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Education at Workplaces:
Long-Term Unemployment, Wages and Enrolment

by

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Preface

This Ph.D thesis consists of three separate articles, which focus on qualifications received at workplaces. The articles were written between 2004-2007 at the Danish National Centre for Social Research (DNCSR). The DNCSR and the Danish Employment Ministry (DEM) financed the research through a Ph.D. stipend. Throughout the Ph.D. research I have been enrolled in the Ph.D. program at The University of Copenhagen (UC). I also spent 5 months at the Universitat Pompeu Fabra (UPF) in Barcelona, Spain during the Ph.D. period.

I want to thank DNCSR and DEM for providing me with a stipend and for giving me many financial opportunities to participate in courses, conferences, and seminars overseas. Furthermore, the DNCSR and The National Labor Market Authority (NLMA) have been instrumental in helping me obtain the data I needed for my Ph.d. Many of my colleagues at DNCSR have also been very supportive and helpful during the Ph.d. period. Jane has especially been a true friend.

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Finally the support and encouragement I have received from family and friends, especially from my husband James, has made me believe that a Ph.D. was the right choice. The best feeling throughout the Ph.D. period has been to see my daughter grow up. Lately, my daughters comments have also been a source of motivation. She wanted me to finish my Ph.D. so I would have more time to play with her.

Cecilie Dohlmann Weatherall

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Introduction and Summary

Today the common opinion is that the driving force behind increasing participation rates, steady employment and competitiveness in a globalized world is education. Therefore Governments from developed countries focus on education from early childhood to advanced adulthood. The attention on education has resulted in major education initiatives in Denmark as well. Apart from the very generous subsidies given to formal education (i.e. vocational and further education) a lot of education initiatives have been created for adults. Especially, for adults who suffer from unemployment. In addition to the many initiatives by the Danish government to upgrade skills in the workforce there are also a lot of education and training initiatives at Danish workplaces. The results of upgrading skills among the adult workers at workplaces are the focus of this Ph.D. thesis.

The public debate on education among the adult workforce revolves around three concerns. The first major concern is the upgrading of skills among a group of potentially long-term unemployed workers. The first chapter examines this issue by looking at how last workplace experiences influence the risk of becoming long-term unemployed. The connection between workplace experience and long-term unemployment has not been evaluated in previous studies.

The second major concern is the continuing upgrading of skills in the workforce regardless of formal education. This concern is discussed in chapter two by looking at the effect of job-related training (JRT) on employees wage return. Very few Scandinavian studies have looked at the effect of extensive JRT on wage returns. Furthermore, the potential endogeneity problem between separation and wages is taken into account when evaluating the effect of JRT. This has not been done in previous JRT studies.

The third concern is to make formal education possible for many adults. In chapter three I evaluate the effect of an adult apprenticeship subsidy (AAS) on the attendance rate into vocational education (i.e. formal education). The AAS is an extraordinary generous subsidy that only exists in Denmark. Therefore the results are important for all the other countries that want to apply a similar subsidy to increase

vocational education among the adult population. To sum up this thesis looks at all three concerns separately for employees at Danish workplaces.

Previous literature on education and upgrading of skills show that it is difficult to separate the effect of education and JRT from individual specific effects and firm specific effects. To separate the effect one needs a lot of good data. This means that panel data full of detailed information on observable individual characteristics and workplace characteristics is needed. This Ph.D. makes use of several Danish register-based panel data sets on the Danish population and the Danish workplaces. The Ph.D. also combines the panels with surveys among employees and employers. This extensive use of data makes it possible to identify the effect of past workplace experiences, JRT and education subsidies that is different from previous literature.

In the following I summarize each chapter and the major conclusions. **Chapter 1** poses the question: **"Does the last workplace experience influence the risk of becoming long-term unemployed?". It** examines how individual characteristics and the characteristics of the last workplace in the private sector influence the likelihood of becoming long-term unemployed. While most studies focus on individual characteristics of the long-term unemployed this chapter looks at workplace characteristics in conjunction with long-term unemployment. The intuition is if a worker obtains some kind of skills or prestige from the last workplace that is in demand at other workplaces then the worker is expected to have a lower risk of becoming long-term unemployed after a job separation.

The correlation between observed former workplace experience and the risk of becoming long-term unemployed is possible to analyze because of an extraordinarily rich Danish register-based employer-employee panel data set from 1995-2001. The data is especially useful because it is possible to disentangle displaced workers in the private sector. Thereby the analysis avoids the sample selection caused by the correlation between workers' separation rates and expected job possibilities.

The results from a multinomial logit model show that displaced workers have a high risk of becoming long-term unemployed if they previously worked in workplaces with certain characteristics, such as small manufacturing firms with low shares of skilled employees. Thus at certain workplaces workers probably obtain

additional skills or prestige. This increases their future job possibilities and reduces their risk of becoming long-term unemployed after displacement.

Chapter 2 explores the question: "**Does job-related training increase future wages?"** The focus is on wage return to the extensive job related training (JRT) initiatives taking place at Danish workplaces. The Danish panel data, which includes administrative data and survey data on employers and employees, is used to analyse the effect of JRT on wages. The information on employees' participation in JRT in 1995, wages, and historical job shifts make it possible to take individual specific effects into account and to instrument job separation. To overcome the potential endogeneity between wages and job separations by using historical job shifts as an instrument is new in the JRT literature.

The results show that the OLS estimates are consistent even when job separation is included as an exogenous variable. Moreover women with vocational education who received JRT and then separated to a new job receive a high wage return. The JRT has a positive and significant effect on wage return among men and women with a vocational education. Surprisingly no wage return to JRT is found among other educational groups. Finally the overall wage return to the extensive JRT in Denmark is very small compared to international findings.

Chapter 3 asks the question **"Does subsidized adult apprenticeship improve the aggregate level of education?"** The purpose of this chapter is to evaluate the effect of the generous adult apprenticeship subsidy (AAS) on the attendance rate into vocational education from 1996-2003. The generous apprenticeship subsidy for adults over 25 years of age was introduced in 1997 to address the challenges of globalization and the increased demand for skills. The aim of the AAS was to increase vocational skill levels among the non-educated in order to fill job vacancies (i.e. prevent bottlenecks).

Through a simple theoretical human capital model, I show that AAS is likely to influence education decisions in the whole population. Additionally, a simulation of the model illustrates the difficulties of finding an empirical strategy capable of evaluating the effect of a subsidy in the absence of an obvious control group. The empirical strategy in this chapter is to examine the effect of the subsidy, given the exogeneous shift in AAS in 1997, among the unskilled 24 and 25-year-olds by the

difference-in-differences estimator used in international educational evaluation studies on a rich panel data.

The results show that the AAS has a significant positive effect on the vocational attendance rate among 25-year-old men in 1998. However 25-year-old unskilled women were not affected by the subsidy. Additionally, the AAS has no significant effect on the attendance rate after 1998. Thus, the results do not unambiguously find that a generous AAS increases the attendance rate among the non-educated, which was originally expected.

