Evaluating the relative effects of active labor market programs in Denmark

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Abstract: This paper investigates the relative effects of three different active labor market programs for unemployed in Denmark by using propensity score matching techniques. The performance of these programs is investigated in terms of employment probability and earnings. The relative treatment effects are estimated by comparing the performance amongst unemployed individuals participating in different labor market programs over time. The analysis is based on rich administrative data covering the whole population of Denmark from 2002 through 2006. The central findings suggest that participating in private-sector employment programs is the best option. Especially women, 35-39 years old, vocational education, and unskilled participants benefit from private-sector employment programs.

1. Introduction

Over the past decades labor market policies have gained a central role and policy makers' interest in the effectiveness and efficiency of these policies/programs has escalated. Due to the wide application of active labor market programs (ALMPs) a vast amount of program evaluations have been undertaken both in the US and in Europe. However, in Europe where spending on labor market interventions is very high it is only within the last decade that attention toward program- evaluation has started to increase.

Until recent the most common approach for program evaluations was to analyze the effects of participating in a program versus not participating using the Roy - Rubin framework[1]. However, Imbens (2000) and Lechner (2001) have introduced an extension to this framework by estimating causal treatment effect of pair-wise program comparison. This approach provides information on whether participants in one program would have performed better if they had participated in another program. This lead to the main questions of this paper: "How do the Danish ALMPs work over time, do they work equally well, and who benefit the most?"

To examine these questions this paper investigates the relative effects of different ALMPs for unemployed. In Denmark, it is mandatory to participate in ALMPs, which makes it relevant to focus on the relative effectiveness. Since analysis of the relative effects of the ALMP system is still very sparse this paper reduces the lack of assessment of the relative effectiveness of a large-scale ALMP system. Evaluation of the relative effects will be a new contribution in the evaluation literature of the Danish ALMP system.

Because the main purpose of the ALMP is to improve unemployed individuals employability prospect the outcome measures for this study are employment rate and earnings over time. Propensity score matching with treatment assignment to capture program heterogeneity and selection bias is applied to estimate treatment effects. The methodology is inspired by several evaluation studies conducted by Lechner (a2002, b2002), Sianesi (2004, 2007), and Gerfin and Lechner (2002). The treatment effects are estimated by comparing the performance among unemployed individuals participating in different labor market programs. One of the key advantages of relative effects (program vs. program) is that it allows for direct-

[1] See the standard methodology by Roy (1951) and Rubin (1974).

ly comparing ALMPs, and gives the opportunity to ask the question of what would have happened if participants in one program had participated in another program. This is a particularly relevant question in a mandatory ALMP system as everyone has to participate. Furthermore, given that the programs are ongoing comparing a program to another program eliminates the concern about the starting date for participation. Evaluating the effectiveness of different types of programs will provide information to policy makers about which programs are most valuable and to whom.

In Denmark the ALMP system is based on a very rich menu of measures and is one of the most extensive in the OECD, especially after 1994 when benefit collection became conditional on participation in ALMPs. These policies were implemented without much prior knowledge about potential beneficial effect (Jespersen et al. (2008)). However, the ALMP system is viewed as a very significant part of the Danish labor market and the Danish economy. Consequently the extensive use of ALMPs has gained immense international attention and resulted in a large number of evaluation studies[2]. The overall findings present strong evidence of a motivation or threat effect, which capture the impacts the ALMPs prior to actual participation. For instance, Rosholm and Svarer (2008) find a robust and significant threat effect, which is shown to reduce average unemployment duration[3]. Given the large amount of studies, which conclude that compulsory program participation motivates or threat individuals to find employment prior to participation this paper will not replicate these findings. Instead it will use a different methodology and focus on the upgrading and the locking-in effect[4]. By estimating the relative effects of program participations and focusing on the upgrading and the lock-in effect this paper is a significant contribution in the literature on the Danish ALMP.

The ALMPs in Denmark can be categorized into four main programs; private-sector-, public-sector employment programs, educational training, and other programs[5]. In the existent literature the most successful program amongst the different types is the private-sector employment programs. Rosholm and Svarer (2008) find that private-sector employment programs reduce unemployment duration for unemployment

[2] See Kluve et al. (2007) for a comprehensive review of the Danish evaluations studies.

[3] Also Geerdsen (2006) finds a large significant threat effect by estimating individuals' reaction to compulsory programs using legislative changes in the duration of benefits period for identification.

[4] See Andersen and Svarer (2006), Geerdsen and Holm (2004), and Toomet (2008) for more studies on the threat effect.

[5] Other programs include: individual employment program, entrepreneurship programs, remedial educational programs, and job search assistance. This program is not included in this study because of the broad variety that may not provide any guidance on the effectiveness of these programs.

insurance recipients, where all other programs increase unemployment duration, due to the locking-in effect. Graversen and Van Ours(2008) also find that private-sector employment programs have positive effects. These studies do not produce any conclusion about which programs work best for whom. In this paper, three different program types are evaluated and their relative performances are investigated by comparing participants and evaluate if they would be better off participating in another program.

Because many programs are associated with a certain time period where they "lock-in" participants it is necessary to address medium- and long run effects. Hence, the data set used in this study is constructed from several sources, which include extensive register-data for the whole population of Denmark from 2002 through 2006. Working with data for the whole population in a recent time period will add to the existing results of evaluation studies of Danish labor market programs.

The data for this study include a large number of labor market indicators to measure the attachment to the labor market. For example, duration of unemployment, whether or not it depends on past unemployment experience is discussed in great length among labor economists and policymakers and rigorously explored by Heckman and Smith (1999)[6]. In a Danish evaluation study conducted by Jespersen et al. (2008), which covers the years 1995-2005, a large variation of measures of previous unemployment spells is included. Many of their measures are replicated in this study, for example their measure of unemployment duration which is equal to UI seniority in the Danish labor market. Because their study solely focuses on matching between program participants vs. non participants this study can be viewed as an extension suited to today's labor market regulations.

Another very closely related study is conducted by Sianesi (2008) on the Swedish labor market and covers the time period from 1994-1999. Sianesi emphasizes that given duration dependence or unobserved heterogeneity it is critical to make sure that comparison of individuals in different program must have at least the same duration in unemployment before joining a program. Due to these findings, this study also includes a measurement of duration of unemployment to ensure that comparison of labor market history is taken into account. In addition to Sianesi (2008) this study includes the whole population in a recent time period and results on earnings.

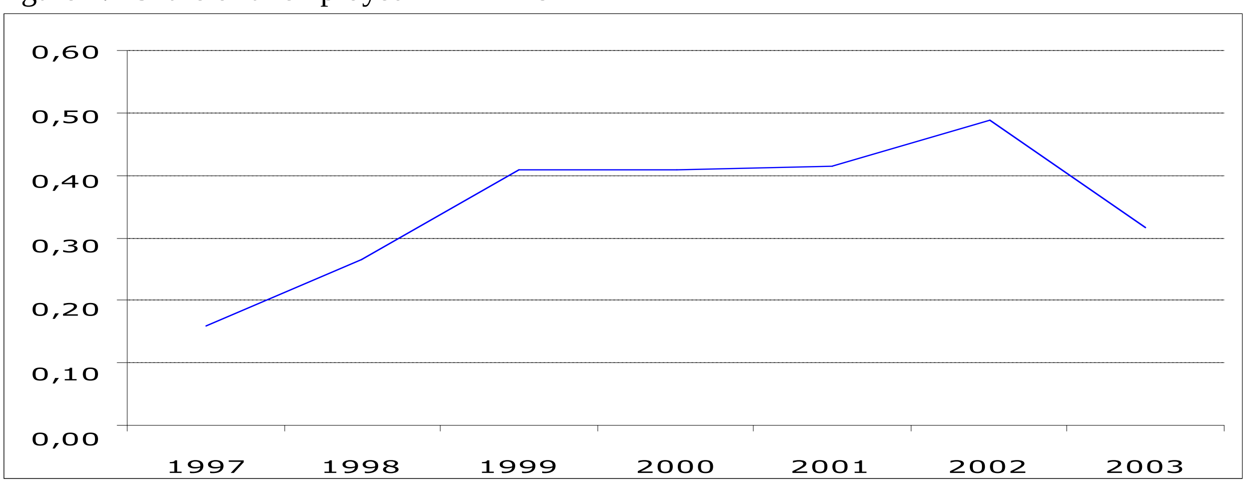
[6] For further standard references about the importance of previous duration of unemployment spells see Heckman et al (1999).

The rest of this paper is organized as follows. Section 2 presents the Danish labor market policy and resent findings. Section 3 describes the data and the selection process. Section 4 clarifies the evaluation strategy. Section 5 presents the empirical results and section 6 concludes.

2. The Danish labor market policy set-up

In the beginning of the 90s Denmark was in a recession and had almost 300.000 unemployed (12.4 percent). This number was reduced to about 1.5 percent in the mid 2008, Andersen and Svarer (2008). Today approximately 2 percent of the labor force participates in some kind of ALMP, Graversen and Jensen (2010). Due to the high unemployment rate in the mid 90s several labor market reforms were introduced. One of the most important changes was that the period of receiving unemployment benefit in "passivity" was reduced and participating in ALMPs became mandatory. In addition, participating in ALMPs no longer automatically made a person eligible for benefit. Since introduction of the labor market reform the share of unemployed in ALMP has increased see Figure 2.1.

Figure 2.1 Share of unemployed in ALMPs[7]



[7] Note: From 2001 to 2002 changes in the register accounts. Source: UNI-Statistics and analysis.

In Denmark there are two parallel administrative systems, the labor market and the social system, which are related to the fact of whether an unemployed is insured or uninsured. Insured and eligible individuals who experience unemployment will receive unemployment insurance benefits (UI). The social system covers individuals that are not insured or who are not eligible for UI benefits can receive social assistant benefits. It is voluntary to be insured and about 80 percent of the labor force in Denmark is insured. The duration for collecting UI benefits has changed over time and today there is a four year time-limit. Individuals who join a UI fund must be a member for a certain time period to receive benefits. The level of benefits depends on the level of previous earnings. For example, a low income worker can receive UI benefits can up to 90% of previous wage depending on the level of personal wealth.

The category of uninsured often includes more traditional welfare recipients such as non-workers, sick, and disabled people, and people with other social problems[8]. In general there is no time limit for collecting welfare. Unemployed in both systems participate in ALMPs. The Public Employment Service organizes ALMPs for recipients of UI benefits, and the local municipality organizes ALMPs for recipients of the welfare benefits, Graversen and Jensen (2010).

Throughout the 90s, as the benefit period and requirement of mandatory participation were gradually tightened and after implementation of the reform ("*More in Work*") in 2003 the labor market policy set-up was in principal the same for both insured - and uninsured unemployed[9]. But the system differs for the two groups in terms of the amount of benefit and the duration period for receiving benefits. The voluntary nature of the Danish UI system implies that individuals may self-select into or out of the UI system, which is why it this study only includes the insured unemployed.

2.1 Rules and regulation of ALMPs

The rules and regulations of ALMPs have been tailored around the approach that an unemployed has a right and a duty to join a labor market program. This basically implies that an unemployed has a right to be offered a qualified labor market program and is obligated to participate. To collect benefits the unemployed person is obligated to distribute and register his/hers CV online, register at the local employment of-

[8] These problems are not always only related to being unemployed, see Bolvig et al. (2003) for detailed description of the social security recipients and the programs their offered.

[9] Since 2003 the two parallel systems have used the same types of programs, and from 2007, the systems are made even more similar and ALMPs are provided regardless of benefit status and what system one belongs to, Kluve et al (2007).

fice (every week), reside in Denmark, actively search for a job, accept a job offer, or participate in ALMPs.

The ALMPs are ongoing and an insured unemployed can join any time. However, different rules apply depending on the age of the unemployed. If the unemployed is over 30 or less than 50 years old then after a 12 months period of unemployment it is mandatory to conduct a work plan for future employment or participation in ALMP[10]. The work plan is produced together with a designated caseworker. The caseworker evaluates the unemployed and has considerable discretion to select an appropriate program to the unemployed. The caseworker is supposed to take into account the background, needs, and desires of the unemployed as well as consider availability of the of the program type, the cost of the program, and the current state of the regional labor market. Certainly caseworker's preferences, incentives, and experiences could affect the placement of the job-seeker[11]. For example, Lechner and Smith (2007) find evidence of systematic allocation from the caseworker to the assignment of courses. In Denmark a profiling system was developed with the labor market reform in 2002. The profiling system assesses the employability of unemployed workers based on five categories, ranging from fully to far from employable[12]. Yet data on caseworker assessment about unemployed levels of job readiness and ability is first recorded in 2005, with would be endogenous for this study.

Table 2.1 Changes in the unemployment insurance and activation period (in years)

Time of change	Unemployment insurance in years ("passivity")	Maximum time in ALMP in years (with UI)
1994	4	3
1996	3	3
1998	2	3
1999	1,75	3
2000	1,5	3
2001	1	3
2003	1*	4

Source: Danish economic council (2002). * The unemployed can participate in ALMP before end of one year.

The unemployed could participate in programs before the end of the 12 months time period, which provide a real possibility of instant participation in ALMPs[13]. The time period for when the unem-

[10] This rule changed in 2009 to 9 months.

[11] Behncke et al. (2007, 2008) analyze the effect of caseworker heterogeneity.
[12] See Rosholm et al. (2004) for further details on the Danish profiling system.

