks but are not discussed if they confirm the main implications of our findings.<sup>15</sup>

First, to provide an idea about the impact of each program separately, Figure 7 displays the estimated ATT separately for the AEI and the LMT. The matched comparisons were here taken from the pool of individuals registered as unemployed in 1997 but *not* registered in either the LMT or the AEI.<sup>16</sup> The results imply positive estimates for both programs, sometimes only borderline significant at a five percent level (displayed in Figure 7). General training displays an incremental earnings payoff whereas specific training has a relatively large payoff in the years immediately following program participation, which tapers off to about half.

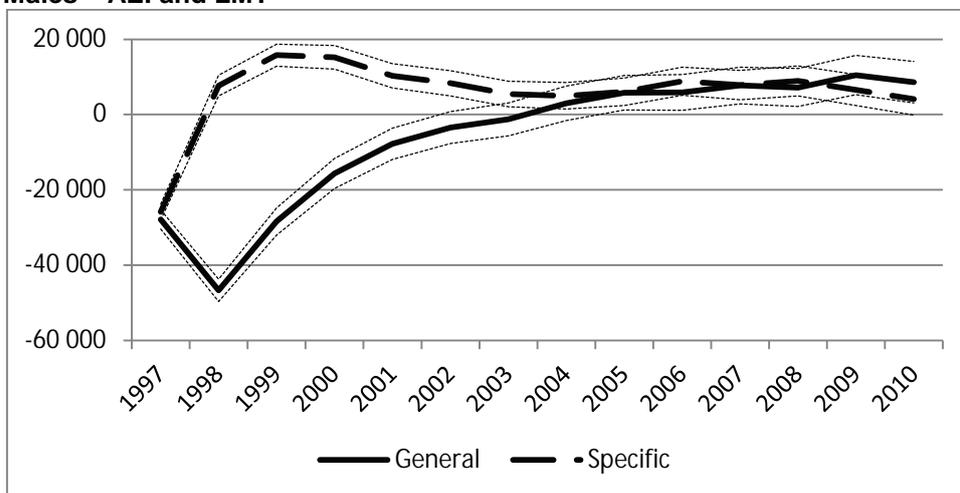
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<sup>15</sup> Complete results are available from the authors on request. The total sample estimates are left out here because balancing was difficult to achieve and the program impacts likely are diluted.

<sup>16</sup> 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. Calmfors et al. 2002 for LMT, Stenberg 2011 for Komvux. See also references given in footnote 1..

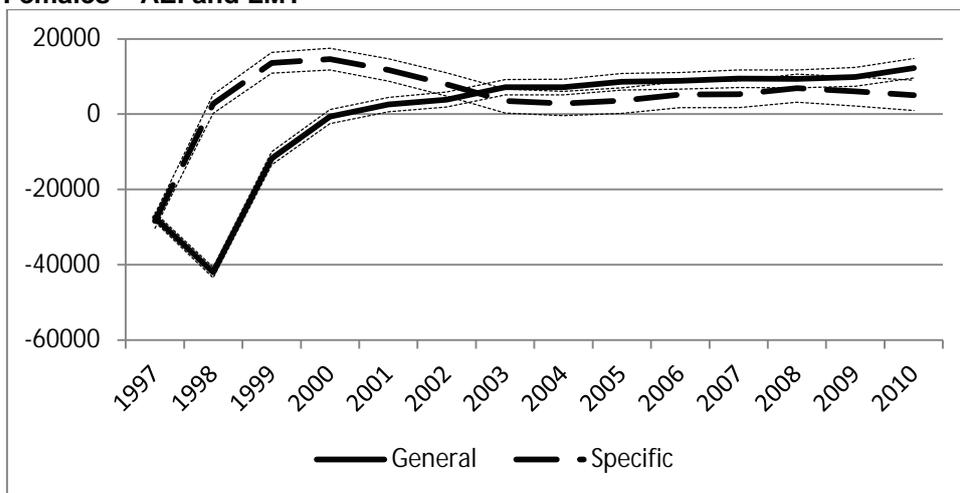
Figure 7: Difference in difference estimates of program effects on annual earnings, separately estimated for the AEI (general) and LMT (specific)

**Males – AEI and LMT**



AEI:  $N^{TREATED} = 4,088$  and  $N^{UNTREATED} = 14,223$  (weighted).  
 LMT:  $N^{TREATED} = 6,682$  and  $N^{UNTREATED} = 22,336$  (weighted).

**Females – AEI and LMT**



AEI:  $N^{TREATED} = 11,583$  and  $N^{UNTREATED} = 32,158$  (weighted).  
 LMT:  $N^{TREATED} = 6,698$  and  $N^{UNTREATED} = 21,868$  (weighted).

Turning now to the relative program estimates, Figure 8 (males) and Figure 9 (females) present the difference-in-differences estimates of the earnings disparity between participants of the AEI and the LMT, where only matched individuals are considered, i.e. a subset of comparable individuals from each program. For both males and females, there is an initial and large drop in the relative earnings among the AEI participants, with estimates tending to converge thereafter. When one alters the set up for the estimation of the propensity score (i.e. switching the binomial indicator for the

dependent variable in the probit), the estimates change slightly in the expected direction and favor the program chosen as the “treatment” indicator. A rough summary of the results is that when the matched samples are derived from Pr[AEI], estimates of the relative treatment effects for males tend to converge and the estimates for females are significantly above zero from 2003 and onwards. These findings are shifted downwards when the matching is based on estimates of Pr[LMT], becoming below zero for males and hovering close to zero for females.