Chapter 1

Does the last workplace experience influence the risk of becoming long-term unemployed?

Abstract

Politicians throughout Europe are concerned about a group of potential long-term unemployed workers. While most studies focus on individual characteristics of this group of workers, this study examines how individual characteristics and last workplace characteristics in the private sector influence the likelihood of becoming long-term unemployed. The study uses an extraordinarily rich Danish register-based employer-employee panel data set from 1995 to 2001. Therefore it is possible to look at the correlation between observed former workplace experience and the risk of becoming long-term unemployed. The analysis is restricted to displaced workers in the private sector, avoiding the sample selection caused by the correlation between workers' separation rate and expected job possibilities. The results from a multinomial logit model show that displaced workers have a high risk of becoming long-term unemployed if they worked previously in workplaces with certain characteristics, such as small manufacturing firms with low shares of skilled employees. Thus at certain workplaces workers probably obtain additional skills or prestige, which increases future job possibilities and reduces their risk of becoming long-term unemployed after displacement.

1. Introduction

Throughout Europe a group of workers are more likely to end up in long-term unemployment than other groups of workers. The long-term unemployed have lower possibilities of job matching relative to other unemployed individuals, and thereby have a higher risk of structural problems in the labour market. In 2001, Denmark had 68.000 long-term unemployed.[1] The government subsidises these people in one of three possible states: passive unemployment, participation in active labour market training programs or leave-of-absence for training of more than one year. Despite a decrease from 95.000 long-term unemployed in 1996 to 68.000 long-term unemployed in 2001, the long-term unemployed still make up 2,5 percent of the workforce (see figure 1). Having many potential workers who are long-term unemployed is problematic for two reasons. First, given recent unfavourable demographic developments, most welfare states can not financially afford having any potential workers not working. Second, some workers appear to be unwillingly long-term unemployed, because they claim to desire working at the given wage in the labour market but remain without a job offer.

How individual characteristics and especially how last workplace experience influence a worker's risk of ending up in the unfavourable state - long-term unemployment - is precisely the focus of this paper.

Two factors have to coincide for an employee to end up becoming long-term unemployed. First, the worker has to separate from a job. Second, the worker must not be able to find a job again. Restricting the analysis to workers working in private sector workplaces with a minimum of 10 employees ensures the consistency of workplace characteristics. Among these private sectors workers the yearly average separation rate was close to 20 percent in 1995-2001.[2] A separation is when a worker is employed at a certain workplace one year and not employed at that workplace the following year. Here a workplace is defined as a legally-registered unit at a specific address. Thus the separation rate includes intra-firm movements because firms easily can contain more workplaces. Due to the fact that workers have to be working for at least one year and public benefit and support are taken into account the separation rate

[1] Author's calculations on the Danish register-based employer-employee panel data set from 1995 to 2001 described in section 3.
[2] Author's calculations on the Danish register-based employer-employee panel data set from 1995 to 2001 described in section 3.

in this paper is lower than previous Danish studies, which find an average separation rate of 30 pct. (Frederiksen and Westergaard-Nielsen 2002). In other words a person who receives an unemployment benefit while working and then separates into unemployment is not recognized as an separation in this analysis. Regardless, of the separation definition, the separation rate in Denmark is relatively high. The reason is that Denmark has liberal hiring and firing rules compared to the rest of Europe. Thus, the Danish labor market remains secure, because the benefit system is generous compared to other European countries.

Previous literature on long-term unemployed workers has concentrated on socio-economic environments and individual characteristics, especially individual specific effects (Machin and Manning 1999; Mincer 1991; Portugal and Addison 2000). However, the applied definitions on long-term unemployed individuals are very diverse. For example, Portugal and Addison (2000) define people with more than 8 weeks of unemployment as long-term unemployed individuals, whereas Manchin and Manning (1999) define long-term unemployment as more than 1 year of unemployment. Studies on US data find that a person's human capital seems especially important to unemployment duration and the risk of becoming long-term unemployed (Machin and Manning 1999). Additionally, the majority of the long-term unemployed in Denmark have very little or no formal education. Since, research suggests that adult education and other initiatives for improving worker's skills will help the long-term unemployed to find a job, most Danish and international political initiatives focus on these factors. Often forgotten is one vital fact. A vast majority of the unskilled never become long-term unemployed. For example, 85 percent of the unskilled employed workers from 2000 were still employed in 2001, although some of them had changed workplaces. [3] The percentage of skilled employed workers who were still employed was 90.

Furthermore, looking at unemployment periods for all workers who separated from a job in Denmark between 1995-2001, there exists differences among workplace sizes regardless of the employee's educational background (see figure 2). Thus, former workplace experience could shed new light on the risk of becoming long-term unemployed. Previous literature has already shown that workplace characteristics influence different aspects of workers' job situations, such as separation rates, job

[3] Author's calculations on the Danish register-based employer-employee panel data set from 1995 to 2001 described in section 3.

creation and destruction (Albæk and Sørensen 1998, Davis and Haltiwanger 1992), wage transitions (Abowd, Kramarz and Margolis 1999), employment mobility (Cappellari and Jenkins 2004), as well as payment schemes.

To my knowledge, no studies have looked at workplace characteristics in conjunction with long-term unemployment. The intuition is that workers coming from certain workplaces, through which they receive some kind of skills or reputation different from their formal education, have better future job possibilities and a lower risk of becoming long-term unemployed.

Past research has revealed a difference between lay-offs and quitting, where laid-off workers have high risks of becoming low paid workers in the future, and workers who quit have a high risk of becoming high paid workers in the future (Antel 1985; Hashimoto 1981). Thus seeking to establish if the last workplace has an effect on becoming long-term unemployed, it is necessary to know if job separations result from either quitting (a decision that the employee makes) or lay-offs (a decision the employer makes). Possibly workers who either have a new job offer or expect to get a new job quickly are more likely to quit. Conversely workers who have no alternative job offers could be likely candidates for lay-offs because they are burnt out, inefficient, or not up-to-date with their skills, or because they have firm specific skills that are not transferable to other workplaces.

The data makes it possible to isolate a special group of laid-off workers who are displaced. The displaced workers are usually associated with three characteristics (Fallick 1996). First, the workers are displaced because of structural changes, such as demand changes or technological developments. Second, these very changes limit the chances for the displaced workers to return to a comparable job. Third, the displaced workers are strongly attached to their former sector (e.g industry, occupation or location). This paper uses the wide definition for a displaced worker, which has been applied by Jacobson, Lalonde and Sullivan (1993) and Browning, Danø and Heinesen (2003): a displaced worker is defined as one who separates from a private workplace that annually reduces staff by more than 30 percent. In Denmark, from 1995-2001, more than 25 percent of all separations (among workers with at least one year of steady employment in private workplaces with more than 10 employees) result from

displacement.[4] These are the displaced workers I look at when analyzing the influence of workplace characteristics on the risk of becoming long-term unemployed.