[13] Unemployed below 30 or over 50 years old the time period is 6 months before the work plan and participation in ALPM must be realized.

ployed is obligated to participate in ALMPs has changes since the labor market reform in 1994. The main objective of the reform was to secure the employment status and prevent long periods of unemployment. Table 2.1 shows the reduction in the amount of time spent in “passivity” (where an unemployed is entitled to unemployment insurance) and an increase maximum time spent in ALMPs. These changes have aimed to intensify the goals of the ALMPs, which are to increase employment rate by upgrading skills and intensify job search.

2.2 Program features

The focus for this analysis relies on individuals eligible for unemployment insurance and the programs that they are offered. The ALMPs at the Danish labor market consist of a substantial diversity. Overall the different programs are by law required to be offered to all unemployed giving the same opportunities for everyone despite geographical placement[14]. Still, some programs may not be selected due to a substantial transportation aspect. For instance, a higher education program might not be supplied in a small province. Given that distance to provider may impact the selection of program geographical dummies are included in the study.

In this paper ALMPs has been aggregated into the following groups: private-sector-, public-sector employment programs, and educational training (see Table 3.1 for means characteristics for the programs). Other programs are available but are rather heterogeneous and mainly intent to build competence and self-esteem. Generally these programs appeal to weaker groups of unemployed who are not able to participate in regular private- or public-sector programs. These characteristics may cause doubt on whether this group of programs constitute a valid control group for participants in the other ALMP programs, hence excluded from the analysis. (i) *private-sector employment programs* offer regular work experience that intent to upgrade and strengthen qualifications of the unemployed. In addition, it should improve the competencies and increase the prospects of getting a permanent job. The program could either be in a well known job area for the unemployed or in a new type of work field. The private-sector employer receives a wage subsidy of 50 percent of the minimum wage in the duration of maximum 12 months[15]. The participants are paid a wage rate that follows the negotiated salary of a regularly employed. The municipal decides the number of working hours in collaboration with the employer. Furthermore, the

[14] Transportation cost will be covered depending on distance.
[15] See ”Bekendtgørelse af Lov om en aktiv beskæftigelsesindsats”.

employment must not substitute for a "real" employment and there should be a "fair" number of employees with regular jobs compared to participants in a wage subsidy program. The unemployed may find a private employer themselves. If that is the case the local job center evaluates the content of the work and decides if it satisfies the requirements described above. If the job is accepted a contract will be made between the unemployed, the private company, and the job center. The job-center can buy staff hours to mentor the unemployed for upgrading. While the unemployed is in the program he/she is still registered at the job center. To be eligible to participate in a private-sector employment program the unemployed must have had 6 months of unemployment duration. The length of a private-sector employment program varies a lot but with a maximum duration of 12 months.

(ii) *Public-sector employment programs* follow the same set of rules and eligibility criteria as the private-sector job training, except that a maximum hourly wage rate applies. The work in the public-sector sector is either in a public-sector institution or in a special employment project. The employer decides the number of work hours. Depending on the distance from home to the work the participants can be reimbursed for transportation[16]. The duration of the public-sector job training is between 6-12 months. In general, the duration of public-sector job training tends to be longer than in the private sector, which might be due to the participant in private-sector job training on average have better employment prospects.

(iii) *Educational training* intent to increase the educational level and upgrade qualifications. Educational training program is rather heterogeneous, which includes a considerable number of programs such as vocational training, language courses, computer courses, ordinary education and higher education. The participants must be over 25 years old to participate and will receive a compensation equivalent to the amount of unemployment benefit. Given that the average duration for educational training from a few months to a maximum of two years it is imperative to look at the medium-long run effects.

2.3 Recent evaluation results

Although the evaluation literature is extensive (see Kluve et al. (2007) for a recent overview)[17], the

[16] If distance from home it is over 24 kilometers the participants will be reimbursed.

[17] Other overview studies by Heckman, Lalonde and Smith (1999), Martin and Grubb (2001), and Kluve and Schmidt (2002).

amount of studies that focus on the relative effects of different types of programs are still modest, especially among Danish evaluation studies. Some of the first studies to consider differences in the outcome of different programs are Gerfin and Lechner (2002) and Gerfin et al. (2002). They evaluate several ALMPs effect's on the individual employment probability by using a matching estimator for multiple programs for Switzerland. The both find that wage subsidy for temporary jobs has the most positive effect. Another early study is by Brodaty and Fougère (2002) who evaluate the short- and long-term effects of ALMP programs for unemployed workers in France. They find that an apprenticeship with a fixed-term labor contract seems to be a more effective program, in terms employment prospect, compared to other programs.

A more recent study of the relative effects ALMPs is conducted by Sianesi (2008). She takes into account that ALMPs often consist of a multiple set of programs that are implemented at the same time and herby expand her 2004-study to include a multiple treatment framework to comparing six different programs to each other or by staying longer in open unemployment. By comparing programs to programs it is possible to evaluate how heterogeneous the impacts are and how well programs are targeted. Hence, it is possible to conclude if one program is superior to another program. Her main results shows that both relative to one another and compared to more intense job search, the best performing program is the employment subsidies, followed be trainee replacement and labor market training. Another study on Swedish data that also analysis the relative effect of comparing programs is Carling and Richardson (2004), which suggests that subsidized work experience and training provided by firms have better outcomes than classroom vocational training.

In the literature employment programs are often found to provide the most positive results in terms of getting unemployed back to work. Especially, programs in the private-sector produce positive results. In Denmark, private-sector employment programs seem to be the most promising. Most recent studies that present positive effects of private-sector sector employment programs are found in, Graversen and Van Ours (2008), Munch and Skipper (2004), and Jespersen et al (2009)[18]. However, Graversen and Jensen (2010) evaluates the effects for welfare benefits recipients and find no significant mean treatment effect of private sector employment program participation compared to participating in other programs.

[18] Rosholm and Svarer (2004) do not find positive significant treatment effects.

In other countries for example Germany, Gerfin and Lechner (2002) find a positive effect of wage subsidy programs. Blundell et al. (2004) who evaluate of the “New Deal for Young People” in the UK find treatment effects that amount to a rise in employment of 5 percent point in the short run. However, they do not conduct a long term analysis. Also Martin and Grubb (2001) find that the effects of private sector employment are most successful at getting the unemployed back to work. Reasons for this positive result for private sector employment programs, could relate to potential job opportunities, unobserved heterogeneity like motivation, personal apperance, caseworkers discretionary selection mechanism, etc.

Several caveats might be considered when estimating effect of employment programs with a wage subsidy, because an upward biased may appear if the subsidized worker substitutes other workers, which implies that a worker is hired by a firm in a subsidies job instead of an un-subsidized worker who would have been hired otherwise. This could compromise the Stable Unit Treatment Value Assumption (SUTVA), which requires no spreading of impact between treated and controls. Estimating the effectiveness of wage subsidy programs in the private sector may generate a dead-weight loss due to the fact that the same person would have been hired by the same firm without the help of a subsidy. The macroeconomic literature has widely documented this type of negatively potential, Akerlof, G. A (1978). To measure the deadweight would require information that is not available in this study. Given the complexity of measuring this effect this paper mainly focuses on the upgrading and the locking-in effect.

The locking-in effect is a well known characteristic for an ALMP and both Danish and international evaluation studies have shown high degrees of locking-in effect, especially for training programs in the short run. For example, Rosholm and Svarer (2008), find large locking-in effects for public-sector pro- grams and education training. Fitzenberger and Speckesser (2007) find a locking-in effect in the short-run for job creation scheme in West Germany. Also Lechner et al. (2004) shows high degrees of locking-in effects in the short run evaluating training programs in Germany.

Given the locking-in effect it is necessary to include medium or long term perspectives like Jespersen et al. (2008) who evaluate the costs and benefits of labor market programs by estimating long term treatment effects using a propensity score matching technique. They include a time period 1995-2005 and find a positive effect of private- and public-sector job training on employment rates and earnings. They present a net surplus after taking into account the costs of the programs and by including a longer time horizon. The length of the evaluation period is essential to identifying different effects for different program types. For example, the impact effects of educational training will be underestimated in the short run, while private-sector job employment, will be realized quickly in the short run.

In the literature the long-term effects are increasingly getting more and more attention. Because of the long lasting locking-in effect it is imperative to derive long-term treatment effects. Also Sianesi (2008) examine long-term effects and find positive post-program effects but typically only after 1-3 years.

Based on these findings there is still a demand for further research on the relative effects of ALMP in the Danish labor market. This paper contributes mainly by two aspects; first the knowledge on the relative effectiveness of program participation in different programs, which is still relatively low in Denmark. Second, the selected data include the whole population of Denmark in a recent time period, including new explanatory variables.

3. Data and sample selection

This section describes the data sources and the selection criteria used in this study. The data includes extensive register-data for the whole population of Denmark from 2002 through 2006. This comprehensive administrative data provides an opportunity to rigorously estimate the impact effects of ALMPs.

3.1 Data

The data set used in this study includes the following; socio-economic individual characteristics, income data, and longitudinal labor market history, which provide information on individual's labor market status over time. The socio-economic and income data are observed annually and extracted from registers at Statistics Denmark[19]. The labor market history is weekly observations of whether an individual is employed, unemployed, joining an ALMP, out of the labor market, etc. The data set consists of combined data from different administrative registers[20]. The combination of these registers enables the crea-

[19] The socio-economic data comes from the integrated database for labor market research, IDA.

[20] CRAM (unemployment), AMFORA (program participation), CON (employment) and SHS (social income transfers i.e., sickness benefits, maternity leave etc.).

tion of variables that measures the duration of the different labor market spells. This data provide information about when an employment spell has ended. The labor market history includes mean duration of previous employment and unemployment spells. Also indicators of former participation in ALMP and previous time spent in receiving sickness benefits are recorded.

A stock sample is generated by including participants in the first week of 2002 then followed through 2006. In that time frame the effects on employment and earnings are analyzed. The employment outcome is constructed as quarterly employment rate through 20 quarters from 2002-2006 based on employment spells derived on the weekly employment status from the administrative registers. Hence the employment outcome includes the employment probability conditional on the choice of program participation. The employment rate is between 0 (full time unemployment) and 1 (full time employment) and is measured in percentage point[21].

The outcome variables on earnings are constructed as annual labor income from 2002 through 2005. This annual labor income is measured in the registers, as employers are bound by law to inform the authorities about earnings of their employees. It consists of taxable wage income because the reported earnings are the basis for income taxation. This also include wage earned during participation in any active labor market program. Both outcome variables are measured directly in public registers and are regarded as highly reliable.

3.2 Sample selection process

In this study an individual that participate in one of the three program categories within the first week of 2002 that person is selected into the treatment- or comparison group. The time period is chosen to analyze a recent time period and at the same time be able to measure medium-long term effects compared to other studies[22]. The sample is limited to adults between 30 and 50 years who are entitled to unemployment benefits. The reason for excluding individuals below 30 over 50 years is because different rules and eligible criteria's apply to this category. Furthermore, individuals who die or emigrate during the sample period are also excluded. In general, individuals need to be as homogeneous as possible on

[21] A part-time work would be registered on a weekly basis and depending on how much time spend in employment it would allocate to the employment rate.

[22] Jespersen et al. (2008) analyze the first week of 1995.

basic characteristics when comparing effects of program participation. Hence, to avoid problems of missing information about educational attainment and work experience of immigrants, this group is excluded from the analysis. This data selection provides a sample of 10,612 individuals who satisfy the rules of registration, age, and entitlement. There are 935 participants in private-sector employment program, 1617 in public-sector employment programs, and 8060 in educational training. Descriptive characteristics for a selected sample for the various groups of treatments are shown in Table 3.1.

The sample includes demographic, human capital, and labor market variables all variables that affect both participation and outcomes. The socio demographic variables consist of age group dummies, gender, marital status, dummies for number of children, and geographical living status. Attained education is included as dummies of different categories such as short education (2 years of education upper secondary school), medium education (3-4 years of education), and vocational education, unskilled (e.g. lower secondary or less - not educated), and with higher (5 years or more) as the reference category. Grades and memberships to a union are also included see Appendix Table A.1 for a full description of the data.

Table 3.1 shows that female participants dominate in public-sector job training. ALMP participation decreases by age of the participants no matter what type of program is being examined. Human capital variables are captured by dummies of education-level and work experience in terms of years of work. Participants across all the programs have a high share of human capital level is found for participants in private-sector employment programs.

Detailed information on labor market history (such as work experience, first time unemployment, proportion of time spent in employment, unemployment duration, sick leave, ALMP, UI seniority) is included. Work experience is included in years since 1964. First time unemployment spell indicates whether the present unemployment spell is the “first”. Private-sector employment participants have the highest share of participants in that category. The length of unemployment duration prior to the program participation is included, which is highest for participants in the employment programs. The different rates of unemployment, sickness, employment are fractions of time spent in either unemployment, sickness, employment in 2001.

Table 3.1 Selected sample means and stdv.