For males, the estimates based on limited samples and employing the extended model specification strengthen the impression that the LMT seems a more efficient tool to enhance average earnings for unemployed males. By the time the last cohort retires at age 65, even if we extrapolate into future years the largest estimate of the relative impact of the AEI (SEK 6,400, limited sample), the relative excess earnings returns of general training would still not cover half the initial relative earnings loss 1998-2004 (recall that the direct costs are assumed equal for the two programs). The extrapolation assumes a two percent discount rate and that everyone retires at age 65, taking into account the age structure of the samples. We will apply this simple framework repeatedly below to assess what the estimates imply for the net benefits from society’s perspective.<sup>17</sup>

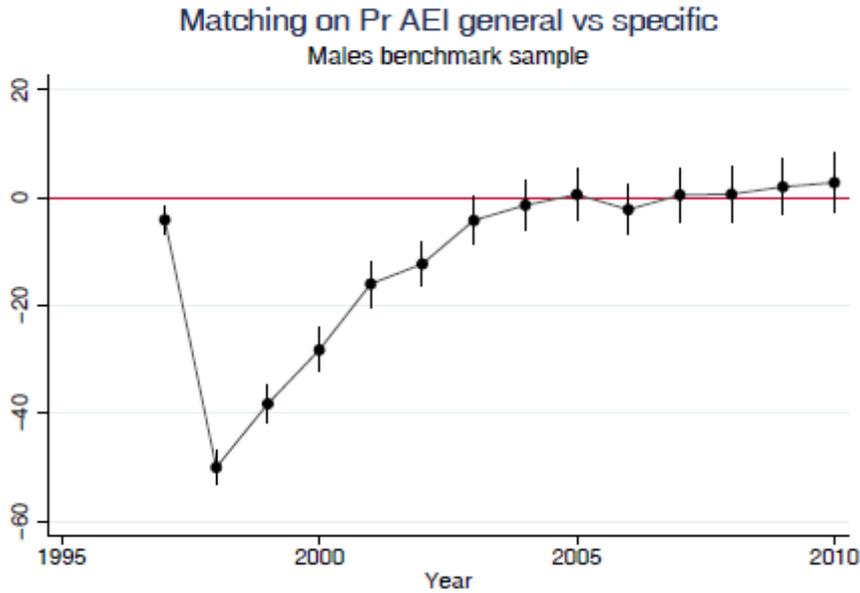
For the sample of females, the estimates based on Pr[AEI] indicate a relatively positive impact of general training in 2003-2010, which is significantly different from zero. Applying the above used framework to extrapolate these estimates implies that the initial relative earnings losses (costs) in 1998-2002 are recovered by approximately 2025.<sup>18</sup> The youngest cohort in the sample is then 53 years old, and about half of the individuals are still aged below 65. The corresponding calculations based on the limited sample estimates imply a slightly shorter time to recover costs, by approximately 2020, but the estimates based on Pr[LMT] provide no support that theoretical programs would be beneficial in the long term.

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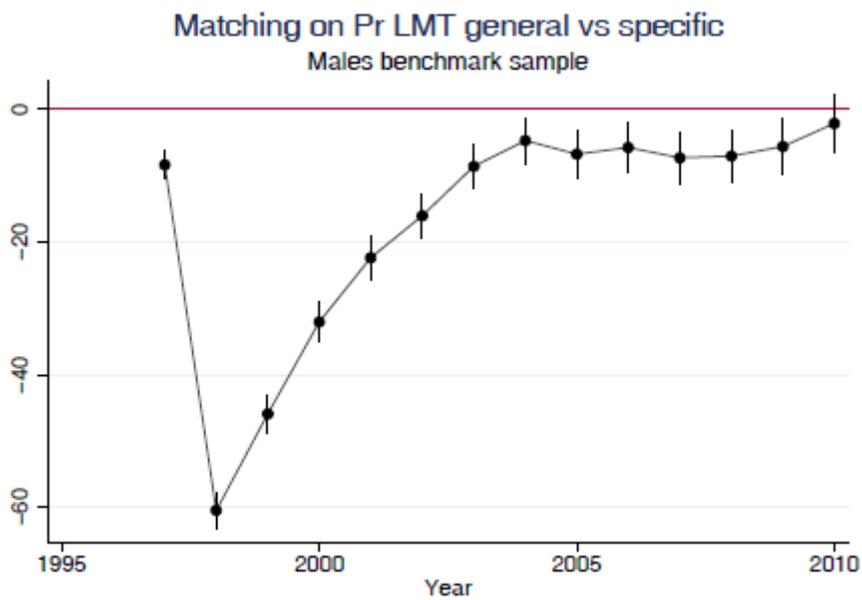
<sup>17</sup> We fully acknowledge that this may be developed further, not least by making sensitivity analyses regarding the assumptions made, by considering externalities and other side-effects of education. However, simplification is necessary to make the discussion here intelligible, and because we are in relatively unexplored territory, the priority is to establish the qualitative results rather than to pin down the precise numbers of the specific estimators. Most importantly, we do not believe our account of the results is misleading.

<sup>18</sup> The magnitude of this loss (app. SEK 80,000) is only about half that of the males.

Figure 8: Male difference-in-differences (SEK in 1000s) propensity score matching estimates, benchmark samples

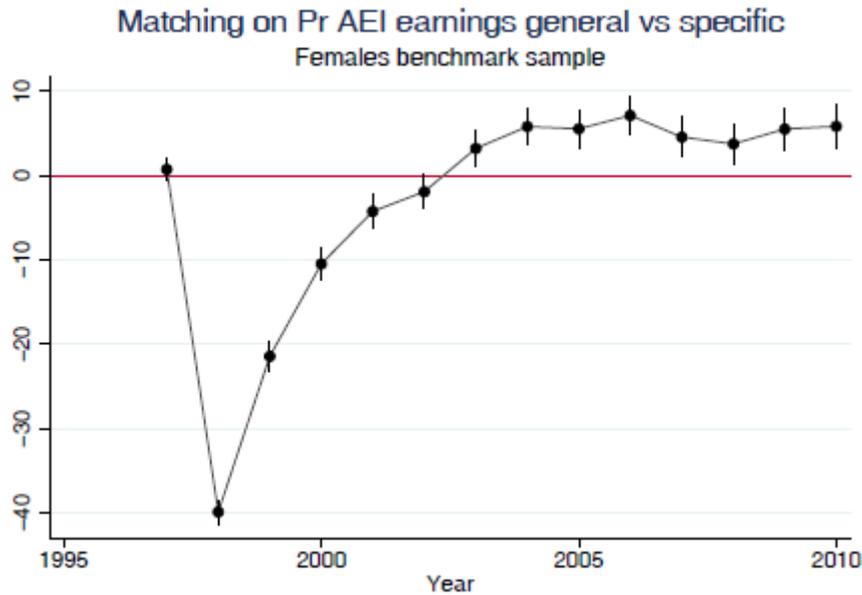


$N^{AEI} = 4,138$  and  $N^{LMT} = 5,893$  (weighted)