The paper is set up as follows: Section 2 explains in detail why workplace characteristics can affect the risk of becoming long-term unemployed. Section 3 describes the rich employer-employee data and some descriptive results. Section 4 illustrates the multinomial logistic model of seven different exit states after displacement. Results regarding the former workplace influence on the risk of becoming long-term unemployed appear in section 5. In section 6 conclusions are drawn.

2. The risk of becoming long-term unemployed after displacement

Despite an extensive amount of literature on long-term unemployment, most literature focuses on differences in inflow and outflow rates (Machin and Manning 1999). Fewer international studies focus on individual characteristics such as age, education, family status and unobserved heterogeneity, and no studies have looked at previous workplace characteristics in conjunction with long-term unemployment.[5] In this section I first illustrate how individual characteristics and business cycles influence workers' risk of becoming long-term unemployed. Afterwards, the possible influence of workplace characteristics on the risk of becoming a long-term unemployed individual is described.

Individual characteristics and business cycles

Most researchers and politicians have pinpointed a lack of formal skills as the major reason that some workers become long-term unemployed. From the perspective of human capital theory, skilled workers should have better job-match possibilities than less skilled workers (Mincer 1991). Thus, workers without formal education should have a higher risk of becoming long-term unemployed. Addison and Portugal (1987), Portugal and Addison (2000), and Obben, Engelbrecht and Thomphson (2002) also find that unskilled workers in the US and New Zealand have a relatively high risk of becoming long-term unemployed.

[4] Author's calculations on the Danish register-based employer-employee panel data set from 1995-2001.
[5] One descriptive Danish study (Hussain and Geerdsen 1998) has looked at the correlation between workplace industries and long-term unemployment.

Many studies also show that seniors have a relatively high risk of becoming long-term unemployed (Nickell 1979; Portugal and Addison 2000).[6] An exception is the study by Obben et al. (2002), which finds no positive correlation between age and the risk of becoming long-term unemployed in New Zealand. However, suppose age is correlated with less productivity because older workers have health problems or do not have the latest skills in demand. Then the arrival rate of job offers is expected to reduce and thereby the risk of becoming long-term unemployed increases among seniors. On the other hand, age and health might not determine long-term unemployment, but rather preferences for work and search costs. For example, a person with poor health may have higher preferences for leisure than a healthy person and therefore the person searches less for a job and has a higher risk of becoming long-term unemployed.

A worker's family situation and partner's income are often suggested as important factors with respect to preferences, economic incentives and search costs for job seekers. Addison and Portugal (1987) find that singles have longer unemployment durations than couples. Danish studies also show that families consisting of single female breadwinners, having at least one child and little education, do not have any economic incentives to work (Smith 1998). Therefore, such single female parents have a high risk of becoming long-term unemployed. The economic incentive is especially absent when the replacement ratio is high. A condition, which is common to welfare states that have high unemployment insurance like in Scandinavia.[7] However, Nickell (1979) also shows that unemployment duration among individuals from the US increases when the replacement rate is high.

If employers discriminate due to gender, ethnicity or unemployment history then the arrival rate of job offers is reduced and that can increase the risk of long-term unemployment for the discriminated groups. Discrimination exists when employers select employees according to superficial characteristics such as skin colour and name, which is strictly based on imperfect information on real human capital. They use these characteristics as a proxy for a person's qualifications, despite the irrelevance

[6] Some Danish studies also indicate that age increases long-term unemployment (Arbejdsministeriet 2001; Dansk Arbejdsgiverforening 2000).
[7] The puzzling thing is that a lot of wage earners in Denmark work even though they lack economic incentives.

for the job. Addison and Portugal (1987), Portugal and Addison (2000), and Obben et al. (2002) find that people of color in the US and New Zealand have a relatively high risk of becoming long-term unemployed. Moreover, Addison and Portugal (1987) find that women have an increased risk of becoming long-term unemployed. However, in contrast to the last result Obben et al. (2002) find that unemployed women in New Zealand have a relatively low risk of long-term unemployment.

A majority of studies that analyze long-term unemployment and individual characteristics control for business cycles. Business cycles that reduce the demand for goods reduce the demand for labour. Long depression periods combined with structural changes can therefore cause people who were unemployed in the short run to become unemployed in the long run. A high regional unemployment rate is a good indicator of bad conditions on the local labour markets and reduced job possibilities. Portugal and Addison (2000) also find that long-term unemployment increases when the unemployment rate increases.

Previous studies clearly show that individual characteristics influence long-term unemployment among workers. Thus any analysis of the workplace characteristics influence on the risk of becoming long-term unemployed after displacement must take these characteristics into account.

Workplace characteristics

As we have seen, a worker's individual characteristics and business cycles influence the risk of long-term unemployment. Therefore it is expected that being an unskilled senior female worker is a disadvantage with respect to long-term unemployment. However, in reality most of the potential disadvantaged workers never become long-term unemployed. Therefore factors different from observed individual characteristics and business cycles must explain why some workers become long-term unemployed and some do not become long-term unemployed. Explanations for long-term unemployment are also found on the demand side of the labour market. The intuition is that workplace experience influences workers' future job opportunities. If the worker has obtained some kind of skills or prestige from the last workplace that is in demand in other workplaces, then the worker is expected not to become long-term unemployed after a job separation. The fact that about 50 percent of Danish wage earners participated in

some kind of job-related training (JRT) in 1998/1999 (OECD 2001) supports the idea that a great deal of skills are obtained at today's workplaces. JRT is very diverse, from computer courses to truck licensing courses and team-building courses. The volume of JRT in Denmark is also extensive compared to other OECD countries.

In the human capital framework, a worker who gains skills or prestige at a workplace increases his or her human capital and the arrival rate of job offers. The workplace experience – skills and prestige – is assumed to be different from skills obtained through the formal educational system. Furthermore, the skills come through JRT which includes courses on the job and informal tutoring while working.[8] Prestige can result from working in well-known companies, such as big concerns or companies producing brand names. Prestige can also have negative connotations, such as having worked in industries using outdated machinery and tools. JRT and prestige are assumed to be financed directly or indirectly by the employer.

In contrast to Becker's traditional division between firm specific human capital (paid by the employer) and general specific capital (paid by the employee), this study does not divide human capital. Instead, it assumes that all kinds of JRT and prestige increase the workers' human capital, thereby improving their positions with respect to other jobs. The reasoning is two-fold: First, many companies pay for general training. They presumably offer such training because they expect to benefit from it, even though it increases their employees' job opportunities elsewhere (Weatherall 2007). Acemoglu and Pischke (1999) also show that firms pay for general training when the wage structure is distorted due to labor market frictions and institutions. Second, gaining non-transferable skills through JRT or prestige that no other workplace demands is difficult to imagine. Even if the skills are not directly usable in other firms, one would expect that the experience of learning something new would enhance one's learning ability and increase one's knowledge base.