Variables [a]	Private-sector employment program Mean	Private-sector employment program Std. Dev.	Public-employment program Mean	Public-employment program Std. Dev.	Educational training Mean	Educational training Std. Dev.
Age 30 - 34	0,24	0,43	0,21	0,40	0,24	0,43
Age 40 - 44	0,17	0,38	0,20	0,41	0,18	0,39
Age 45 - 50	0,16	0,36	0,21	0,41	0,17	0,38
Women	0,44	0,50	0,64	0,48	0,59	0,49
Married	0,39	0,49	0,42	0,49	0,44	0,50
Short education (2 years)	0,06	0,24	0,04	0,20	0,06	0,24
Medium education (3-4 years)	0,05	0,22	0,07	0,25	0,09	0,28
Higher education (5 years)	0,04	0,20	0,06	0,23	0,06	0,24
Unskilled (non-qualified)	0,43	0,50	0,47	0,50	0,44	0,50
Vocational education	0,41	0,49	0,36	0,48	0,35	0,48
Work experience in years	9,24	6,66	9,10	6,33	8,65	6,54
Duration of unemployment in 2001	41,46	46,04	39,47	39,99	32,85	29,83
First U spell	0,10	0,30	0,07	0,25	0,09	0,29
Unemployment rate	0,22	0,18	0,26	0,18	0,22	0,17
Sickness rate	0,02	0,05	0,02	0,05	0,02	0,04
Employment rate	0,54	0,27	0,47	0,25	0,53	0,25
Mean duration E 2000-2001	92,51	119,81	73,40	98,24	88,37	114,92
Mean duration U 2000-2001	22,92	18,34	25,82	19,03	23,03	18,31
UI Seniority in weeks	65,30	90,94	89,53	101,61	61,51	82,39
Proportion with no UI Seniority	0,38	0,49	0,26	0,44	0,34	0,47
# U spells last year	0,58	0,81	0,66	0,80	0,57	0,80
# E spells last year	0,77	0,90	0,70	0,87	0,78	0,89
Private-sector job training 2001	0,04	0,20	0,01	0,10	0,01	0,09
Public-sector job training 2001	0,01	0,11	0,06	0,24	0,01	0,11
Educational training 2001	0,18	0,38	0,29	0,45	0,29	0,45
Local labor market:						
Copenhagen	0,55	1,29	0,73	1,44	0,69	1,42
Roskilde	0,06	0,41	0,10	0,52	0,09	0,49
Vestsjælland	0,12	0,55	0,12	0,55	0,16	0,63
Storstrøms	0,18	0,75	0,23	0,84	0,13	0,63
Bornholm	0,03	0,25	0,04	0,33	0,04	0,32
Fyn	0,25	0,77	0,27	0,79	0,32	0,86
Sønderjylland	0,16	0,60	0,16	0,60	0,11	0,52
Ribe	0,09	0,45	0,10	0,47	0,10	0,49
Vejle	0,29	0,99	0,15	0,72	0,16	0,76
Ringkøbing	0,16	0,70	0,10	0,56	0,11	0,59
Aarhus	0,33	0,87	0,37	0,92	0,40	0,95
Viborg	0,13	0,55	0,09	0,46	0,10	0,48
Nordjylland	0,43	0,94	0,34	0,86	0,35	0,87
Number of individuals	935		1617		8060	

[a] Two variables measure the mean duration of employment (E) and unemployment (U) and spells respectively since 2001- 02. The UI seniority denotes the number of weeks the unemployed previously were unemployed and received UI benefits at the beginning of the present unemployment spell. 'First U spell' indicates whether the present unemployment spell is the first and 'No. of U spells'

denotes the number of previous unemployment spells. The three proportion 'rates' are variables that measure the proportion of time spent in unemployment, employment and receiving sickness benefits respectively in 2001. Four dummy variables for participation in programmes in 2001. The local labor market indicators are defined as number of participants in a program within a county divided by all individual in a program.

Mean duration of previous employment and unemployment spells are also included. UI seniority is measured by the number of weeks the unemployed previously were unemployed and received benefits. The indicator of UI seniority determines when the unemployed is obliged to participate in ALMPs. The UI seniority is highest amongst participants in public-sector employment programs. The share of participation in programs in the previous year 2001 is also included.

Finally, local labor market conditions have shown to be essential to the choice of participation in a program see Heckman et al. (1997). Inclusion of these types of indicators should catch some of the unobserved local aspects surrounding the local labor market. In this study the local labor market indicators are constructed by the number of participants in the three programs as a proportion of all individuals registered at the individual's municipality. Inclusion of this local labor market indicator has not been used in any study of Danish ALMPs. Additional, geographical variables that distinguish between Copenhagen, the five largest cities, and other parts of the countries, have also been included to control for the "size" of the local labor market. Both Sianesi (2008), and Jespersen et al.(2008) include these local labor market variables. Program duration were include as a control variable in Sianesi (2008), but since the outcome variable is employment rate measured over the time period then program duration would be endogenous. Sianesi (2008) includes the median duration of programs, which is not included in this study.

4. The evaluation strategy

The main objective is to measure how different types of ALMPs affect the unemployment and income rate over time. Hence, the evaluation strategy is based on comparisons with (multiple) control groups. The approach and the implication of the evaluation strategy will be discussed in the following sections.

4.1 The evaluation problem

By studying how unemployed individuals perform by joining a program compared to joining another program, in terms of employment and income rate, it is possible to get some insights on which programs work the best and for whom. In Denmark ALMPs are ongoing and therefore take place continuously over time. The programs are available for every registered job-seeker who wants to participate and who have an obligation to participate, given that the job-seeker stays unemployed[23].

Ideally the effects should be measured by observing the same person being treated and untreated in the same time period with everything being the same. Even though this scenario is not possible to observe several estimation techniques deal with this problem such as fixed effects, duration analysis, and matching. But given that the ALMP system becomes mandatory after a short period of unemployment[24]it is appropriate to focus on the relative effectiveness among programs. This makes the choice of comparison groups less complicated in terms of the institutional setting with ongoing programs.

In Jespersen et al. (2008) the treatment effect is identified as the effect of participating in ALMP com- pared to waiting longer in unemployment. They define a comparison group as not participating at all, which corroborate to the time period of their analysis. Since, at that time period duration for receiving UI without any required participation in ALMP was 4 years. This implies that "no participation" could not be viewed as person who is employed. Sianesi (2004) also evaluate program participants versus non participants by examining the average effect for those who join a program, compared to those who postpone the participation decision by not joining any program at least up till then. Because the comparison group includes individuals who may participate in a future program, the effects evaluate participation against possible delayed participation.

The concern of defining a credible comparison group in ongoing programs is often very complex and may require a dynamic selection process[25]. In general, it is recommended to apply different types of control groups and test the robustness of the results.

4.2 The evaluation approach and implications

This study uses propensity score matching, which has become a very popular method for estimating average treatment effects. The differentials average treatment effects are estimated for the treated, who participate in treatment $k = 1,2,3$ (program k versus program $k´$). The outcome variables; employment- and

[23] If the comparison group was defined by future outcomes the choice of a valid comparison group is not trivial, this has been formalized by Frederiksson and Johansson (2003).

[24] See table 2.1.

[25] See Heckman and Navarro (2005) and Adda et al. (2007) for details on dynamic selection.

earnings rates are evaluated for unemployed who in the first week of 2002 participate in a program over the time period of 20 quarters[26]. The types of treatments include private -, public-sector employment programs, and educational training[27].

This empirical analysis of a non-experimental estimator follows the traditional formulation for an impact estimator and it is assumed that the outcome variables for each person are not influenced by the actual participation of other persons, which is the SUTVA. The evaluation approach follows the framework developed by Imbens (2000) and Lechner (2001), therefore the average treatment effect for participants receiving different treatment can be derived as the following:

$$\theta_0^{k,k'} = E(Y^k - Y^{k'} | D = k) = E(Y^k | D = k) - E(Y^{k'} | D = k) \quad for\ k, k' \in \{1,..4\},\ k \neq k' \qquad (1)$$

where $\theta_0^{k,k'}$ is the expected effect for an individual randomly drawn from the population of participants in treatments k and k'. $E(Y^k|D=k)$ is the term that is observed in the data and $E(Y^{k'}|D= k)$ is the term that the counterfactuals would have experienced <u>if</u> they had participated. $D \in \{1,3\}$ denotes the different assignment indicators and Y^k denotes the potential outcome for different types of treatments k.

In general, evaluations do not construct the person-specific impact but evaluate constructed means. As a result the treatment effect in (1) gives the average effect on Y^k for participants in program k compared to receiving a different treatment k'. Hence, the first part of (1) is based on observables it is a problem to find the counterfactual of the type $E(Y^{k'}|D= k)$, which is the outcome that participants in k would have experienced, on average, had they chosen any treatment other than k. Because this part is unobserved, further assumptions are needed for identification of an adequate comparison group (see Rubin (1974)). Hence, without imposing any functional form the conditional independence assumption is applied (CIA). The CIA basically proposes that the outcome from the treatment is independent from the outcome from the counterfactuals.

In this context this implies that conditional on observables, X, the counterfactual average Y^k for individual participating in program k is the same as the observed average Y^k for individuals participating in k´, which gives the following:

[26] All estimates are performed using STATA-module *psmatch 2* (Leuven and Sianesi, 2003).

[27] The selected sample is sorted after individual's first program participation in that period.

$$E(Y^{k'} | D = k, X = x) = E(Y^{k'} | D = k', X = x) \text{ for } k, k' \in \{0,1,..K\}, k > k' \quad (2)$$

The construction of a comparison group among all the *k'*-participants, which is as similar as possible to the groups of participants in *k* in terms of observed characteristics is exactly what matching methods is based on. Matching methods build on the assumption that all differences between *k* participants and -*k'*-participants are captured by observed characteristics, which is the CIA. The covariates that are held fixed are assumed to be known and observed, which is the core justification for a casual interpretation of a regression, Angrist and Pischke (2009)[28]. So even though the CIA can not be guaranteed this study relies on a very comprehensive data set, which provides sufficient information to assume that the CIA is plausible and most heterogeneity is observed. Hence, in this study any difference will be attributed to the treatment (the program). A sample from group *k´* produces a match group in which the distribution of pre-treatment observables *X* is as similar as possible to the distribution in group *k.*

In cases with non-experimental data the matching techniques is applied to identify a comparison group, but only for those individuals in the treatment group who have a positive probability of being in the comparison group, this is also called the common support condition. To utilize the CIA (2) all participants in *k* must have a counterpart in group *k´* and hereby ensuring that the common support condition restriction is fulfilled. This imply that comparisons between *k* participants and *k'*- participants for all individuals with probability larger than the smallest maximum and smaller than the largest minimum are deleted. If no restriction is placed on the functional form matching methods can eliminate any bias due to the observed differences between the participants and non-participants. However, matching is based on the restriction in (2), which induces a potential bias based on the selection on unobservable identified by Heckman et al. (1998). So even though the comparison is based on relevant observed characteristics there might still be unobserved factors that invalidate the comparison such as motivation, intelligence, unknown background parameters, etc.

Summing up, individuals participating in different programs are compared based on personal characteristics, demographic measures, caseworker assessment, past labor market history, including previous unemployment duration and local labor market indicators. The matching technique is chosen as it seems ap-

[28] Rubin (1974), Imbens (1999), and Lechner (1999; 2001) formally show that the CIA identify the parameters of interest in the case of multiple treatment.

propriate given the research objective and the available data.

4.3 Applying the matching estimator

To apply the matching estimator the CIA restriction in (2) must hold. The result from Rosenbaum and Rubin (1983) shows that if (2) holds for the binary case $D \in \{0,1\}$ conditional on X then it will hold for the balancing score $b(X)$. They prove that when a large set of covariates is in use, matching can still be used where the balancing score $b(X)$, is defined as a function of X, given that the characteristics X are balanced across the treatment groups. Hence the focus shifts from a set of covariates to the probability of program participation, where the propensity score can be derived as $Pr(D=1|X)$. This imply that instead of matching on a large set of variables it is sufficient to match on the propensity score to obtain the same probability distribution for treated and comparison individuals. This is a way to adjust for the differences in the full set of characteristics X and in scalar terms derived as $D \perp X| Pr(D=1|X)$. Implying that in terms of the pair-wise comparison, the pair-wise average treatment effects of treatment k and $k´$ for the participants in k, the $b(X)$ are needed for each k and $k´$. The balancing score ensure the balancing of X in the two sub-populations, which implies that $D \perp X \mid b(X)$, for each separate comparison $D \in \{k,k´\}$.

The average treatment effect of the treated can then be formulated as[29]:

$$\theta_0^{k,k´} = E[Pr(D=k|X, D \in \{k,k´\})|b(X)] = Pr(D=k|X, D \in \{k,k´\}) \equiv P^{k|kk´}(X) \text{ and } 0<P^{k|kk´}(X)<1 \qquad (3)$$

where the conditional probability of treatment (the balancing score) k given either treatment k or $k´$:

$$P^{k|kk´}(X)= \frac{Pr(D=k|X)}{Pr(D=k|X)+Pr(D=k´|X)}$$

As long as 2 and 3 holds the counterfactual average treatment effect can be estimated as:

$$\theta_0^{k´,k} = E(Y^{k´}|D= k) = E_{pk|kk´}[E(Y^{k´}|D= k´, P^{k|kk´}(X))|D=k] \qquad (4)$$

This term (4) together with the balancing score $P^{k|kk´}(X)<1$ are the only conditions needed to justify propensity score matching to estimate the average effects of the treated, Heckman et al (1998).

[29] Formulated by Rosenbaum and Rubin (1983) and Lechner (2001).

To avoid systematic difference in observed characteristics participants in *k* are match to individuals in *k´* given assumption (2). The average outcome for the matched pool of *k´* participants indentifies the counterfactual outcome that participants in *k* would have experienced, had they taken treatment *k´* instead. Hence, comparing program *k* to *k´* for participants in program *k*, the balancing score is calculated for each *k* participants. When two programs are compared the existence of multiple treatments can be ignored, since individuals who do not take part in neither of the compared programs are not needed for any identification. This implies that no information from other than those participating is needed for estimating the average treatment effects, $\theta_0^{k,k'}$ and $\theta_0^{k',k}$ (see Lechner, 2002).