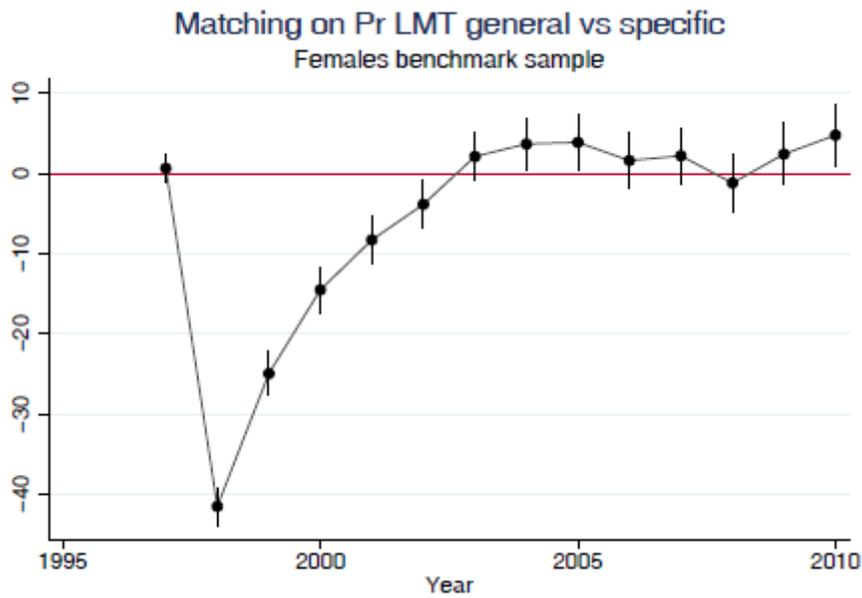


$N^{LMT} = 7,503$  and  $N^{AEI} = 3,970$  (weighted)

Figure 9: Female difference-in-differences (SEK in 1000s) propensity score matching estimates, benchmark samples



$N^{AEI} = 11,478$  and  $N^{LMT} = 5,809$  (weighted)



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

Thus far, the results imply that the earnings effects of general training catch up, but there are only weak indications that they would be sufficient to compensate or exceed the short-run relative earnings losses vis-à-vis specific training (LMT). The question one might ask is whether there are some groups for which AEI type of programs would be preferable compared with the LMT. To get a first impression of potential heterogeneity in the relative treatment effects, we follow the stratification method proposed by Xie et al. (2012, p323) and display estimates for groups divided into quartiles of the propensity scores in Figure 10 (males) and Figure 11 (females). These indicate substantial heterogeneity in the relative program effects and that some groups are associated with relatively beneficial effects of the AEI. However, this exercise may be of little policy relevance, because the propensity score is not observable to the individual or the case worker (unless the employment office would use propensity score techniques to target individuals for programs). In the next section, we analyze the extent to which this heterogeneity is linked to easily observable individual characteristics.<sup>19</sup>

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<sup>19</sup> The probit estimates for our matching procedures (not displayed) indicate that enrollment in the AEI for males is correlated positively with a professional track in upper secondary school, employment in public sector, residing in the Stockholm area, and a full year registered as unemployed, but negatively correlated with children at home and annual earnings. For females, AEI enrollment is positively correlated with children at home and public sector employment, but negatively correlated with age, a business track in upper secondary school and residing in the Inland of Norrland.

Figure 10: Males – benchmark sample estimates by quartiles of the propensity score distribution