This section focuses on workplace characteristics associated with JRT or prestige advantages. Both can increase wage earners job possibilities and thereby reduce the risk of long-term unemployment. Workplaces with a lot of employees can invest in more training than those with few employees, because they achieve increasing returns to scale. The training programs at such big workplaces might also be well known and

[8] JRT in this paper includes informal tutoring, which is different from the definition in Weatherall (2007) chapter 2 "Does job-related training increase future wage and mobility?"

acknowledged by other companies. Sørensen (2000) and Weatherall (2007) find that Danish workplaces with many employees on average invest more resources in training. Although employees in small workplaces might be trained to handle many different situations, employees in big workplaces are expected to be offered more JRT. Therefore, employees from large workplaces can expect more job offers and lower risk of long-term unemployment.

A low rate of staff turnover can increase the employer's investment in training because the new skills stay at the workplace. If a spill-over effect exists, this situation could benefit the untrained employees. Even though little staff turnover can result in no new skills added at the workplace, workers from workplaces with a low rate of staff turnover are likely to get more JRT (Sørensen 2000; Weatherall 2007). Again the job offers are expected to increase and the risk of long-term unemployment is expected to be reduced.

In contrast to workplaces with many part-time workers, workplaces that have a high percentage of full-time workers have less total training costs because fewer employees need to be trained. Therefore, more training is offered, and the workers working full-time can gain both more training and very diverse training, because they have many different tasks when working full-time. Thus, coming from a workplace with a high percentage of full-time workers is an indirect advantage that reduces the risk of becoming long-term unemployed.

Workplaces may pay high wages because their employees have been trained and are productive or because they want to try to compensate employees for very demanding jobs (e.g. physical or psychological challenging job assignments). A positive spiral exists if high wages are associated with productive workplaces, because these workplaces are expected to have resources for training. Therefore high paid employees become even more productive. Sørensen (2000) and Brown (1990) find that employees who are paid higher wages also receive more training offers. Therefore, workers coming from high-paying workplaces are likely to have a lower risk of becoming long-term unemployed. Furthermore, paying high wages can signal that workplaces are productive, thereby giving them a good reputation, with a spill-over effect for the employees. At the same time, these workplaces might be productive because their employees already are trained, not because the workplace offers training.

Industries undergoing structural change need to retrain their personnel, e.g. if most Danish industries outsource unskilled production due to globalisation, one would expect unskilled workers to have problems in finding new unskilled jobs and long-term unemployment can result.[9] Although the overall negative quantitative impact of outsourcing is small, Munch (2005) finds that low-skilled workers from outsourcing Danish manufacturing firms have an increased risk of becoming unemployed. Other Danish studies show that outsourcing has a limited negative effect on the labour market apart from the textile industry (Geerdsen et al. 2004, Olsen et al. 2004).

Highly innovative industries that use high-tech equipment also need to continually retrain their employees. Sørensen (2000) finds that workplaces using advanced technology prioritise JRT. Although industries can be associated with exceptional physical or psychological demanding jobs, workers from innovative workplaces using high-tech tools are expected to constantly obtain new skills, and the industry is therefore associated with having qualified employees. Workers from these industries therefore have a low risk of long-term unemployment. Furthermore, Jones (1999) argues that in a very competitive consumer goods market, firms need extra good publicity, which they can achieve by beng socially responsible, by not firing employees, and by taking care of employees, e.g. by training offers. Employees are likely to benefit from these social initiatives.

The workforce composition can influence the training possibilities in a workplace. Even though a differentiated workforce offers the possibility of obtaining different skills, a homogeneous workforce makes training desirable for the employer because of the possibility of increasing returns to scale. In Denmark workplaces prioritise training for large groups of workers (Sørensen 2000). Workers at workplaces with homogenous workers are expected to gain new skills and therefore they have a lower risk of becoming long-term unemployed.

Finally goals, expectations and visions will likely influence employers' training decisions. The literature on human resource management and organisation theory suggests that organizational structures and processes deeply affect occupational choice, skills development and job mobility (Booth and Chatteriji 1989, Lazear 1996, Ichniowski and Shaw 2003). Organizational cultures that foster dialogue and dissent are

[9] This result might not be perfectly clear if production depends on total workforce structure or capital.

likely to increase employees' understanding of their own capacity and their need for further training. Sørensen's (2000) and Weatherall's (2007) findings support the idea that workplaces offer more training when prioritising their employees' human resources. Therefore, workers at such workplaces have better job opportunities, with less risk of becoming long-term unemployed.

Thus some workplace characteristics somewhat correspond to investment in JRT and gaining prestige. JRT and high prestige are more likely for workers from big productive workplaces that pay high wages and have low staff turnover. Furthermore, working in industries undergoing structural changes and having both a homogeneous workforce and a very reflective, innovative management could increase a worker's informal work-related skills and prestige. Workers from such workplaces have better job offer rates and have a lower risk of becoming long-term unemployed.

3. The employer-employee panel data and definitions

The empirical analysis is based on a rich linked employer-employee panel, combining workers and their workplaces in the private sector. The panel data allows one to specifically identify employment periods and unemployment periods, and to follow workers' histories from one workplace to another, as well as into unemployment or out of the labour force.

The data consists of two parts. One part is a 10 percent sample of the Danish population aged 16 years and more. This panel data from 1994 to 2002 includes very detailed information on individual characteristics, such as; age, family status, educational skills, personal income and unemployment history. Especially the information on unemployment is very precise on a daily basis. The second part is a panel on workplaces. The workplace data includes rich information on industry, workforce composition, number of employees, etc., from 1980 to 2001. No information exists on employers' goals, organisation structure and management culture. These factors are therefore not the focus of the empirical analysis. Statistics Denmark collects all the workplace characteristics annually in November. A unique identification of individuals and workplaces allows the matching of the two data sets. Additionally, the panel structure allows me to describe the group of long-term unemployed and to analyse the transition into becoming long-term unemployed. In the following I describe the

selection of workplaces, employees, and long-term unemployed, and I describe variable definitions.

Workplace and displacement

Even though 1.6 million people worked annually in the Danish private sector from 1995-2001, the analysis in this paper concentrates solely on a specific group of displaced workers who worked for at least one year at the same workplace of 10 employees or more in the private sector. This allows one to separate the laid-off workers from the workers quitting and thereby avoids the sample selection caused by the correlation between workers' separation rate and their expected job possibilities. A detailed description of the selection follows.

An assessment of 'last workplace influence' on becoming long-term unemployed calls for combining certain employment periods with certain workplaces. As mentioned in the section 1 a workplace is defined as a legally-registered unit at a specific address, but some firms in the service industry are not registered as a workplace because the production is not associated with a specific geographical address. An example is the house cleaning service in which the employer sits at home and coordinates 10 employees, all of whom work in 10 different homes. I excluded workers with no physical workplace because all workplace characteristics were missing. Excluding these workers reduced the number of observations. However, they appear to be similarly distributed by year, age and separation state to the rest of the sample. Therefore the exclusion is not a problem with respect to the empirical analysis. A few characteristics are missing for a few workplaces. However, instead of excluding such workplaces from the analysis for a few missing observations, I have substituted the missing values with the average values for workplaces within the same industry, geographical area and year. Furthermore, for workplace characteristics to make sense the analysis only examines employees from workplaces with at least 10 employees annually.