Based on the matching requirements several different matching methods have been estimated to achieve the result of (3)[30]. In this study the specification for *b(X)* includes estimation of several binomial logits, since 4 programs are compared. A kernel specification with replacement is used in the estimated propensity score. The estimated results in the empirical section refer to individuals within common support in the treatment group. Estimating treatment effects across programs using matching can produce problems in terms of a small sample size and difficulties with common support. This is why matching methods with replacement is applied still knowing that using a comparison person more than once in the matching procedure can inflate uncertainty. In general it often depends on the sample size, but in most cases matching with replacement is applied.

5. Empirical results

5.1 Selection

Before evaluating the relative effects of ALMP it is essential to look at the selection mechanism of these programs. Because even though a rich set of observables is applied, it is still necessary to examine what is driving the selection into the program and how it relates to the various outcomes. Table A.2 (Appendix) contains the results of running 6 binomial logit models for participation in each of the dif-

[30] Several matching algorithm such as nearest neighbor, radius matching, with and without caliper, Kernel with replacement, and Mahalanobis did not provide any important variation in the overall results of the depended variable, see Appendix Figure A.7 for detailed results.

ferent pair-wise estimation. It seems like the most important determinants for selection are gender, experience, UI seniority, previous participation in programs, and more general labor market history.

Some of the general household characteristics that matters for the selection probability are gender and being married. Being married impact in some cases the participation probability negatively. Skills of the participants impact positively selection into public-sector employment programs and negatively in private- sector employment programs. Given that skills do not impact a large variety of the selections may be due to the motivation for participation and highly motivated people may get higher education.

General labor market history such as unemployment duration and previous program participation seems to impact the participation probability. For example previous participation in private-sector employment program increased the participation probability for being selected into public-sector employment program. UI-seniority also increases the probability to participate in many of the models. Labor market history is well known to be key variables in matching technique, given that these historical values implicit include fixed effects and unobserved heterogeneity, which makes the CIA plausible.

The supply of different types of program should in principal be equal to all unemployed across the country. But as the administration of ALMPs are managed and provided by the local county the coverage may deviate, especially in areas where distance to program provides matters. Several of the local labor market indicators included to account for this seem influence the selection for participation for some of the programs. Over all this shows that different variables are important for the selection into different programs hence conditioning on these variables is therefore important for this analysis.

Results in the literature on Danish ALMPs have proved that some programs yield considerable employment and earning effects when compared to non participants. Which programs are the most effective for a given subpopulation is the question that will be addressed in the following. Section 5.2 presents the pair-wise matching results including the matching results of different sub-groups such as gender, age, and education. Section 5.3 presents the sensitivity analysis.

5.2 Pair-wise comparison

In this paper the relative effect of participating in ALMP is defined as whether a person would have been better off had that person participated in another program in terms of employment or earning rates. By evaluating the relative effect of ALMPs it is possible to evaluate whether a person participating in for example classroom training would have improved their employment rate by attending private-sector employment program, or that participants in public-sector employment training would have lost from taking part in educational training.

Programs vs. Public-sector employment program

The results of comparing public-sector employment training to different programs are presented in Figure 5.1a shows a positive significant effect in the short and medium- run, with only a short lock-in effect, implying that a participant in public-sector employment programs would be better off in a private-sector employment program in terms of improving the employment rate. Because a positive average treatment effect implies that a participant has a higher employment rate than a participant in a comparison program, and vise versa when the effect is less than zero. Figure 5.1b shows that given that you participate in public-sector program you would in the short-run be better of participating in educational training. However, in the medium run one would be worse off but in some quarters not significant.

Programs vs. Educational training

Figure 5.2a presents large positive significant effects of participating in private-sector employment program compared to participating in educational training both in the long and medium run. On the contrary, Figure 5.2b shows small negative significant effects in the short run of participating in public-sector employment program vs. educational training. In general, participants in educational training only gain significantly by moving into private-sector employment training. Given that educational training has the highest number of participants and is by fare the most expensive program (Jespersen et al (2008)) makes it a candidate for further investigation. By dividing participants into sub-groups by gender, age, and education it is possible to verify if anyone benefit[31].

Figure A1. In the Appendix shows the relative employment effect of programs vs. educational training across gender. Women seems to benefit more than men had they participated in private-sector employment programs. The results of private-sector employment programs vs. educational training (see Appendix Figure A2.) do not vary a lot but the highest effects are found amongst the 35-39 years and 45-50 years old. Other age groups only have small negative effects and are mostly not significant.

[31] Appendix Figure A.1-A.7 provides all the results of the subgroups estimations.

Figure 5.1a Relative employment effect

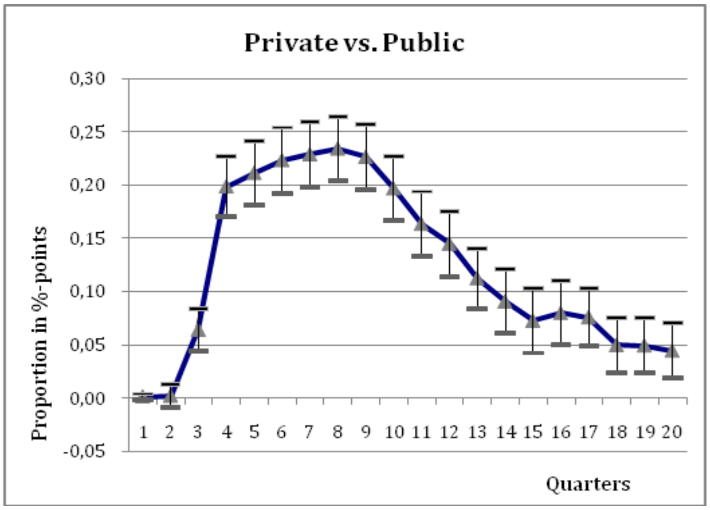


Figure 5.1b Relative employment effect

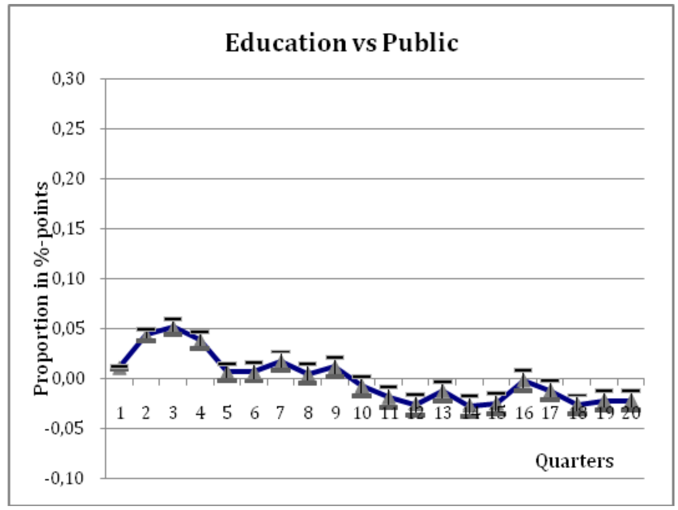


Figure 5.2a Relative employment effect

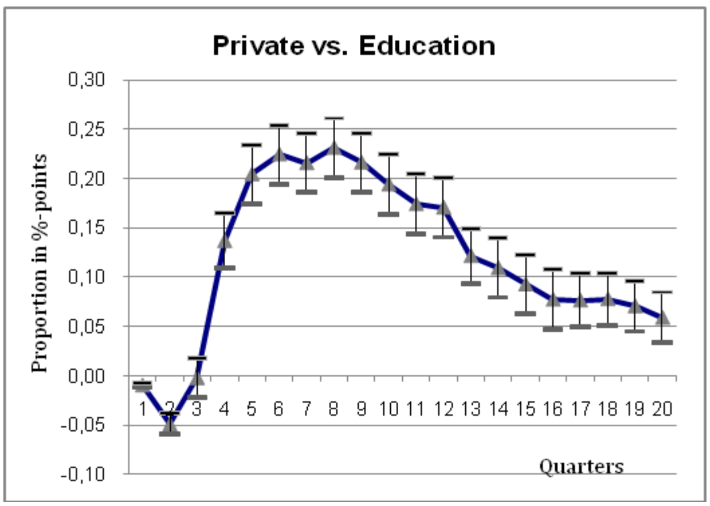


Figure 5.2b Relative employment effect

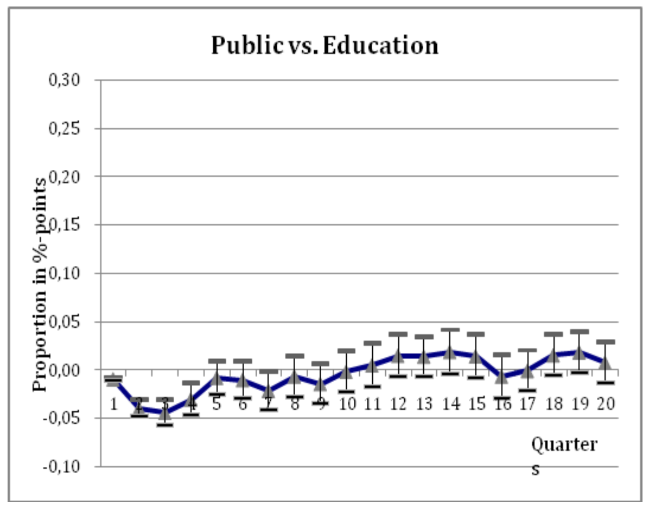


Figure 5.3a Relative employment effect

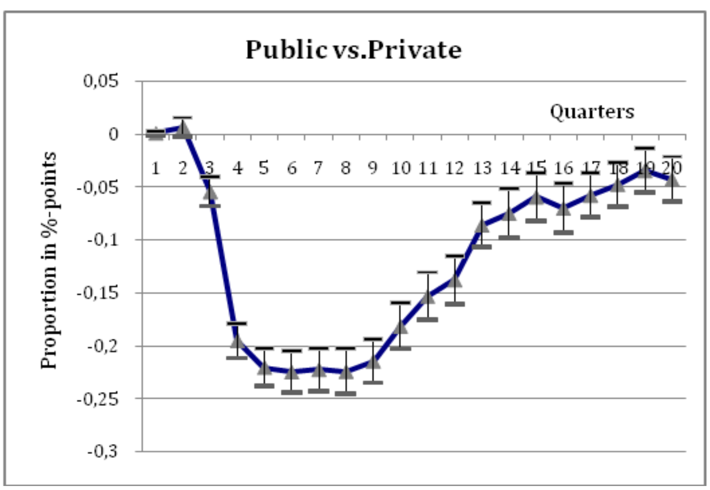
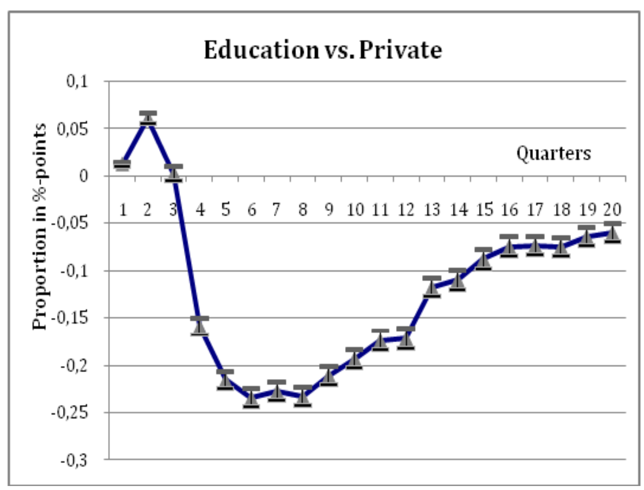


Figure 5.3b Relative employment effect



The impact across different education levels are expected to vary, as different educational levels may be motivated differently. Appendix Figure A4. and A5. show the results. Vocational training and unskilled seem to benefit the most moving from education training to private-sector employment programs.

Programs vs. Private-sector employment training

Given the results from above it is not surprising that every graph in Figure 5.3 show that a participant would be worse off by leaving private-sector employment program to participate in any of the other programs. Between 4 to approximately 13 quarters the effects are large, negative, and significant. In the medium-long run the negative effects are getting smaller.

Overall these results indicate that private-sector employment training derives the highest positive effect among all the programs. Some explanation of the positive effect might be due to the characteristics of private job training participants. First of all given that these participants are selected on the basis of the expectation of becoming future potential employees they tend to be more job ready than other types. However, the methodology and the rich data set should account for this selection bias. Nevertheless it might be the case that participants in employment programs are slightly "better" on average in terms of skills, qualifications, or experience than the matched comparisons. Secondly, the displacement effect is not taken into account. The design of the employment program may simply crowd out regular employment because a program participant might be hired instead of an unsubsidized worker who would have been hired otherwise. A policy conclusion would therefore not be to offer this program to all unemployed job-seekers, because this would be a way of subsidizing the private-sector sector. Among the sub-group comparisons (programs vs. educational training) women benefit the most from participating in private sector employment. Moreover, the age groups 35-39 is clearly better off in the long run participating in either private- or public-sector employment programs compared to educational training. Different qualifications impact the estimation results differently. It seems like mainly skilled and unskilled workers would be benefit from participating in private-sector employment programs rather that educational training.

Earnings results

The earning effects for all the above results are presented in Table 5.1. The registered annual labor income consists of all taxable wage income of the individual, indicating that wages earned while enrolled in subsidized private and public job training are included. This will explain why there is an immediate positive effect, while in the employment effect there is a negative effect (locking-in effect).