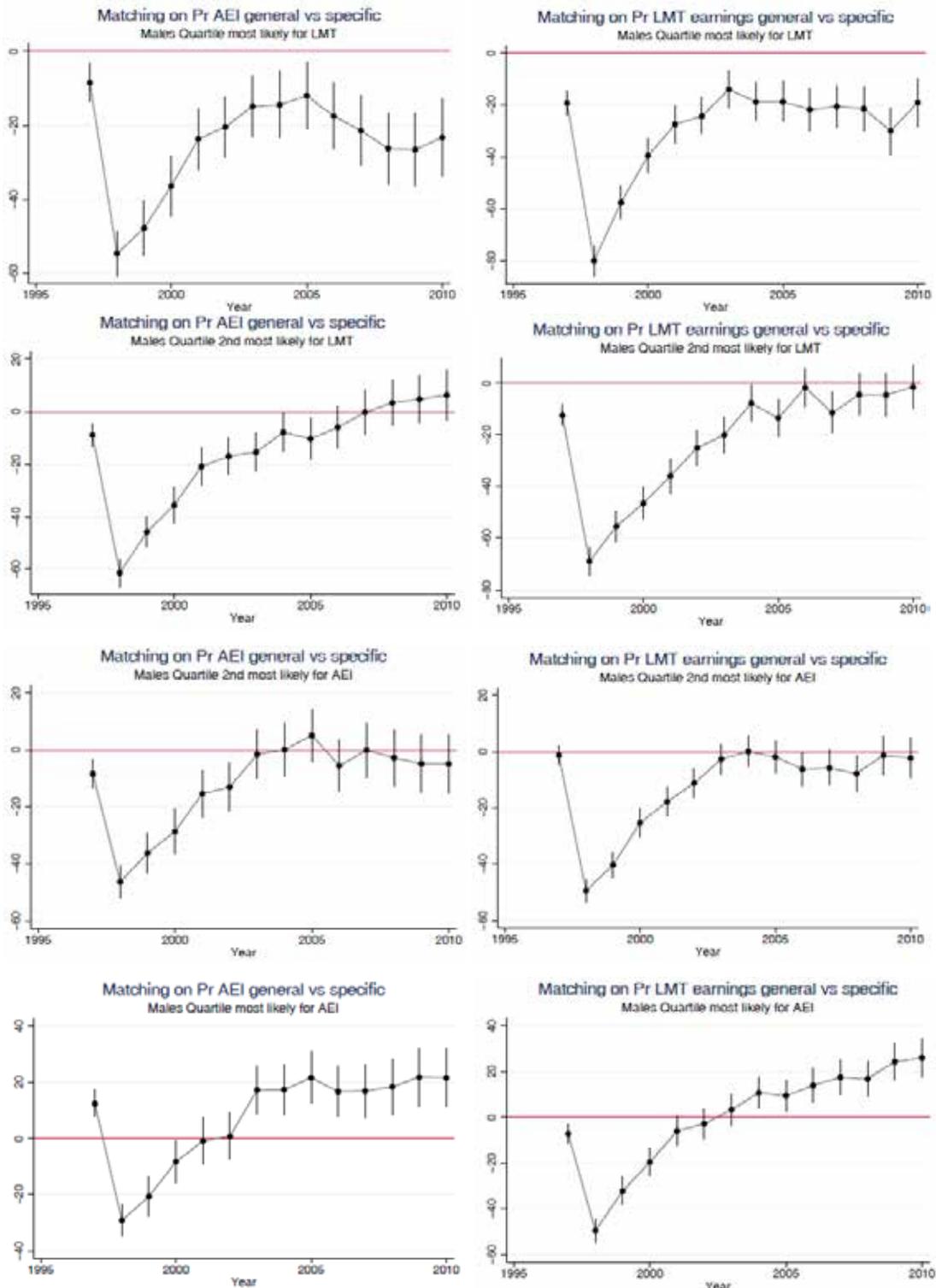
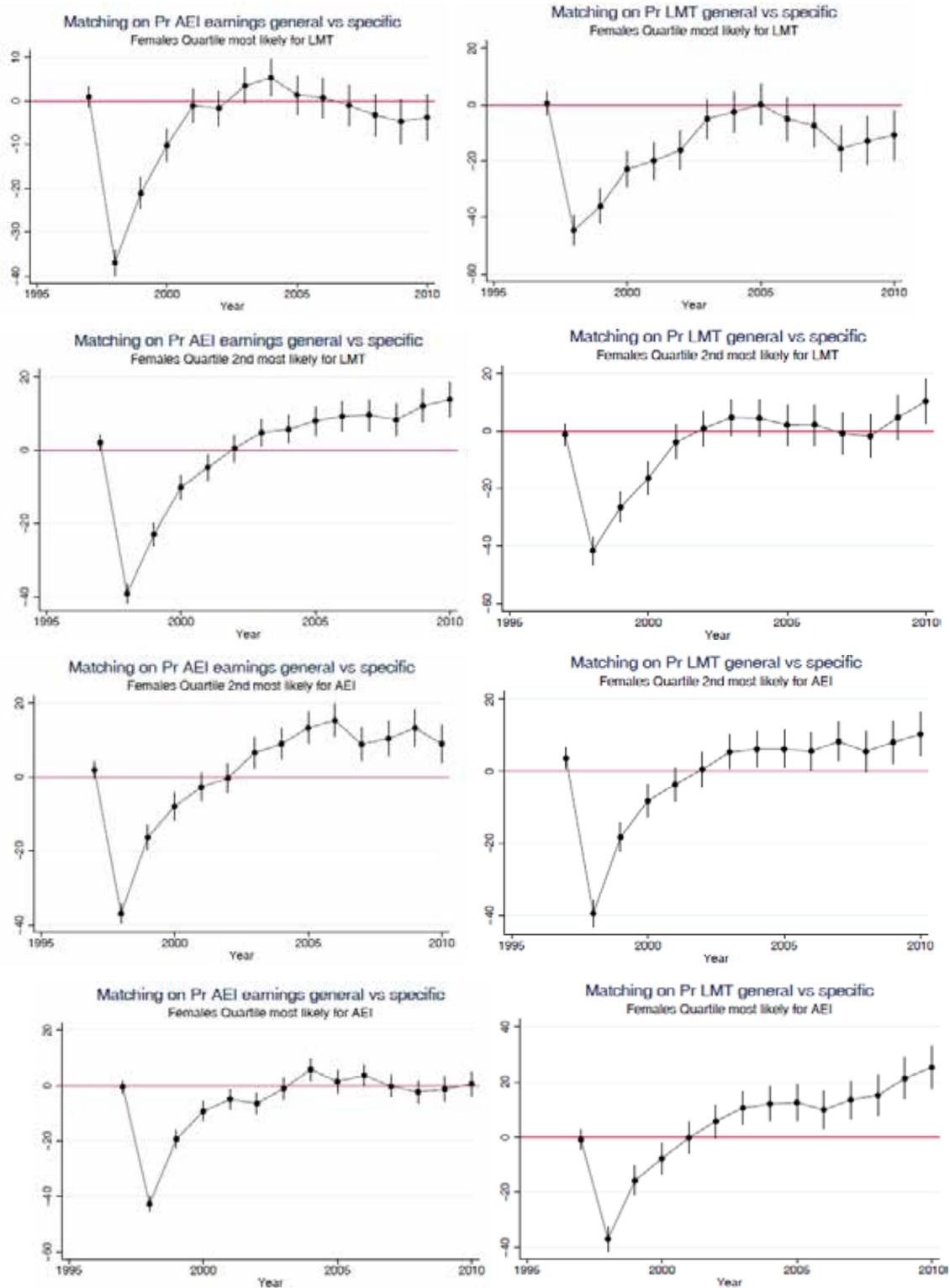


Figure 11: Females – benchmark sample estimates by quartiles of the propensity score distribution



## 7 Heterogeneous effects

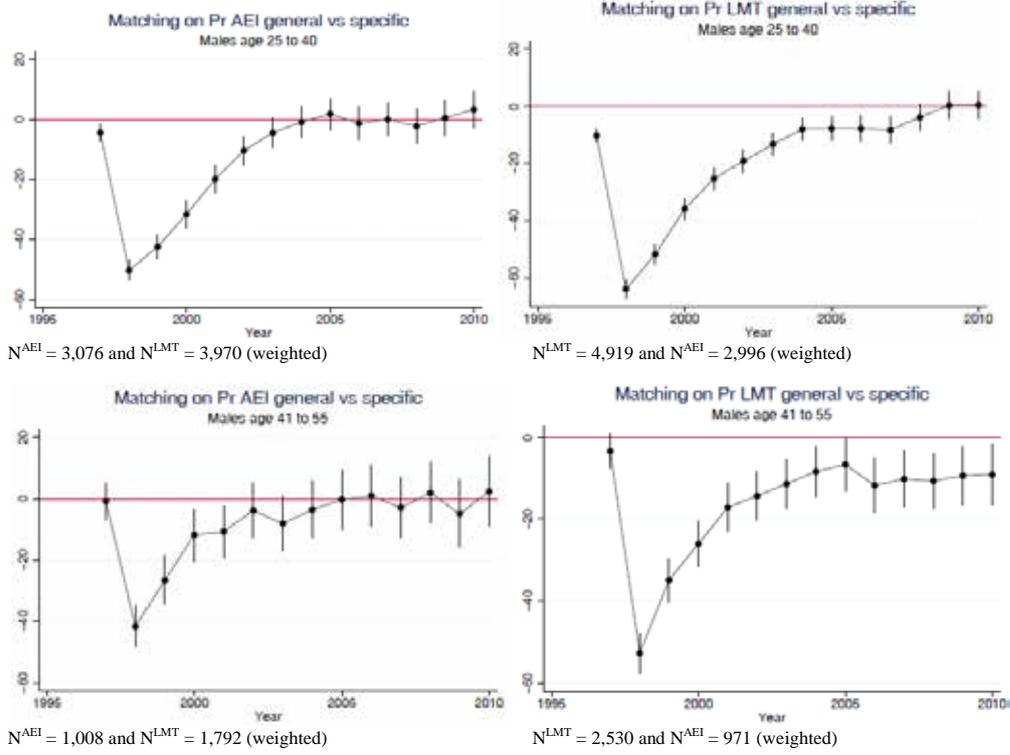
The results discussed in this section concern subgroups stratified by age, above/below median earnings, prior educational attainment and, for males born 1953 or later, whether test scores of cognitive and non-cognitive skills are above or below median.