When assessing the influence of skills obtained at the former workplace on someone's risk of becoming long-term unemployed, it is necessary to look at workers who have been employed for a period. For workplace characteristics to matter, the assumption is that the workers must be employed for at least one year at the same

workplace.[10] For each employee, I calculate the employment period by using the detailed information on employment periods within the year. In the private sector, almost 75 percent of the employees have worked at least one year in one workplace (see table 1 columns 1-3).

From 1995-2001, about 80 percent of workers had worked at least a year in the same workplace with at least 10 employees in the private sector stayed in the workplace the following year. Whereas, about 20 percent of the workers separated (either quitting or are laid-off) each year from 1995-2001 (see table 1 columns 4-6).

By focusing on labour market status after displacement, I can avoid the problem differentiating between lay-offs and quitting. Although an extensive amount of literature agrees on a common verbal displacement definition, the literature uses many different empirical definitions of displaced workers. The verbal displacement definition as mentioned in section 1 consists of three parts - job loss from structural changes, limited chances of returning to a comparable job, and strong attachment to the former sector. In contrast to most US studies that use the Displaced Worker Survey, where workers above 20 years old are asked about job losses in the previous 5 years, this study uses the displacement definition of Jacobson, Lalonde and Sullivan (1993) and Browning, Danø and Heinesen (2003). A worker who separates from a workplace that reduced its workforce by a minimum of 30 percent from one year to another is defined as a displaced worker. Due to the fact that the register data on workplaces are very detailed, the yearly employment rate information is used to define displaced wage earners. Table 1 columns 7-9 show that approximately 28 percent of workers who separate from steady employment in the private sector are displaced on a yearly basis from 1995-2001. Some of the workers have been displaced more than once in the sample period. These individuals are excluded in the estimations, because of the overrepresentation of new employment. Table 2 illustrates all the workers who separate from their jobs and displays if their previous workplace's number of employees increased or decreased in the period 1995-2001. The majority of workers clearly separated from workplaces that reduced their staff by between 0 to 30 percent.

Throughout the rest of the paper "displaced workers" will refer to workers that have at least one year of employment in a private workplace with more than 10

[10] Although as it turns out, changing the employment period to six months does not change the final results significantly.

employees where the workplace has reduced the number of employees by a minimum of 30 percent from the previous year.

The displaced worker group consists of six percent of all workers that had a steady job in private workplaces with a minimum of 10 employees. The characteristics of the workplaces that the displaced workers come from differ slightly from the average workplace in the private sector with more than 10 employees (see table 3). Around 80 percent of displaced workers have worked in industries such as manufacturing, wholesale, transportation and finance. The same is true for the rest of the workers from similar workplaces. Nevertheless the transport industry and hotel and restaurant industry are more common for the displaced workers than for other workers. As expected, the displaced workers on average originate from workplaces with fewer employees and have relatively fewer full-time workers. Surprisingly, the displaced workers are from relatively high-paying workplaces (except among managers). The workforce composition does not differ between workplaces that displace their employees and those that do not.

Long-term unemployed

International studies using detailed register data for long-term unemployment periods and participation in active labour market programs for insured and non-insured workers are scarce. These studies have used one year of unemployment as a measuring point for long-term unemployed (Machin & Manning 1999). I do the same, although the definition includes different kinds of unemployment periods. This is due to the fact that the available Danish register data on insured and uninsured worker benefits for unemployed individuals, participation in active labour market programs, post-employment periods etc. is very detailed. It is possible to precisely calculate long-term unemployment periods from 1995-2001. Workers who are subsidised for reasons of unemployment, participation in active labour market programs or educational leave can receive monetary benefits only if they make themselves available to the Danish labour market. Thus, the long-term unemployed have to conduct a minimum amount of job-search. Moreover, unemployment should be their main problem, in contrast to people receiving welfare benefits for personal problems, sickness, etc. Consequently, as previously mentioned, this paper defines the long-term unemployed as people receiving

subsidies because of unemployment, participation in active labour market programs or educational leave for more than one year.

The very precise registration of subsidies allows us to observe when a person receives unemployment insurance for three months then has a two-week holiday break and then resumes receiving unemployment insurance for another nine months. For this paper's analysis it does not make sense to accept a two-week holiday break as the end of an unemployment spell. Thus this analysis does not take such small breaks into account. Changing the length of the breaks from 14 days to 6 weeks does not change the amount of long-term unemployed significantly. Due to the fact that mandatory holidays in Denmark are 5 weeks in the observation period this paper ignores breaks less than 6 weeks (i.e. one week more than the mandatory holiday laws to make sure that all holidays are included). Thus, over an unemployment period, an individual can have small breaks and still be considered long-term unemployed. Given this definition and given these exclusions, 3,4 percent of workers that had worked at least one year became long-term unemployed after being displaced from a private Danish workplace with at least 10 employees from 1996 to 2001 (see table 1 columns 10-12).

The percentage of displaced workers becoming long-term unemployed decreased from 1996 to 2001, which is a trend similar for all long-term unemployed in Denmark. However, the group of previously displaced workers who become long-term unemployed distinguish themselves from the rest of the long-term unemployed. Table 4 shows that the percentage of women among all Danish long-term unemployed is higher than among the previously displaced workers who become long-term unemployed. Additionally, the percentage of people over 50 and couples without children is higher among the long-term unemployed, who were displaced, than among all long-term unemployed. The displaced also have a much higher previous income, even though their educational level is lower on average than that of other long-term unemployed. This is not puzzling because the displaced workers have by definition worked and had a wage income in the previous year, while a percentage of the rest of the long-term unemployed has not. Finally, the regional unemployment rate is on average lower in regions where previous displaced workers who become long-term unemployed live, than in regions where the most long-term unemployed live.

Transition states and sample period

In this paper the post-employment labour market states constructed from different register sources are defined in the following way for each year: If a person is identified as a pensioner, as out of the labour force or as under education by the end of a year, then the person receives that status for the whole year. With respect to unemployment, participation in active labour market programs and educational leave, the long-term unemployed and short-term unemployed is defined by information on subsidised periods. The employed are defined as not being in any of these groups and as having at least one observed employment period.

The most precise unemployment information is available from 1995. Therefore, the analysis will focus on transitions into long-term unemployment from 1995 through to 2001. Lack of workplace information after 2001 is the reason for restricting the period to 2001.