Table 5.1 Yearly average relative treatment effect for earnings

	2002	2003	2004	2005
Private vs. Public	**-10.447**	**-1.718**	**3.190**	**3.157**
95% Confidence	-9.484	-530	4.797	4.873
95% Confidence	-11.410	-2.905	1.584	1.442
Education vs. Public	**13.609**	**19.451**	**23.647**	**22.344**
95% Confidence	10.247	15.593	18.904	16.810
95% Confidence	16.972	23.310	28.392	27.877
Private vs. Education	**21.571**	**21.672**	**18.358**	**17.203**
95% Confidence	24.927	25.553	23.125	22.728
95% Confidence	18.215	17.791	13.591	11.677
Public vs. Education	**9.297**	**1.315**	**-3.296**	**-3.200**
95% Confidence	11.145	3.643	-192	-209
95% Confidence	74.849	-1.012	-6.400	-6.190
Public vs. Private	**-10.418**	**-15.929**	**-20.348**	**-21.099**
95% Confidence	-8.559	-13.572	-17.218	-18.054
95% Confidence	-12.276	-18.285	-23.478	-24.144
Education vs. Private	**-22.386**	**20.554**	**19.108**	**15.816**
95% Confidence	-21.423	-19.361	-17.496	-14.090
95% Confidence	-23.349	-21.748	-20.720	-17.544

Note: The confidence interval for differences between the treated and the comparison participants.

In addition, it should be kept in mind that these estimates of earnings effects are yearly and not quarterly as the employment rates. However, the main results of the earning effects confirm the above results on employment that the private-sector employment programs appear to be the most effective program with the highest positive wage effect followed by the public-sector employment program. The results for earnings effects among the sub-groups for different programs vs. educational training also resemble the results found on the employment rates[32].

Indeed it would be valuable to measure whether the most effective program also is the most cost effective program. But estimating a cost benefit analysis for this comparison is outside the scope of this analysis.

5.3 Sensitivity analysis

Matching is not a panacea, but if data can provide a plausible CIA, then it is a preferable evaluation estimator. In general, matching estimators can reduce (but not eliminate) the level of bias generated by the unobserved heterogeneity, see Heckman et al. (1999). It often depends on the quality of the control va-

[32] Results are presented in Appendix, Table A3-A5.

riables how well the estimator works. The quality of the matching can be analyzed by several sensitivity measures of the matching estimators for example by testing the balancing property of the propensity score, which is the key to consistency of propensity score matching estimators, see Ichino and Becker (2002). It is necessary that the propensity score specification is capable of balancing the covariates' distribution in the group of treated and non-treated units.

One of the measures that indicate the status of quality of the match is the mean standardized bias (MSB), between each treated - and matched comparison group across all variables of X as described in Rosenbaum and Rubin (1985)[33]. Furthermore, a joint significance test, Pseudo-R^2, and t-test for significant differences in covariate averages between the treatment and the comparison group, can indicate the balancing of covariate after matching. Appendix Table A.6 and Table A.7 present MSB, Pseudo-R^2, and observation of common support. The MSB is reduced and fairly low after matching. In all estimations a low value of the bias indicates more similarity between participants in the compared programs[34]. The Pseudo- R^2 is also very low especially after matching. Nearly all observations are in the common support region and it is therefore possible to produce a match for nearly all treatment observations. Only a few cases have a treatment observation off support. In addition, the propensity score distributions of each pair-wise comparison are presented in Appendix Figure A.6. They all show very similar distributions for treatment and comparisons, which indicate that the propensity scores overlap between the treated and non-treated[35]. Lechner-bounds that test the common support could also be implemented as another robustness check, but in this case almost none of the pair-wise comparison estimation seems to have problems with common support[36].

Ultimately the balance in the matched sample is of great concern, but specifying the model correct is also imperative. Shaikh et al. (2009) present a specification test for the propensity score using its distribution conditional on participation. This test uses the conditional densities of the treatment and the comparison to derive a formal test for misspecification. In every estimation at least at 5 % significant

[33] The mean standardized bias is defined as the difference of means in the treated and matched comparison sample, divided by the square root of the average sample variance.
[34] At a value of 5 percent the program type may not be comparable in respect to the participants but there is no clear evi- dence of when bias is acceptable but Caliendo and Hujer (2006) provide as a rule of thumb of a range between 3-5 percent as sufficient. In this study most estimation is between 1-5 percent.
[35] The same results are also found for sub group comparisons and are available on request.
[36] The Lechner bounds is defined by the weighted average of the estimated average treatment effect and the average distance of observations for treated persons throughout common support from the bounds potential outcome (Lechner (2000), Ca-

level, the specification can not be rejected. The results are insensitive to the choice of bandwidth see Appendix Table A.8. If the specification can not be rejected this will include consistent estimators.

6. Conclusion

This paper evaluates the employment and earning effects of participating in a large scale system of ALMPs in Denmark from 2002 through 2006. The focus is the relative effectiveness of three categories of ALMPs for unemployed insured recipients. The measurement of the relative effectiveness is a new contribution in the Danish evaluation literature. In light of the mandatory participation it is particularly interesting to study how unemployed individuals perform by joining one program compared relatively to the performance had they joined another program. The treatment effects are estimated by propensity score matching on a rich administrative data set, which makes it possible to estimate medium-long term effects and account for individual heterogeneity.

The results of the pair-wise comparison suggest that one type of program dominates in terms of increased employment and earnings rate. Private-sector employment program is by far the best option, though in the medium-long run the relative effects are decreasing but remain positively significant. For all the different combination of pair-wise comparison the participants in private-sector employment program are better off both in terms of employment- and earnings prospects. This result corroborate with earlier studies of Danish ALMPs, which also show that private-sector employment program generates the highest impact by comparing to non participants. Internationally this result is also in line with other results in the literature (see Kluve et al. (2007) for an overview). The new contributions of this study beside the measurement of the relative effects are the inclusion of a recent data set for the whole population of Denmark, the estimation of medium-long relative effects, and the inclusion of caseworker assessment as control variable. By applying these components produces a valuable and rigor input to the existing literature.

Still, to make any policy recommendation about what works best for whom it is essential to measure the relative treatment effects across sub-groups. This study demonstrates that programs do not work

liendo and Kopeing (2005)).

equally well for different individuals. Several heterogeneous effects with respect to gender, age, and qualification seem to be important for the outcome. For example, participants in educational training are better of in terms of employment and earnings if they instead participate in private-sector employment programs and the effects are slightly larger for women than men. This fits well with international results that generally find that women tend to benefit more in private-sector employment programs. In the literature it is found that when female labor force participation increase over time, the effectiveness of ALMP seems to decrease for women, and move into more limited effectiveness (see Bergemann and van den Berg (2006) for an overview). This could explain why the effect is rather small given that Denmark has a high female labor participation rate. Moreover, different age groups seems to benefit differently, the 35-39 years old benefit from moving out of educational programs, especially when they move into private-employment programs. In some cases, this also applies to individuals with low educational qualifications. Unskilled and skilled individuals benefit more had they participated in private-sector employment programs instead of educational training. From a policy perspective this is a central finding given that educational training on average is rather expensive.

Introduction of ALMP system with compulsory participation has intensified the amount of conducted studies and especially studies that focus on the incentive or threat effect of ALMP. Given that this paper contributes with new knowledge about the relative effect between programs, improvement could still be made to achieve even more unbiased results. Pointing a direction for further research would be to investigate the effect of private-sector employment programs by study the displacement effect, which may crowd out regular employment.

Finally, some caveats of this study are to measure the cost effectiveness of the programs, especially given the size of the programs in Denmark. The obvious questions to explore are for example "Is the most effective program also the most cost effective one?” or “How effective is the most expensive program?" To answer these questions, it is necessary to identify and compare the benefits and respective costs of a program. In addition, the concern of defining a credible comparison group in ongoing programs is also a future work to be considered. Hence, applying dynamic treatment effect and taking into account that the programs are ongoing would reveal more insight on the estimated effect. But these aspects are beyond the scope of this study.

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Appendix: Tables and Figures

Figure A1. Relative employment effect of programs vs. educational training across gender

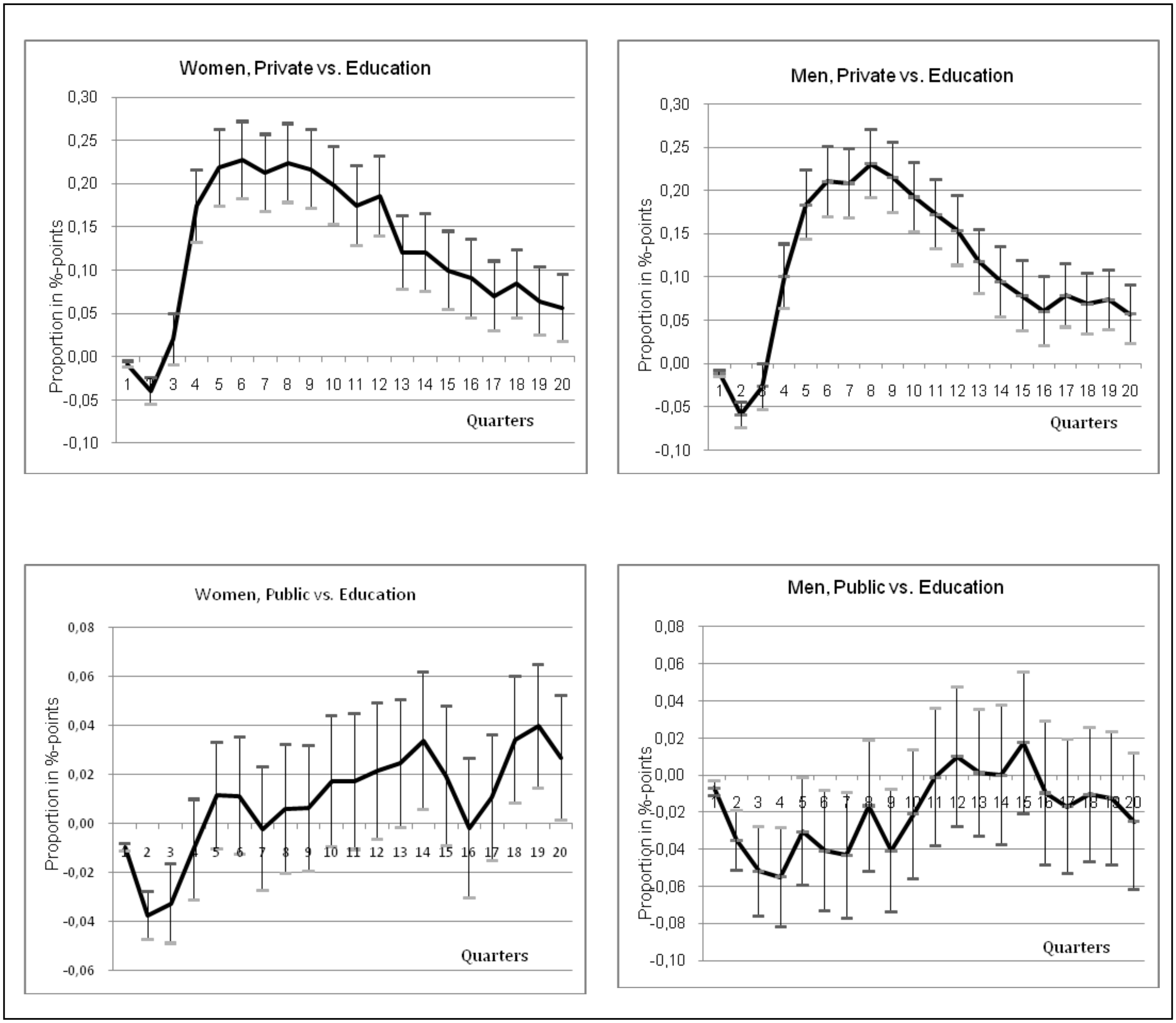

Figure A2. Relative employment effect of <u>private</u> vs. educational training across age