Figure 12 shows the results by age groups. For males, the estimates of individuals aged 25-40 and 41-55 are similar to the overall estimates. For females aged 41-55, there are positive point estimates in all years 2001 through to 2010, but they are not significantly different from zero when the matching is based on estimates of  $\text{Pr}[\text{LMT}]$ . It may seem surprising that theoretical upper secondary schooling is beneficial for older females, but it is fully in line with results reported in Stenberg et al. (2011).

In Figure 13, the samples are conditioned on whether the earnings in 1995 were below or above the median annual labor earnings (by gender). Males with below median earnings are associated with negative estimates (closer to zero with the limited sample), indicating a long-term relative advantage of the LMT. As for the above median group, the estimates display an upward trend for the AEI when matching on  $\text{Pr}[\text{AEI}]$ . Extrapolating the estimate into future earnings streams implies that the relative losses incurred in 1998-2002 would be recovered by approximately 2025. This holds when applying the limited sample, but is not reproduced with the extended model specification. For females, the results are again essentially similar to the ones presented in Section 6, but the estimates for below median earnings are more modest (closer to zero or below zero) when using the extended model and/or the limited sample.

Figure 12: Difference in differences matching, benchmark sample estimates by age

**Males by age**



**Females by age**

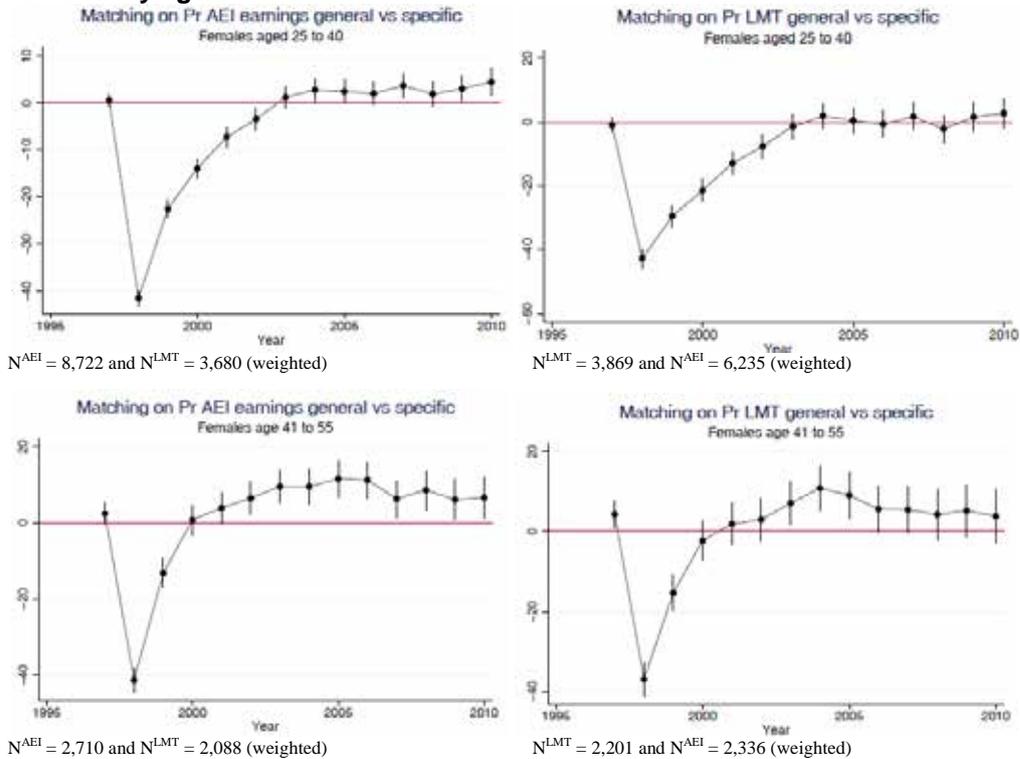
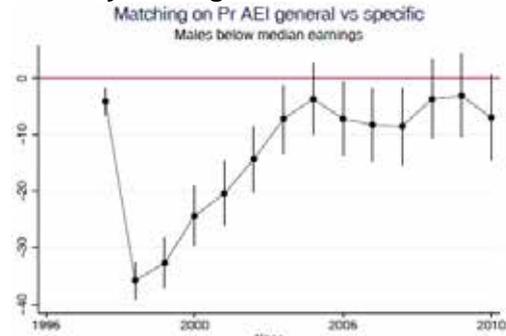
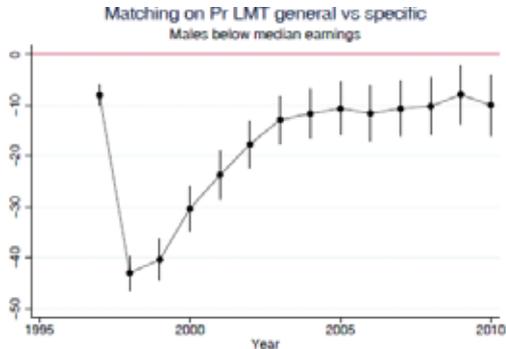


Figure 13: Difference in differences matching, benchmark sample estimates by earnings prior to enrolment