Variable definitions

The goal is to assess, taking individual specifics and business cycles into account, whether displaced workers from certain workplaces have a higher risk of becoming long-term unemployed than others. The hypotheses concerning the former workplace characteristics' influence on the risk of becoming long-term unemployed from section 2 are implemented empirically as described in Table 5. The table shows that workplace observables include industry, number of employees, percentage of different occupational groups, average wage for white-collar workers, the average staff turnover, and workplace reason for closing. Individual characteristics and business cycles include education, occupation, tenure, age, gender family, ethnicity, income, regional unemployment rates, and year dummies. Although, variables such as workplace culture, leadership, production methods or individual preferences are not observed one ought to realise that such characteristics can influence decisions about training, separation and future job offers.

The simple descriptive statistics in Table 6 indicate that the average displaced worker who becomes long-term unemployed is different from the displaced worker who becomes reemployed. Among the displaced long-term unemployed

individuals, a high percentage had previously been employed in the manufacturing industry and in workplaces with low percentages of high-skilled wage earners. Furthermore, these workplaces, on average, paid less than comparable industries and had low productivity. In contrast, the displaced workers who became reemployed originate more from manufacturing, wholesale, financial and transportation industries with higher percentages of white collar workers.

Differences between the displaced long-term unemployed individuals and the displaced reemployed individuals also occur in individual characteristics. The reemployed included more men, remarkably fewer seniors, and more employees with higher education and managerial experience. In contrast, the long-term unemployed on average had more tenure and a higher percentage of immigrants and couples without children. Normally, high tenure is associated with many direct and indirect skills. However, having a long tenure at the same workplace can also mean that the person is burned out and not interested in mobility (i.e. comparable to the age effect). These simple descriptive statistics show differences between the displaced workers who obtain new employment and those who become long-term unemployed.

4. Empirical model on workplace effects influencing long-term unemployment

The data set on employers and employees makes an analysis of the correlation between previous workplace characteristics and the risk of becoming long-term unemployed after displacement possible. As mentioned earlier, the transition from being a steady employee, to becoming displaced and later becoming long-term unemployed instead of finding a new job is of interest. Figure 4 illustrates the idea visually.

This study models the employment flow after displacement as an outcome of a probability model with seven possible states. The seven states are new employed, self-employed, short-term unemployed (10-99 percent subsidised within a year), long-term unemployed (subsidised more than one year), pension/out of labour force, and unknown. Due to the fact that workplace and individual characteristics are annual, except for the unemployment and employment periods, a multinomial logit model is the best choice. The model's outcome probabilities are defined as follows:

$$(1) \qquad P(Y = j \mid X) = \frac{\exp(X\beta_j)}{1 + \sum_{h=1}^{J} \exp(X\beta_j)} \qquad j = 1,...,J$$

Where Y is the outcome variable that can be equal to J different outcome states (new-employment, self-employment, long-term unemployment, short-term unemployment, education, out of labor force, other) and X consists of a set of observable covariates (e.g. age, education, family background, workplace industry, workplace number of employees, workplace labour force composition). The unknown parameter vector is β. Although the assumption about the independence of irrelevant alternatives (IIA) is a constraint, section 5.3 shows that this constraint is not a big problem for this study. Thus the risk of becoming long-term unemployed is estimated with respect to observables. The multinomial logit model including individual characteristics, local business cycles and workplace characteristics is called the `extended model´. However, for comparative reasons, a multinomial logit model including only individual characteristics and local business cycles is also estimated. The latter is named the basic model. Individual and workplace specific unobservable characteristics are not taken into account because there are not enough workers with multiple displacement histories and there are not enough workers in each workplace.

5. Findings: last workplace characteristics influence long-term unemployment

The point is to learn whether workplace characteristics influence a wage earner's risk of becoming long-term unemployed. Where long-term unemployment is the unfavourable state and a new job is the favourable state. Consequently, the discussion of the results in Table 7 focus on the difference in becoming long-term unemployed and getting a new job after displacement.

First, I compare the results for individual and local business cycle variables in the extended model to previous studies. Second, I discuss the results for the workplace characteristics. Third, I compare the extended model to the basic model showing that workplace characteristics add an extra dimension to the risk of becoming long-term unemployed.

Individual characteristics and regional business cycles

The results from the extended model appear in Table 7.[11] As in previous studies, the results show that workers' individual characteristics and the regional economic situation influence the risk of becoming long-term unemployed compared to finding a new job after displacement.

According to the empirical findings, having no formal education compared to a vocational education or a further education increases a person's risk of becoming long-term unemployed after displacement. This result corresponds with the results of Addison and Portugal (1987), Portugal and Addison (2000) and Obben et al. (2002), who find that unskilled workers have an increased risk of having long unemployment durations. Indeed, skills in form of formal education appear to matter for displaced workers' future job opportunities. However the occupational groups, which also indicate a worker's human capital, are only significant at a 20 percent level. Having worked as an unskilled worker, as opposed to a white-collar worker increases the risk of becoming long-term unemployed. Even though it is plausible that there is a combined effect of formal education and occupational group, I exclude the interaction term from the model because of insignificance across all exit states.

The high risk of becoming long-term unemployed found among seniors correlates with findings by Nickell (1979) and Portugal and Addison (2000). That productivity or preferences for work decrease with age, and thereby reduce the likelihood of starting new employment after displacement.

The results also show a negative correlation between the risk of becoming long-term unemployed and being a parent living in a couple and previously having had a high income. These results somewhat confirm the findings of Addison and Portugal (1987), indicating that economic incentives and family status influence job decisions after displacement.

Moreover, the results from the extended model show that being either a woman or an immigrant increases the risk of becoming long-term unemployed. Again, these findings correspond to Addison and Portugal (1987), Portugal and Addison (2000) and Oben et al. (2002) with exception of the latter that finds women have less risk of becoming long-term unemployed.

[11] All the variables included in the final model are significant between at least two exit states

Besides the good correspondence between the results of individual characteristics in the extended model and other studies, the local unemployment rate has the expected counter cyclical effect on becoming long-term unemployed.

Despite the differences among studies in the definition of "long-term unemployment" the significance of the individual characteristics and the regional business cycles remain similar to previous findings in Denmark and in the US.

Workplace characteristics

The central result of the extended model is that many workplace characteristics have significant effects on the risk of becoming long-term unemployed after displacement. Moreover, the likelihood ratio test for all workplace characteristics equalling zero is simultaneously rejected.[12] In other words, when the likelihood ratio of the extended model is compared to the likelihood ratio of the basic model (in table 8) the extended model is preferred.

As expected being displaced from a workplace with few employees increase the risk of becoming long-term unemployed compared to entering a new job. The disadvantage for small workplaces could result either from few skills, ` outdated´ skills, or unknown skills. However, Sørensen's (2000) and Weatherall's (2007) findings on small companies investing relatively few resources in training indicate that the risk of long-term unemployment is due to too little training at small workplaces.