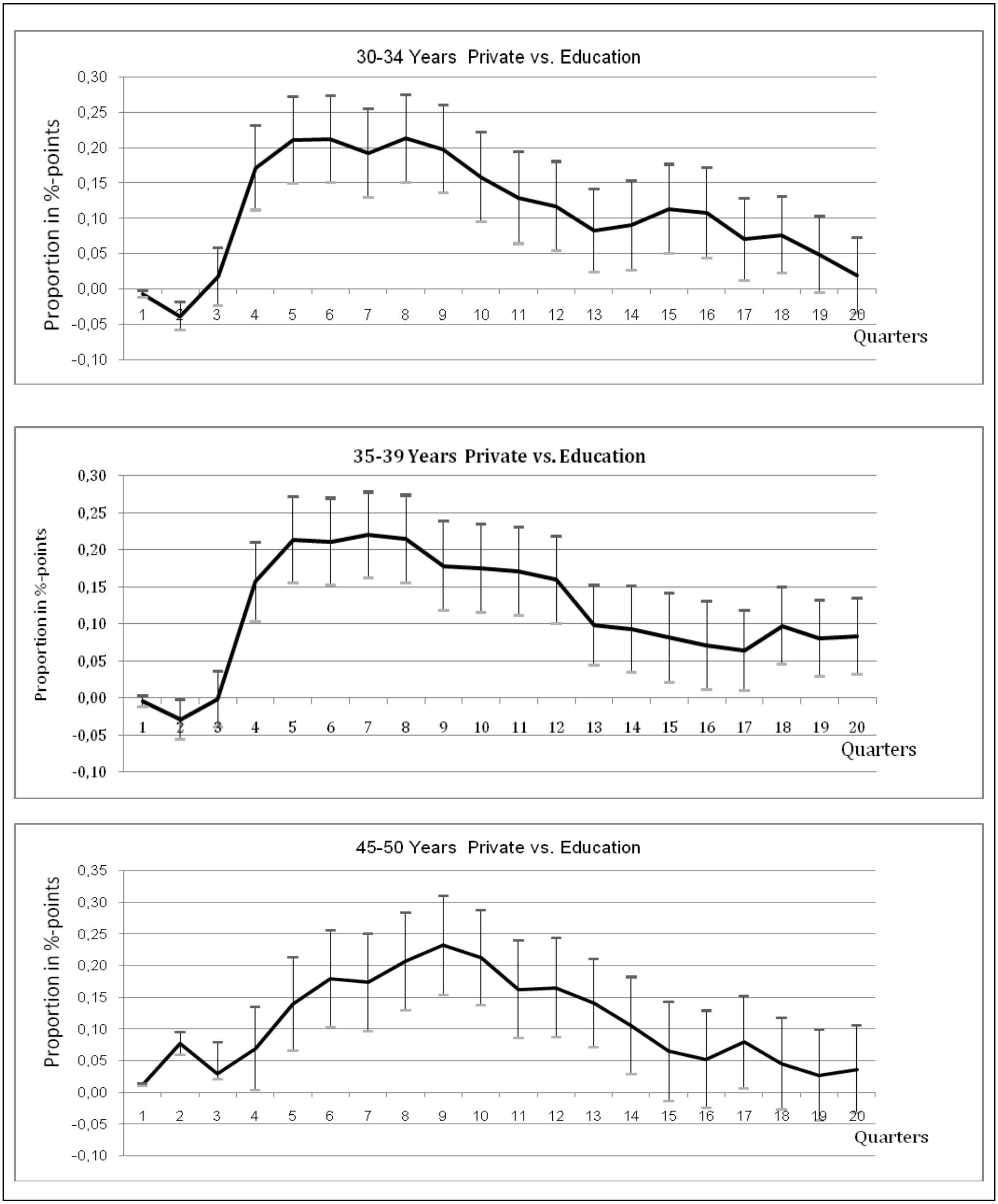

Figure A3. Relative employment effect of <u>public</u> vs. educational training across age

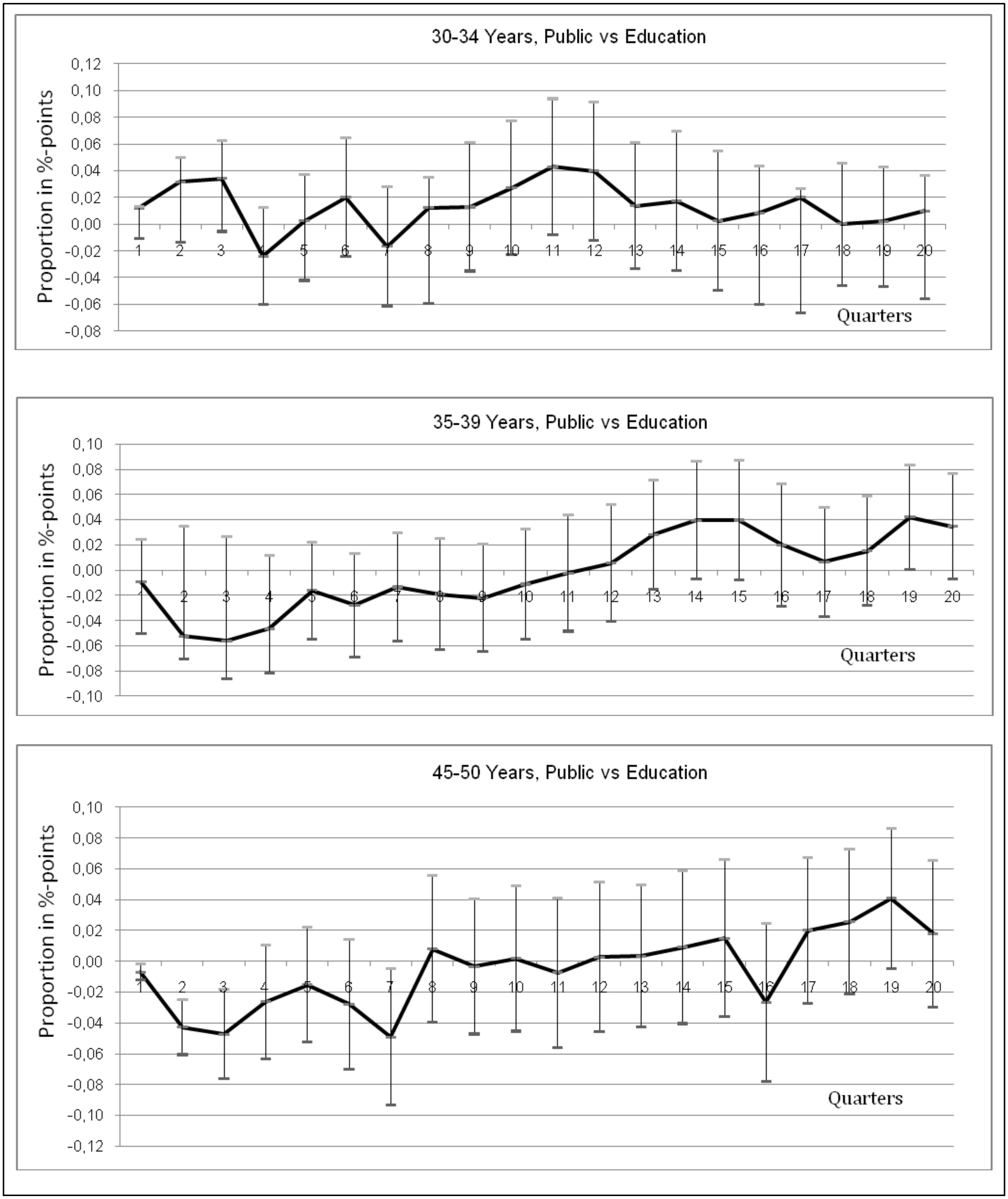

Figure A.4 Relative effect of Private vs. Education across education

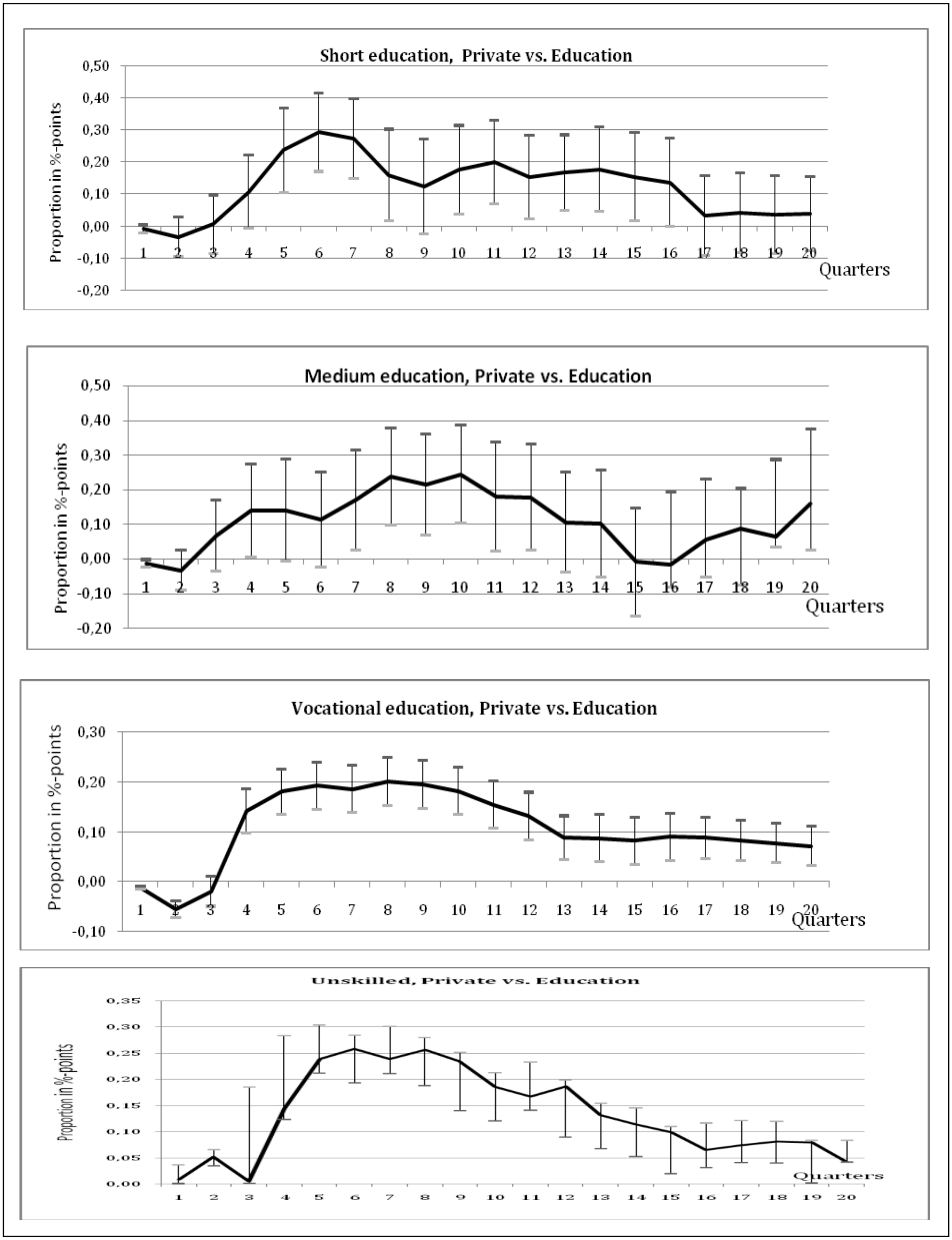

Figure A.5 Relative employment effect of <u>Public</u> vs. education across education