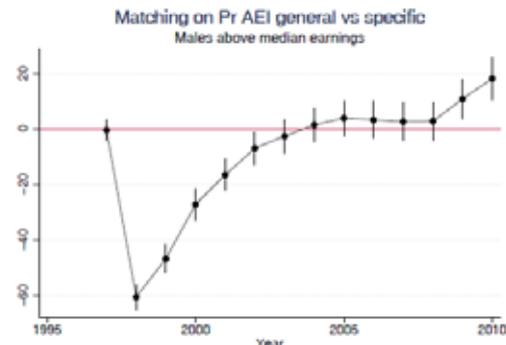
**Males by earnings**



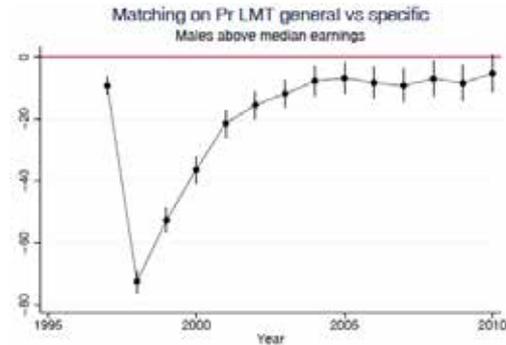
$N^{AEI} = 1,803$  and  $N^{LMT} = 2,549$  (weighted)



$N^{LMT} = 3,061$  and  $N^{AEI} = 1,625$  (weighted)

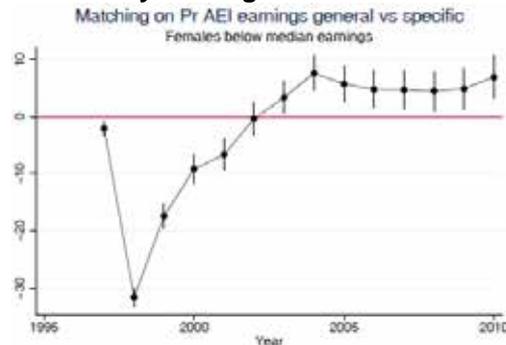


$N^{AEI} = 2,388$  and  $N^{LMT} = 3,338$  (weighted)

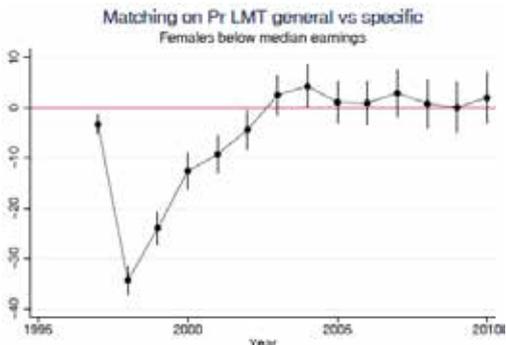


$N^{LMT} = 4,303$  and  $N^{AEI} = 2,182$  (weighted)

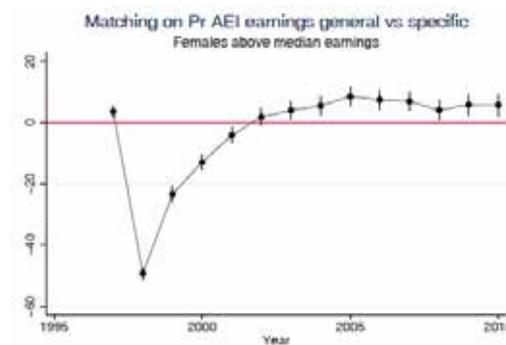
**Females by earnings**



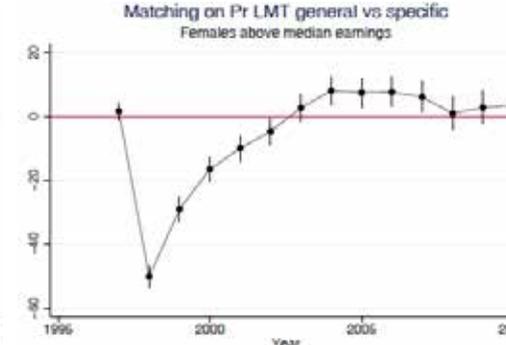
$N^{AEI} = 5,224$  and  $N^{LMT} = 3,132$  (weighted)



$N^{LMT} = 3,354$  and  $N^{AEI} = 4,178$  (weighted)



$N^{AEI} = 6,027$  and  $N^{LMT} = 2,801$  (weighted)



$N^{LMT} = 3,236$  and  $N^{AEI} = 4,481$  (weighted)

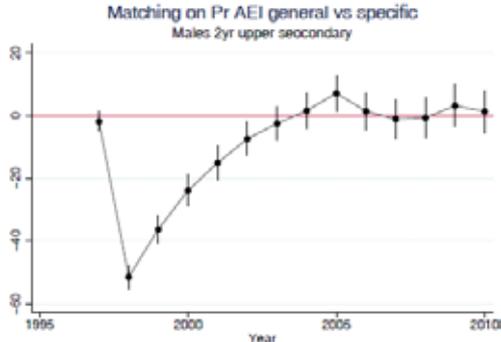
Because the AEI offers education at the lower and upper secondary level, one could argue that groups with short educations are of particular interest. In Figure 14, the estimation results are displayed for groups with 1) a two-year upper secondary school diploma and 2) no upper secondary school completed. For males, the point estimates for both of these subgroups are often above zero when using the Pr[AEI] set up. For males with no upper secondary school, the calculations based on the point estimates imply that the costs are recovered between 2020 and 2025 (i.e. similar to the overall results for females). While the tendency in these results is interesting, the finding is not robust, because most of the estimates are insignificant from zero and not corroborated when the matching is based on Pr[LMT] or using the limited sample.

The results for females with a prior two year upper secondary program indicate positive relative earnings estimates of the AEI from 2003 to 2010. This holds when using the extended model and/or the limited sample, and the estimates also remain borderline statistically significant when the matching is based on Pr[LMT]. While the point estimates in Figure 14 are not large enough to imply a recovery of the initial relative earnings losses, this is the case when applying the extended model specification (recovered between 2015 and 2020). With the limited sample, it also holds when the matching is based on Pr[LMT].

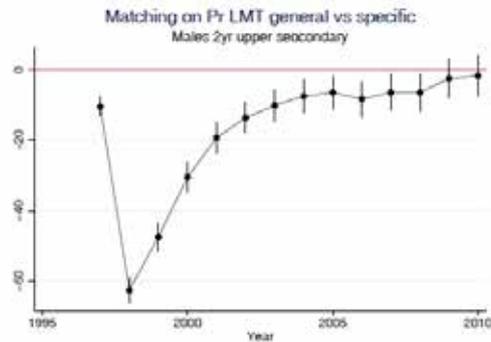
Turning to females with no secondary education, the bottom of Figure 14, the estimates are significant almost throughout from 2003 and onwards, regardless of the “treatment” indicator used. The qualitative results of the extrapolation are very clear because break-even is reached within 10-15 years (2009-2012), i.e. by the end of our observation window or just beyond. This result holds whether the matching is based on Pr[AEI] or Pr[LMT] and regardless of which sample and model specification are used. This implies that for this particular subgroup, expanding the menu of labor market programs to include general training may be associated with substantial efficiency gains. While we acknowledge the difficulty in quantifying this effect, extrapolating the benchmark estimate presented in Figure 14 until all of the individuals are 65 years old yields an excess return to investment representing 10.1 percent.