Instead of showing the expected positive correlation between low rates of staff turnover and getting a new job, the results indicate a negative correlation. Therefore, coming from a workplace with high staff turnover reduces the risk of becoming long-term unemployed after displacement. Apparently workers have to obtain new skills or develop new abilities from working with new colleagues and therefore reduce their risk of becoming long-term unemployed.

Workers from workplaces with a high percentage of full-time workers do not have a significantly better chance of becoming reemployed after displacement. Apparently the employer's investment in training is not positively correlated with the amount of full-time wage earners involved in each year.

[12] Likelihood ratio test value 2(log likelihood of the extended model – log likelihood of basic model) is distributed as the $\chi 2$ – distribution with 114 degrees of freedom => $\chi 2$ (114) = 863.674 , Prob> $\chi 2$ = 0.000.

The group of white-collar colleagues with and without managerial obligations at the last workplace is important for the risk of becoming long-term unemployed. A worker from a workplace with a low percentage of white-collar colleagues who are relatively well paid compared to other white-collar workers in the same industry and geographical area have a high risk of becoming long-term unemployed after displacement. A higher percentage of white-collar colleagues evidently increase the possibility of training, thereby increasing human capital and increasing the arrival rate of job offers for workers displaced from such workplaces. On the other hand, the negative influence of high wages among white-collar workers indicates that the high wages that white-collar workers receive mainly are due to very demanding job assignments not to improved skills or productivity from training. Nevertheless, previous interactions with white-collar workers are important for the risk of becoming long-term unemployed.

Workers coming from manufacturing industries clearly have a high risk of becoming long-term unemployed as opposed to finding a new job. Due to the fact that the manufacturing industry in Denmark has outsourced quite an amount of unskilled jobs over the last decade, one might expect that such industries, for survival purpose, need to train their employees. This increases the industries' employees' human capital and therefore the employees have job possibilities. On the other hand, one might expect that workers from the manufacturing industry have outdated skills because their work tasks have been outsourced. This reduces job opportunities after displacement. Furthermore, industries that are very innovative and use high-tech equipment are expected to invest in training because the working tasks in these industries change constantly. Specific results confirm this expectation. On the other hand, regardless of the reason why a workplace closes (e.g. outsourced, foreign take over or absorption in the sister office) the closure does not influence the risk of long-term unemployment for the displaced workers.

To sum up, the displaced workers have a relatively high risk of becoming long-term unemployed if they have previously worked in small workplaces in the manufacturing industry. Furthermore, being displaced from workplaces having relatively low percentages of white-collar workers and a low turnover rate of employees increases the risk of becoming long-term unemployed. Finally, being displaced from

workplaces paying white-collar workers relatively high wages increases the risk of long-term unemployment. Thus, the composition and level of payment for white-collar workers in the previous workplace significantly affects the long-term unemployment risk.

A comparison of the findings from the extended model in this paper to the findings on workplace investment in JRT result in a clear common pattern. Some of the JRT findings such as Sørensen (2000), Weatherall (2007) and Brown (1990) find that the investment in JRT is prioritised in large workplaces that use advanced technology and give employees' human resources a high priority. Additionally, JRT is to a higher extent offered to large homogeneous groups of employees at the workplace especially if they already have some formal skills (e.g. high percentage of white-collar workers). Hence the workplace characteristics that reduce a worker's risk of becoming long-term unemployed are similar to the workplace characteristics that are positively correlated with workplaces' investment in JRT (Sørensen 2000; Brown 1990; Weatherall 2007).

Test of the extended model and prediction

In the previous section, the findings rely on two assumptions. The first assumption is the independence of irrelevant alternatives (IIA). Suppose, for example, that one of the transition states is removed from the model; the relative probability between becoming long-term unemployed and getting a new job after displacement should not change if the IIA is fulfilled. The second assumption is that the extended model - including individual characteristics, local business cycles and workplace characteristics - describes the transition into long-term unemployment better than the basic model (not including workplace characteristics). Fortunately, the following tests and predictions show that assuming IIA and the superiority of the extended model is reasonable.

Under the IIA assumption, no systematic change in the coefficient is expected if, for example the transition state `self-employment´ is excluded from the model. Therefore, the extended model is re-estimated excluding the self-employment outcome and afterwards a Hausman-Mcfadden test against the full extended model is performed. The test statistics under the alternative hypothesis of IIA violation is a test of systematic differences in the coefficients for all transition states except self-

employment.[13] Table 9 shows that four out of five tests can not reject the IIA assumption.

Even though the extended model seems to fulfil the IIA assumption, it might be the case that some outcome categories should be combined (e.g. long-term and short-term unemployed as one exit state). Therefore I test if any of the outcome categories can be combined by the Wald statistic.[14]This test is done for all outcome categories in pairs and the test results are illustrated in Table 10. The results of the Wald tests clearly show that the outcome categories should not be collapsed.

There are at least three reasons why the extended model is good at modelling displaced workers risk of becoming long-term unemployed. First, the estimation results in table 7 shows that the multinomial logit model does not have problems in finding structure and that most of the coefficients are significantly different from the base category (i.e. new employment). Some coefficients are not significantly different from zero or the base category which to a certain extent is due to the sample size (i.e. the displaced long-term unemployed is relatively small in a 10 percent sample of the Danish population). Second, the Hausman-McFadden test of IIA in Table 9 shows that the assumption of IIA is weakly accepted, which also supports the structure of the extended model. Third, the Wald tests in Table 10 illustrated that none of the outcome categories should be collapsed, which once more supports the structure of the extended model.

In section 5.1 the likelihood ratio test was in favour of the extended model versus the basic model. By looking at the average prediction in a sample - goodness of fit - it is possible to see if the estimated model can distinguish between different exit states after displacement. Tables 11 and 12 illustrate the goodness of fit for the extended model as well as the basic model. The tables show the average predicted exit risk with respect to the actual exit state. Not surprisingly do the average predicted values to a certain extent correspond to the sample distribution of different outcomes regardless of which model results one examines. Notable is that the diagonal (except for the short-term unemployed) are the highest average predicted values, which indicate that the

[13] The test is distributed $\chi 2$ and computed as: $H=(\beta_{no\_selfemploed} - \beta_{full\_model})'(V_{no\_selfemploed} - V_{full\_model})^{-1}(\beta_{no\_selfemploed} - \beta_{full\_model})$.

[14] The test is $\chi 2$ distributed and computed as: $W= (\beta_{short\text{-}term\ unemployed} - \beta_{long\text{-}term\ unemployed})^2/var(\beta_{short\text{-}term\ unemployed} - \beta_{loong\text{-}term\ unemployed})$.

model can separate between the different outcomes. However, the goodness of fit is no guidance for choosing between the extended model and the basic model because no clear model differences occur.