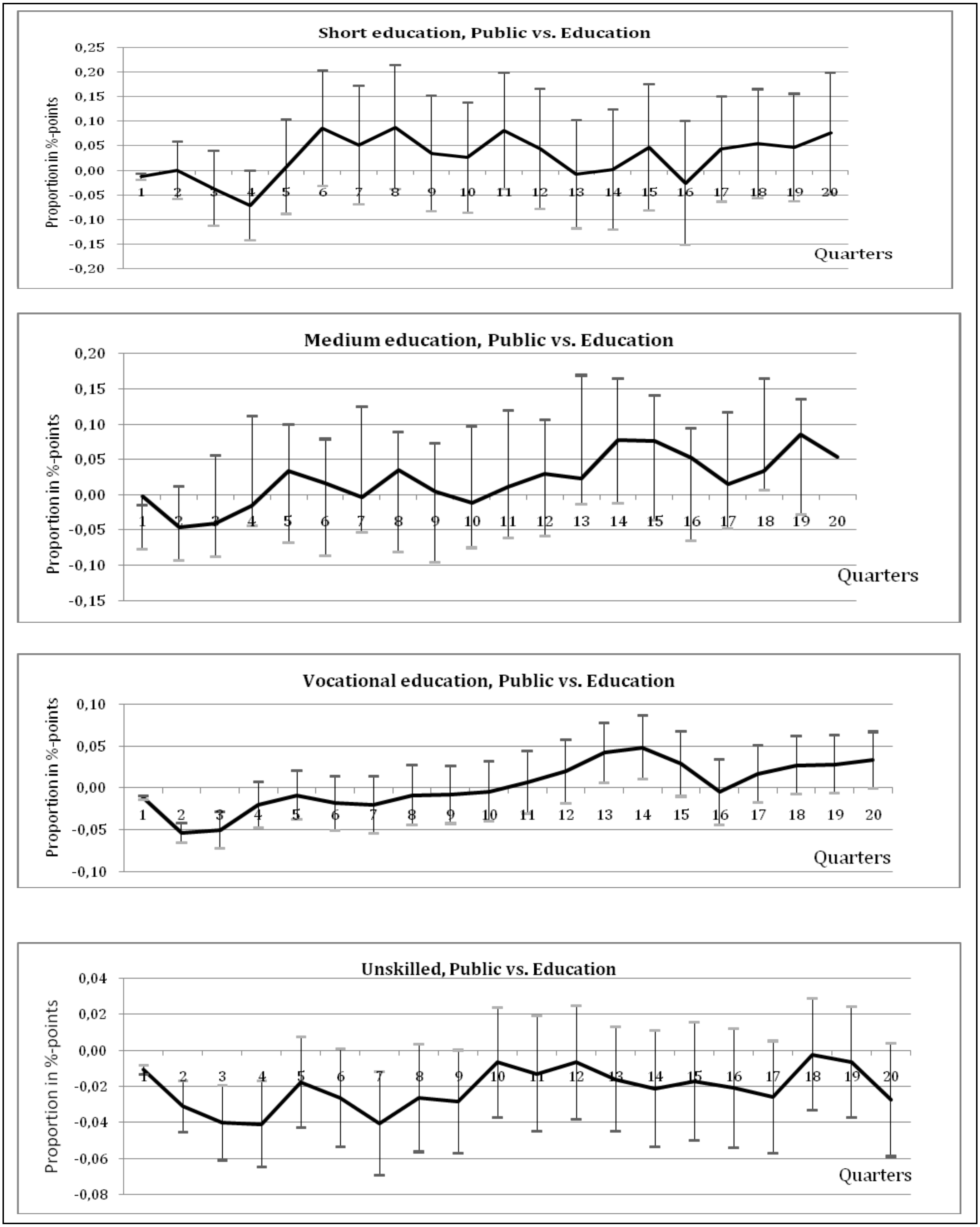

Figure A.6 Sensitivity analysis: Propensity score distributions

1

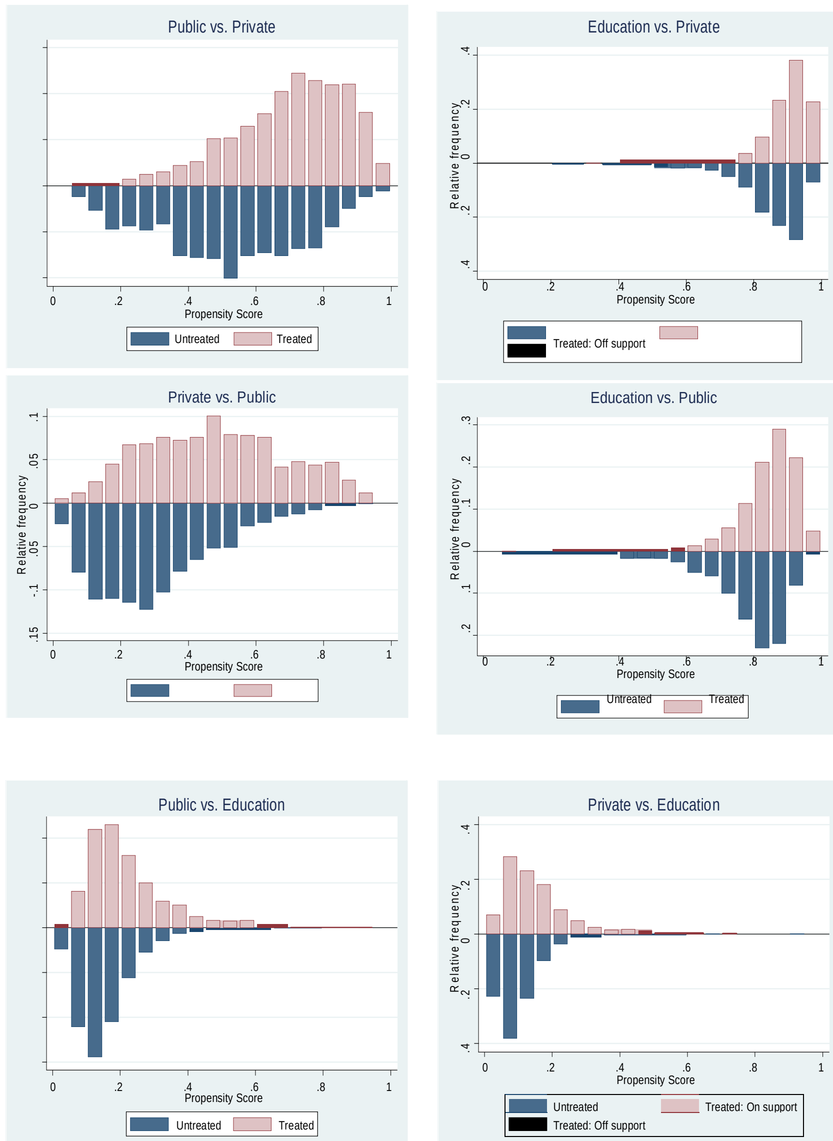

Figure 7.A Sensitivity analysis: Different matching algorithm

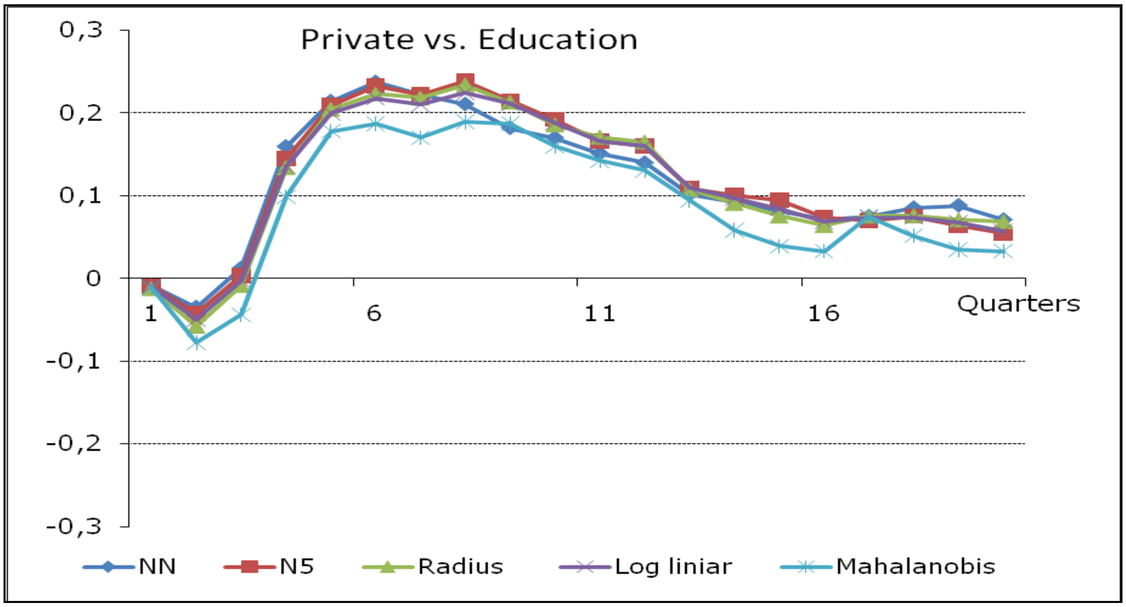

Table A.1Variable description

Variable	Description	Data Source
Age 30 - 39	Dummy indicator for age (=1 if between 30-39 years old =0 if otherwise)	IDA
Age 35-39	Dummy indicator for age (=1 if between 30-39 years old =0 if otherwise)	IDA
Age 40 - 44 *	Dummy indicator for age (=1 if between 40-44 years old = 0 if otherwise)	IDA
Age 45-50	Dummy indicator for age (=1 if between 45-50 years old =0 otherwise)	IDA
Women	Dummy indicator for women	IDA
Married	Dummy indicator for being married	IDA
Kids 0-2 years old	Dummy for having kids in the age-group 0-2 years old	IDA
Kids 3-6 years old	Dummy for having kids in the age-group 0-2 years old	IDA
Kids 7-9 years old	Dummy for having kids in the age-group 0-2 years old	IDA
Kids 10-14 years old	Dummy for having kids in the age-group 0-2 years old	IDA
Kids 15-17 years old	Dummy for having kids in the age-group 0-2 years old	IDA
Primary	Primary	IDA
Short education	Education for 2 years (Upper secondary school e.g. high school)	IDA
Medium education	Education for 3-4 years (e.g. Bachelor)	IDA
Higher education	Education for 5 years and more (Masters, PhD, etc.)	IDA
Vocational education	Education for work	IDA
Unskilled	Low secondary or less (Unqualified worker)	IDA
High	High grades in high school	IDA
Top	Top grades in high school	IDA
Low*	Low grades in high school	IDA
Bottom	Bottom grades in high school	IDA
Medium	Medium grades in high school	IDA
Work experience in years	Work experience in years calculated 1966	CRAM
Citizenship	Citizenship	IDA
UI fund member:	UI fund member:	IDA
Build	Building industry	IDA
Metal	Production	IDA
KAD	Academia	IDA
Manufacture.	Manufacturing	IDA
Tech	Technical	IDA
Trade	Trade	IDA

Services	Services	IDA
Professional	Professional	IDA
Self-employed	Self-employed	IDA
OTHERS*	Reference	IDA
Duration of unemployment	Unemployment duration in 2001	CRAM
First U spell	Indicates whether the present unemployment spell is the first	CRAM
Unemployment rate	Proportion of time spend in unemployment	CRAM
Sick rate	Proportion of time receiving sickness benefits	CRAM
Employment rate	Proportion of time spend in employment	CON
Mean duration E 1988-2002	Mean employment duration since 1988-2002	CRAM
Mean duration U 1988-2002	Mean unemployment duration since 1988-2002	CRAM
UI Seniority in days	Denotes the number of weeks the unemployed previously were unemployed and received UI benefit at the beginning of the present unemployment spell	CRAM, SHS
Proportion with no UI Seniority	Proportion with no UI seniority measured by a dummy indicator (if unemployment insurance seniority = 0 at the beginning at the unemployment period and =1 if UI seniority was zero and = 0)	CRAM, SHS
# U spells last year	The number of unemployment spells last year	CRAM
# U spells two years ago	The number of unemployment spells two years ago	CRAM
# E spells last year	The number of employment spells last year	CON
# E spells two years ago	The number of employment spells two years ago	CON
Private-sector job training 01	Dummy indicator for participation in private-sector job training in 2001	AMFORA
Public-sector job training 01	Dummy indicator for participation in public-sector job training in 2001	AMFORA

Educational training 01	Dummy indicator for participation in educational training in 2001	AMFORA
Country side	Country side	IDA
Metropolis	Largest cities in Denmark	IDA
Local labor market variable:		IDA
Copenhagen	Number of participants in a program within the county divided by all individual in programs	IDA
Frederiksborg*	Number of participants in a program within the county divided by all individual in programs	IDA
Roskilde	Number of participants in a program within the county divided by all individual in programs	IDA
Vestsjællande	Number of participants in a program within the county divided by all individual in programs	IDA
Storstrøms	Number of participants in a program within the county divided by all individual in programs	IDA
Bornholm	Number of participants in a program within the county divided by all individual in programs	IDA
Fyn	Number of participants in a program within the county divided by all individual in programs	IDA
Soenderj	Number of participants in a program within the county divided by all individual in programs	IDA
Ribe	Number of participants in a program within the county divided by all individual in programs	IDA
Vejle	Number of participants in a program within the county divided by all individual in programs	IDA
Ringko	Number of participants in a program within the county divided by all individual in programs	IDA
Aarhus	Number of participants in a program within the county divided by all individual in programs	IDA
Viborg	Number of participants in a program within the county divided by all individual in programs	IDA
Nordjyl	Number of participants in a program within the county divided by all individual in programs	IDA

*Reference in the logit estimation

IDA: Danish database for labor market research

CRAM: A register for unemployed

AMFORA: Statistics that include information about the employment duration

CON: Central register for employment and employer

SHS: Social income transfers

Table A.2 Average derivative estimates and std.err. from participation logits

	Private vs. Public		Education vs. Public		Private vs. Education		Public vs. Education		Public vs. Private		Education vs. Private	
Variables [a]	Coeff.	Std.Err	Coeff.	Std.Err	Coeff.	Std.Err	Coeff.	Std.Err	Coeff.	Std.Err	Coeff.	Std.Err
Age 30 - 34	0,06	0,08	0,05	0,04	0,03	0,05	-0,05	0,04	-0,06	0,08	-0,03	0,05
Age 35-39	0,12	0,07	0,05	0,04	0,05	0,05	-0,05	0,04	-0,12	0,07	-0,05	0,05
Age 45-50	-0,08	0,09	0,00	0,05	-0,11	0,06	0,00	0,05	0,08	0,09	0,11	0,06
Women	**-0,50**	0,07	**-0,08**	0,04	**-0,34**	0,04	**0,08**	0,04	**0,50**	0,07	**0,34**	0,04
Married	-0,10	0,06	-0,01	0,04	-0,06	0,04	0,01	0,04	0,10	0,06	0,06	0,04
Kids 0-2 years old	0,09	0,08	-0,04	0,05	0,08	0,06	0,04	0,05	-0,09	0,08	-0,08	0,06
Kids 3-6 years old	0,10	0,07	**0,11**	0,04	-0,02	0,05	**-0,11**	0,04	-0,10	0,07	0,02	0,05
Kids 7-9 years old	0,03	0,08	0,06	0,04	-0,04	0,05	-0,06	0,04	-0,03	0,08	0,04	0,05
Kids 10-14 years old	0,07	0,07	0,00	0,04	0,07	0,05	0,00	0,04	-0,07	0,07	-0,07	0,05
Kids 15-17 years old	-0,08	0,12	0,10	0,07	**-0,15**	0,09	-0,10	0,07	0,08	0,12	0,15	0,09
Short education (2 years)	0,34	0,19	**0,18**	0,11	0,09	0,13	**-0,18**	0,11	**-0,34**	0,19	-0,09	0,13
Medium education (3-4 years)	0,20	0,18	0,10	0,09	0,00	0,12	-0,10	0,09	-0,20	0,18	0,00	0,12
Vocational education	**0,32**	0,16	-0,07	0,08	**0,26**	0,11	0,07	0,08	**-0,32**	0,16	**-0,26**	0,11
Unskilled (non-qualified)	0,22	0,16	-0,07	0,08	**0,19**	0,11	0,07	0,08	-0,22	0,16	-0,19	0,11
Top	1,68	0,62	0,68	0,44	**0,64**	0,27	-0,68	0,44	**-1,68**	0,62	-0,64	0,27
High	**0,19**	0,23	0,01	0,13	0,15	0,15	-0,01	0,13	-0,19	0,23	-0,15	0,15
Bottom	0,00	0,15	-0,08	0,08	0,07	0,10	0,08	0,08	0,00	0,15	-0,07	0,10
Medium	0,26	0,19	0,16	0,11	0,02	0,12	**-0,16**	0,11	-0,26	0,19	-0,02	0,12
Work experience in years	**-0,01**	0,01	**-0,01**	0,00	**0,01**	0,00	**0,01**	0,00	**0,01**	0,01	**-0,01**	0,00
Build	0,01	0,16	0,07	0,10	-0,05	0,11	-0,07	0,10	-0,01	0,16	0,05	0,11
Metal	**0,38**	0,16	**0,34**	0,10	-0,05	0,10	**-0,34**	0,10	**-0,38**	0,16	0,05	0,10
Academia	-0,06	0,12	0,09	0,06	**-0,13**	0,09	-0,09	0,06	0,06	0,12	0,13	0,09
Manufacture	**0,25**	0,08	**0,22**	0,05	**-0,03**	0,06	**-0,22**	0,05	**-0,25**	0,08	0,03	0,06
Tech	**0,46**	0,13	**0,21**	0,08	**0,16**	0,08	**-0,21**	0,08	**-0,46**	0,13	**-0,16**	0,08
Trade	**0,26**	0,09	**0,10**	0,05	0,10	0,06	**-0,10**	0,05	**-0,26**	0,09	-0,10	0,06
Service	0,17	0,09	0,18	0,09	0,17	0,09	-0,08	0,06	0,18	0,12	**0,18**	0,08
Self-employed	-0,19	0,13	**0,09**	0,07	-0,19	0,09	**-0,09**	0,07	0,19	0,13	0,19	0,09
Duration of unemploy-ment	0,24	0,20	**0,35**	0,11	-0,19	0,12	**-0,35**	0,11	-0,24	0,20	**0,19**	0,12
First U spell	0,00	0,00	-0,01	0,00	0,00	0,00	0,01	0,00	0,00	0,00	0,00	0,00
Unemployment rate	-0,02	0,13	-0,06	0,08	0,00	0,08	0,06	0,08	0,02	0,13	0,00	0,08
Sick rate	0,05	0,27	-0,11	0,16	0,04	0,20	0,11	0,16	-0,05	0,27	-0,04	0,20
Employment rate	**0,04**	0,56	**-0,29**	0,35	0,06	0,42	**0,29**	0,35	**-0,04**	0,56	-0,06	0,42
Mean duration E 1988-2002	0,63	0,20	0,49	0,11	-0,05	0,13	-0,49	0,11	-0,63	0,20	0,05	0,13
Mean duration U 1988-2002	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00
UI Seniority in days	0,00	0,00	**0,00**	0,00	**0,00**	0,00	**0,00**	0,00	0,00	0,00	**0,00**	0,00