Figure 14: Difference in differences matching, benchmark sample estimates by prior level of education

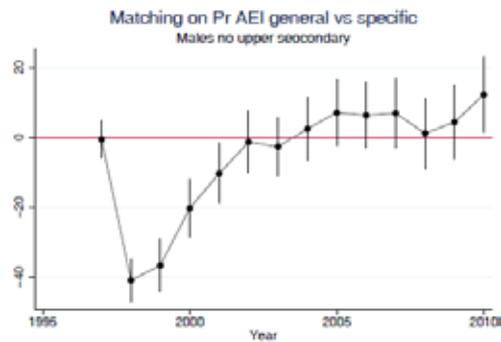
**Males by schooling**



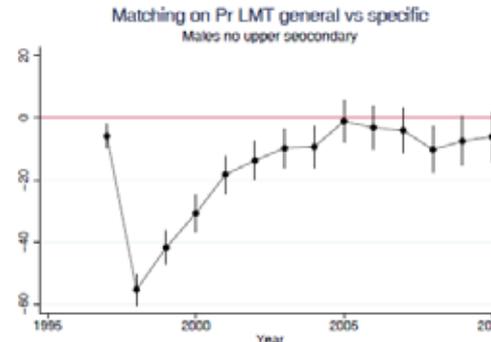
$N^{AEI} = 2,534$  and  $N^{LMT} = 3,375$  (weighted)



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

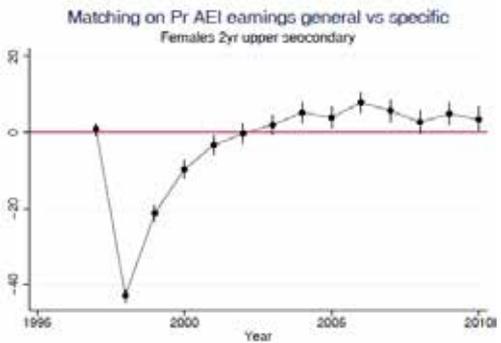


$N^{AEI} = 978$  and  $N^{LMT} = 1,341$  (weighted)

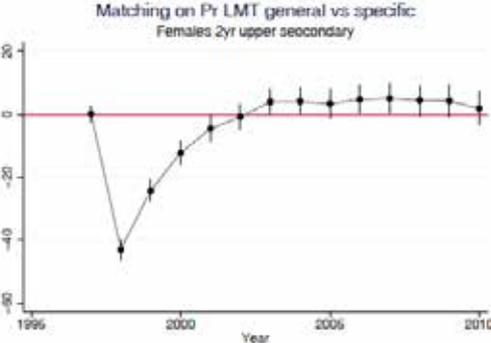


$N^{LMT} = 1,973$  and  $N^{AEI} = 970$  (weighted)

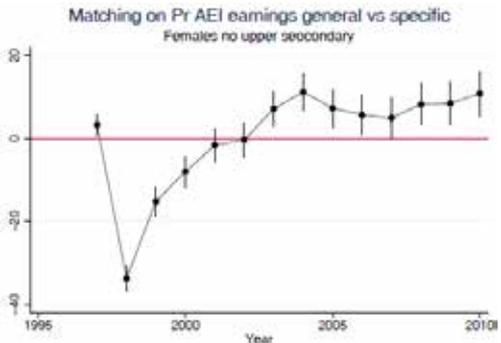
**Females by schooling**



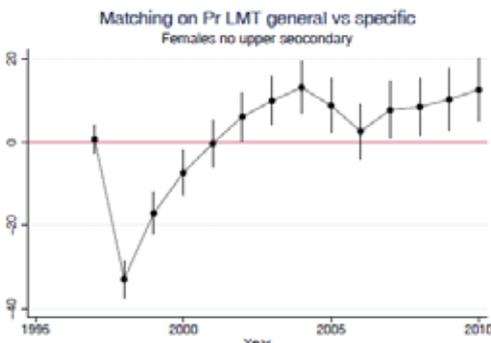
$N^{AEI} = 7,340$  and  $N^{LMT} = 2,831$  (weighted)



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



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



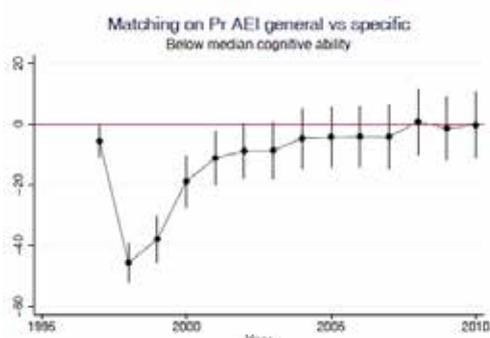
$N^{LMT} = 1,464$  and  $N^{AEI} = 2,051$  (weighted)

While this last result seems relatively compelling, it may be driven by the fact that 1997 was the first year of a reform, where one could imagine an inherent demand caused the individuals with the highest gains from the AEI to be more likely to enroll, potentially making our results difficult to generalize. To check this, we estimated the corresponding relative program effects for participants in 1998, 1999, 2000 and 2001. These imply a recovery by approximately 2015, but the results favor the LMT when employing the 2001 sample or the 1999 sample when matching on  $\text{Pr}[\text{LMT}]$ . The estimates are not directly comparable because unemployment decreased and affected the selection of program participants.