Proportion with no UI Seniority	**0,00**	0,00	**0,00**	0,00	0,00	0,00	**0,00**	0,00	**0,00**	0,00	0,00	0,00
# U spells last year	0,16	0,08	0,14	0,05	0,07	0,05	-0,14	0,05	-0,16	0,08	-0,07	0,05
# U spells two years ago	**0,00**	0,04	0,00	0,03	**-0,01**	0,03	0,00	0,03	**0,00**	0,04	0,01	0,03
# E spells last year	-0,09	0,05	**-0,03**	0,03	-0,06	0,04	**0,03**	0,03	0,09	0,05	0,06	0,04
# E spells two years ago	0,00	0,04	0,04	0,02	-0,03	0,03	-0,04	0,02	**0,00**	0,04	**0,03**	0,03
Private-sector job training 01	**0,04**	0,10	**0,03**	0,06	**0,02**	0,07	-0,03	0,06	**-0,04**	0,10	-0,02	0,07
Public-sector job training 01	**0,79**	0,19	**-0,13**	0,16	0,85	0,13	**0,13**	0,16	**-0,79**	0,19	**-0,85**	0,13
Educational training 01	**-0,80**	0,17	-0,83	0,09	**-0,01**	0,17	**0,83**	0,09	**0,80**	0,17	0,01	0,17
Country side	0,06	0,13	-0,10	0,07	0,12	0,09	0,10	0,07	-0,06	0,13	-0,12	0,09
Metropolis	**0,36**	0,16	0,10	0,09	**0,19**	0,11	-0,10	0,09	**-0,36**	0,16	-0,19	0,11
Local labor market:												
Copenhagen	-0,07	0,05	-0,03	0,03	-0,03	0,03	0,03	0,03	0,07	0,05	0,03	0,03
Roskilde	**-0,14**	0,08	-0,03	0,04	-0,09	0,05	0,03	0,04	**0,14**	0,08	0,09	0,05
Vestsjællande	0,01	0,07	**0,09**	0,04	**-0,08**	0,05	**-0,09**	0,04	-0,01	0,07	0,08	0,05
Storstrøms	-0,08	0,05	**-0,10**	0,03	0,04	0,04	**0,10**	0,03	0,08	0,05	-0,04	0,04
Bornholm	-0,18	0,11	0,03	0,06	-0,12	0,08	-0,03	0,06	0,18	0,11	0,12	0,08
Fyn	-0,08	0,07	0,01	0,04	**-0,08**	0,04	-0,01	0,04	0,08	0,07	0,08	0,04
Soenderj	0,01	0,07	-0,06	0,04	0,07	0,05	0,06	0,04	-0,01	0,07	-0,07	0,05
Ribe	-0,06	0,09	-0,02	0,05	-0,04	0,06	0,02	0,05	0,06	0,09	0,04	0,06
Vejle	**0,10**	0,05	0,02	0,03	**0,07**	0,03	-0,02	0,03	**-0,10**	0,05	**-0,07**	0,03
Ringko	**0,10**	0,06	0,01	0,04	0,06	0,04	-0,01	0,04	-0,10	0,06	-0,06	0,04
Aarhus	-0,08	0,06	-0,02	0,04	-0,06	0,04	0,02	0,04	0,08	0,06	0,06	0,04
Viborg	**0,16**	0,08	0,01	0,05	0,07	0,05	-0,01	0,05	**-0,16**	0,08	-0,07	0,05
Nordjyl	0,02	0,06	0,02	0,04	0,00	0,04	-0,02	0,04	-0,02	0,06	0,00	0,04
Constant	**-0,50**	0,30	**1,40**	0,17	**-1,64**	0,21	**-1,40**	0,17	0,50	0,30	**1,64**	0,21

[a]*Note*: Bold numbers indicate significance at the 5% level. Variable explanations are found in Appendix A.1. The references are for age dummies are group of ”40-44 years”. For education “higher education” is the reference. In the grades category “Low” is the reference. In the union membership the category “Other” is the reference. In the local labor market “Frederiksborg” is the reference category.

Table A.3 Relative yearly earnings effect of programs vs. educational training across gender

	2002	2003	2004	2005
Private men	**22058**	**23056**	**17952**	**16866**
95% Confidence	27352	28919	25216	24959
95% Confidence	16763	17192	10689	8773
Private women	**19576**	**18141**	**16631**	**15613**
95% Confidence	23108	22674	22198	22769
95% Confidence	16044	13607	11065	8457
Public men	**568**	**-655**	**-6391**	**-4378**
95% Confidence	8813	3245	-1299	1485
95% Confidence	3122	-4556	-11482	-10261
Public women	**11609**	**2947**	**-714**	**-1980**
95% Confidence	14004	5857	3218	1331
95% Confidence	9213	38	-4646	-592

Note: The confidence interval for differences between the treated and the comparison participants.

Table A.4 Relative yearly earnings effect of programs vs. educational training across age

	2002	2003	2004	2005
Private 30-34 years	**20282**	**24195**	**16241**	**24753**
95% Confidence	14591	16142	6613	126045
95% Confidence	27065	32249	25868	36422
Private 36-39 years	**24210**	**21401**	**24041**	**12807**
95% Confidence	17403	13802	14483	1041
95% Confidence	31017	28999	33599	24572
Private 45 years	**20683**	**8915**	**8995**	**12310**
95% Confidence	8070	-844	-3600	-169
95% Confidence	33296	18674	21590	24790
Public 30-34 years	**10387**	**1373**	**351**	**1214**
95% Confidence	7919	-3177	-5578	-5607
95% Confidence	13828	5924	6281	8036
Public 36-39 years	**6202**	**-1491**	**-5330**	**-8326**
95% Confidence	3395	6128	-11016	-14379
95% Confidence	9009	3144	354	-2274
Public 45 years	**8645**	**2210**	**-5582**	**-4023**
95% Confidence	5033	-2141	-11177	-10576
95% Confidence	12257	6562	12	2529

Note: The confidence interval for differences between the treated and the comparison participants.

Table A.5 Relative yearly earnings effect of programs vs. educational training across education

Income	2002	2003	2004	2005
PRIVATE VS. EDUCATION				
Short education	**23948**	**12533**	**29580**	**8638**
95% Confidence	11357	-5242	-2551	-30202
95% Confidence	36539	30309	43712	47479
Medium education	**23219**	**16899**	**21123**	**30640**
95% Confidence	8202	-5824	-11149	-4890
95% Confidence	38237	39624	53397	66170
Vocational education	**22839**	**18964**	**15185**	**16860**
95% Confidence	17084	13429	7902	8988
95% Confidence	28595	24500	22469	24731
Unskilled	**19642**	**21859**	**16088**	**12176**
95% Confidence	24403	28032	22865	19566
95% Confidence	14882	15687	9311	4786
PUBLIC VS. EDUCATION				
Short education	**1318**	**8969**	**8167**	**4678**
95% Confidence	3589	-3729	-7327	13906
95% Confidence	22774	21667	23662	23263
Medium education	**11968**	**10817**	**1669**	**2081**
95% Confidence	5020	1995	-10369	-1092
95% Confidence	18916	19638	13709	15082
Vocational education	**8247**	**139**	**-5060**	**-4559**
95% Confidence	5775	-3146	-9156	-9160
95% Confidence	10720	3424	-965	40
Unskilled	**8815**	**-1244**	**-6287**	**-6612**
95% Confidence	6837	-4040	-9886	-10642
95% Confidence	10792	1550	-2687	-2581

Note: The confidence interval for differences between the treated and the comparison participants.

Table A.6 Sensitivity analysis: Matching quality for the pair-wise comparisons: Program vs. Program

	MSB before	MSB after	Pseudo R^2 before	Pseudo R^2 after	Treated on support	Controls on support	Treated on support	% off support
Private vs. Public	10,3	2,3	0,12	0,01	935	1617	0	0
Education vs. Public	7,6	1,5	0,1	0,01	8060	1617	0	0
Private vs. Education	7,2	1,1	0,1	0,002	935	8060	0	0
Public vs. Education	7,6	0,8	0,1	0,001	1617	8060	0	0
Public vs. Private	10,3	1,8	0,1	0,01	1617	935	0	0
Education vs. Private	7,2	2,3	0,1	0,01	8060	935	0	0

Table A.7 Sensitivity analysis: Matching quality for the pair-wise comparisons: Program vs. Education

Pair-wise comparisons	MSB before	MSB after	Pseudo R^2 before	Pseu-do R^2 after	Treated on sup-port	Controls on sup-port	Treated off sup-port
PRIVATE VS. EDUCATION							
Men	7,3	0,9	0,1	0,001	522	3311	0
Women	7,4	1,8	0,1	0,01	413	4749	0
PUBLIC VS. EDUCATION							
Men	11,3	1,2	0,1	0,003	577	3299	0
Women	6,7	0,8	0,1	0,001	1039	4749	1
OTHER VS. EDUCATION							
Men	12	3,34	0,174	0,024	199	3248	2
Women	14,04	5,15	0,24	0,04	215	4662	2
PRIVATE VS. EDUCATION							
30-34 years	9,1	1,8	0,12	0,005	224	1890	0
35-39 years	8,9	2,4	0,10	0,01	246	1949	2
45 years	10,5	2,4	0,12	0,01	144	1395	2
PUBLIC VS. EDUCATION							
30-34 years	8,7	1,8	0,08	0,01	378	1948	2
35-39 years	7,7	1,4	0,07	0,004	335	1896	0
45 years	10,6	1,5	0,10	0,003	337	1398	6
PRIVATE VS. EDUCATION							
Short education (2 years)	13,0	4,4	0,16	0,03	54	451	1
Medium education (3-4 years)	12,9	4,5	0,17	0,3	45	575	1
Vocational education	7,6	1,1	0,08	0,002	387	2815	1
Unskilled (non-qualified)	8,1	1,3	0,08	0,003	405	3547	0
PUBLIC VS. EDUCATION							
Short education (2 years)	14,6	5,3	0,24	0,04	64	504	1
Medium education (3-4 years)	10,1	1,5	0,1	0,01	108	657	0
Vocational education	9,2	0,9	0,1	0,001	580	2819	4
Unskilled (non-qualified)	8,4	1,1	0,1	0,002	767	3542	0

Table A.8 Sensitivity analysis: Results from specification test

	Bandwidth c=0.05		Bandwidth c=0.10		Bandwidth c=0.15	
	Test statistic	P-value	Test statistic	P-value	Test statistic	P-value
Public vs. Private	-2,21	0,03	-1,74	0,18	-1,4	0,16
Education vs. Private	-1,5	0,13	-1,91	0,05	-1,05	0,31
Private vs. Public	-2,21	0,03	-1,74	0,18	-1,4	0,16
Education vs. Public	-0,58	0,56	-0,41	0,68	-0,57	0,56
Public vs. Education	-0,58	0,56	-0,41	0,68	-0,57	0,56
Private vs. Education	-1,5	0,13	-1,91	0,05	-1,05	0,31