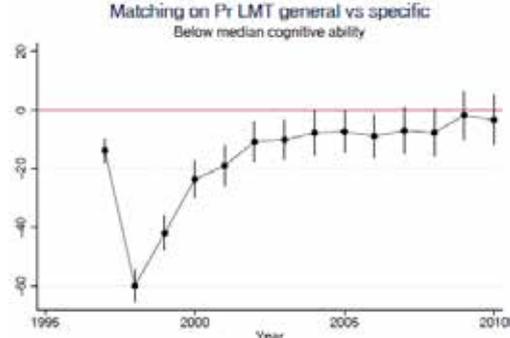
Finally, one may also explore the information contained in the test scores relating to cognitive and non-cognitive skills, which are available for males born 1953 or later. We thus separate this sample based on whether the respective test scores are above or below the median values, in total four groups. The findings are now less precise, as shown in Figure 15, but still display two clear patterns. First, dividing the sample based on cognitive skills, above or below the median, has little impact on our estimates. Perhaps surprisingly, cognitive skills do not seem to be an important factor 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. 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. In Figure 15, only two estimates (2009 and 2010) are positive and statistically significant, but the limited sample estimates are significant 2007-2010 (also with the extended model). A possible interpretation of the result 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 important for benefiting from general training. Any effort to make a quantitative assessment is thwarted here by imprecise estimates, but the qualitative pattern in the results between those above and below median non-cognitive skills is stable across the samples and specifications used.

Figure 15: Difference in differences matching, benchmark sample estimates separately for above and below median of cognitive and non-cognitive skills

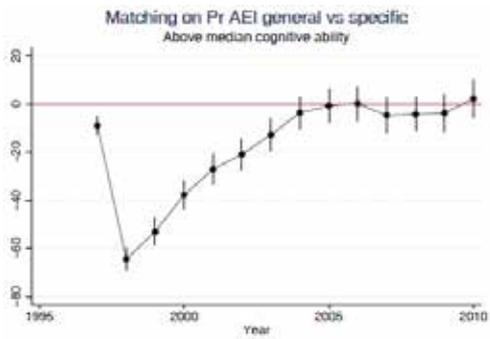
**Males by cognitive skills**



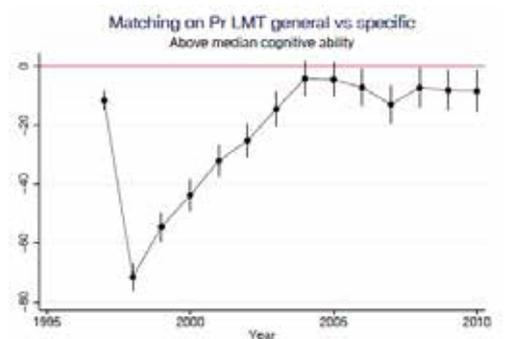
$N^{AEI} = 987$  and  $N^{LMT} = 1,065$  (weighted)



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

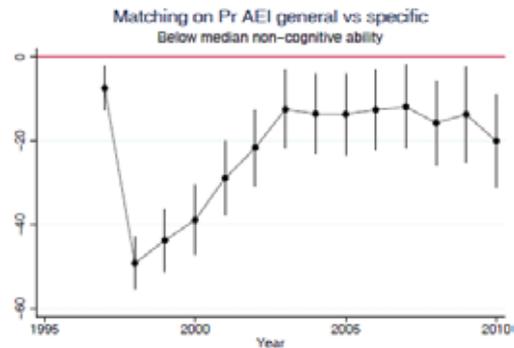


$N^{AEI} = 1,795$  and  $N^{LMT} = 2,156$  (weighted)

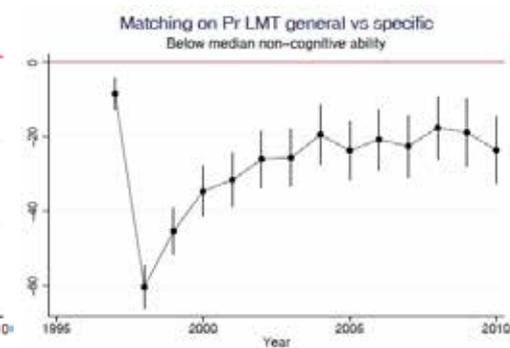


$N^{LMT} = 2,655$  and  $N^{AEI} = 1,653$  (weighted)

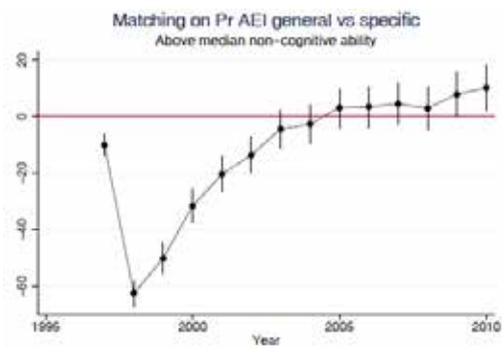
**Males by non-cognitive skills**



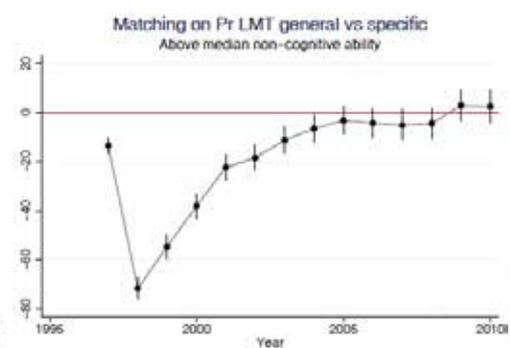
$N^{AEI} = 839$  and  $N^{LMT} = 1,039$  (weighted)



$N^{LMT} = 1,292$  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)

## 8 Summary

In this study, we investigate whether it is economically efficient to expand the menu of active labor market programs to include formal theoretical schooling or general training as an alternative to vocational/specific training. With regard to the data relating to earnings 13 years post-enrollment, the analyses underscore the need for long follow up periods to appropriately assess the programs. Empirically, the analyses are based on selection on observables. A causal interpretation of the results requires that the mechanisms behind enrollment decisions are independent of future earnings, conditional on our control variables. For males aged 25-44 in 1997, we are able to control for ability by using information on cognitive and non-cognitive skills. The inclusion of these variables has little impact on our estimates, lending support to our overall empirical strategy.

We find the estimates of relative program effects are heterogeneous across unemployed individuals and partly consistent with the theory that individuals chose programs to exploit their comparative advantages in ability. Characteristics predicting enrollment in general (specific) training tend to be associated with estimated relative treatment effects that favor general (specific) training. In particular, we find general training for unemployed females with short prior schooling associated with earnings that exceed those following specific training. The result is stable across the sample definitions and matching model specifications. The findings are also consistent with the notion that a wider choice set could increase the probability of a good match between the characteristics of the unemployed and the program content. They are also in line with the theory predicting general training to better enhance labor market prospects in the long run by providing skills which make individuals less sensitive to labor market-related changes. Nevertheless, many 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 (Pissarides 2011, Heckman and Urzua 2008) 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 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 here think 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.

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