Another way to compare the extended model with the basic model is to show the predictive power of the two models. A model predicts well if it has few type I and type II errors. A type I error is when the model fails to predict a displaced worker to be a long-term unemployed individual if the worker is a long-term unemployed individual. A type II error is when the model predicts a displaced worker to be a long-term unemployed individual although he or she is not. For simplicity and interpretational comfort the predictions in this paper concerns long-term unemployed workers compared to the rest of the workers' exit states. Consequently this study assumes a cut off point that matches the distribution of long-term unemployed workers in the sample, which is 3,43 percent out of 22.826 individuals. In other words the individuals among the 3,43 percent highest predicted values are expected to become long-term unemployed.[15] Tables 13 and 14 illustrate the predictive results of both the extended and the basic model. Even though it is clear that both models suffer from type I and type II errors, the results show that the extended model is better in predicting displaced workers to become long-term unemployed individuals than the basic model. Suppose the cut of point is different, for example 10 percent, then the correctly predicted long-term unemployed will increase. However, changing the cut off point is combined with a trade off because the proportion of correctly predicted non-long-term unemployed individuals will decline.

Receiver Operating Cost (ROC) curves is another measure of predictability. By using ROC curves the problem of finding the correct cut off point is overcome because the curve illustrates the correctly predicted outcomes for all cut off points. Figure 4 illustrates the idea of the ROC-curve. On the y-axis is the fraction of correctly predicted long-term unemployed and on the x-axis is the corresponding fraction of incorrect predicted long-term unemployed. A high fraction of correctly predicted long-term unemployed combined simultaneously with a low fraction of incorrectly predicted long-term unemployed is best. Therefore the best models should be very close to the line called perfect fit. A bad model has a ROC- curve close to the

[15] Assuming the outcome is 1 for becoming long-term unemployed and 0 for non-long-term unemployed.

diagonal. For comparing different models the Accuracy Ratio (AR) of the ROC-curve is applied. AR is calculated as the ratio of the area α below the ROC-curve and the diagonal and the area β below the perfect fit line and the diagonal. A high AR indicates a well predicted model.

Figures 5 and 6 illustrate the ROC-curves for the basic model and the extended model. The ROC-curves consist of clusters of observations because the class variables are continuous and there are 22.826 observations. Therefore it is difficult to see if the extended model is a better predictor than the basic model. Instead I calculate the area under the ROC-curve for both models (see table 15). Due to the very uneven distribution of long-term unemployed and non-long-term unemployed, neither the extended model nor the basic model predicts perfectly. However, the extended model including workplace characteristics has an ROC-area of 0.78, which is 0.02 bigger than the ROC-area for the basic model that only includes individual characteristics and local business cycles. The difference is statistically significant.

All tests on the extended model versus the basic model are in favour of the extended model. Thus workplace characteristics are important to account for when evaluating displaced workers risk of becoming long-term unemployed.

Discussion

The results of this study clearly show that individual characteristics and workplace characteristics influence the risk of becoming long-term unemployed after displacement. A concern in all empirical studies is whether or not the observable characteristics actually influence the risk of becoming long-term unemployed because it is possible that the observables actually are a cover up for some other important factors.

Previous literature has shown that the job separation decisions are very much correlated with workers' future job opportunities. Thus this study has taken the worker specific effect concerning workers risk of separating from a workplace into account by just looking at the displaced workers.

In this analysis one could also worry about other worker specific effects and workplace specific effects that are not accounted for in the estimation. For example it is possible that initial ability among workers determines if a worker receives training and prestige or certain organizational structures and workplace philosophies that

determine if workplaces offer training. On the other hand, due to the rich Danish data, a lot of potential factors that could influence the risk of ending up in long-term unemployment are taken into account. Worries about the variation due to workers and workplaces specific effects are left for future research.

One might argue that reemployment after displacement might not be the ultimate success criteria for a displaced worker. Displacement can cause future wage reduction, as well as physical and psychological costs of changing jobs. These factors, despite being important are not examined in this paper.

For many years Danish policy makers have had the impression that certain population groups with certain individual characteristics (e.g. no educational skills, seniors, and immigrants) have a higher risk of becoming long-term unemployed than other population groups. To prevent long-term unemployment authorities have encouraged all the unemployed with no education or short education to participate in new education either through regular studies or active labor market programs. Furthermore, at the end of the 1990's, Danish policy makers took initiatives to focus on JRT at workplaces by subsidizing JRT initiatives, but without focusing on certain workplaces or industries.

Proof that the extended model is superior to the basic model should give a new source of inspiration to prevent workers from ending up in long-term unemployment. Thus more political focus should be on training received at certain workplaces. The analysis can inspire new labour market initiatives that focus on work conditions for people with short periods of education in certain industries with certain characteristics instead of active labour market programs for workers already unemployed, which is currently the case.

6. Conclusion

This paper has two main conclusions based on the very rich Danish register-based panel data analysis. First, the findings confirm results in previous literature that show individual characteristics can influence the risk of becoming long-term unemployed. Especially being older, a woman, an immigrant, having no education or family increase a displaced workers risk of becoming long-term unemployed.

Second, this analysis contributes to the literature by arguing that former workplace influence transitions into long-term unemployment after displacement. The importance of the last workplace could be due to skills gained through JRT at the workplace or due to prestige from working in a well recognised workplace. The results specifically show that being displaced from small manufacturing workplaces with low shares of well paid skilled employees and a low turnover rate is a disadvantage and increases the risk of becoming long-term unemployed.

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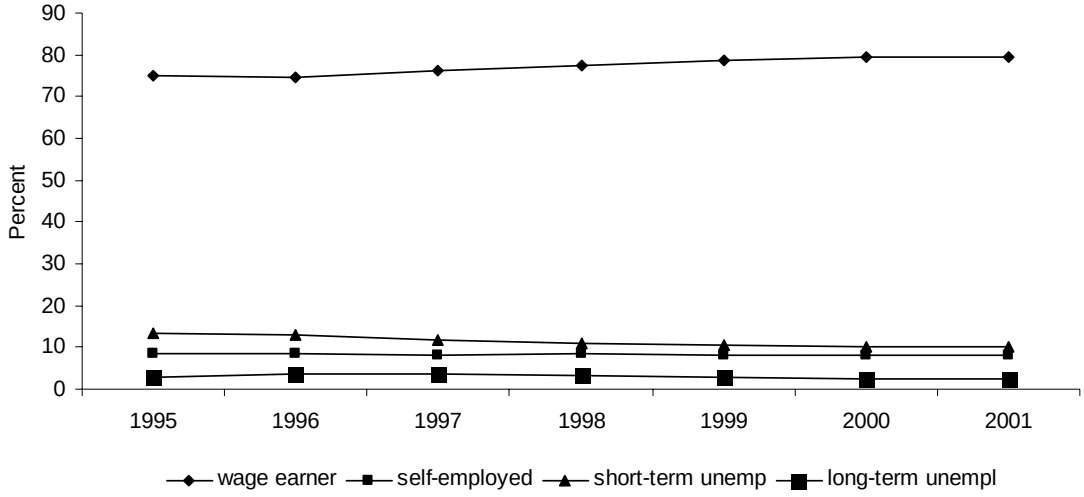
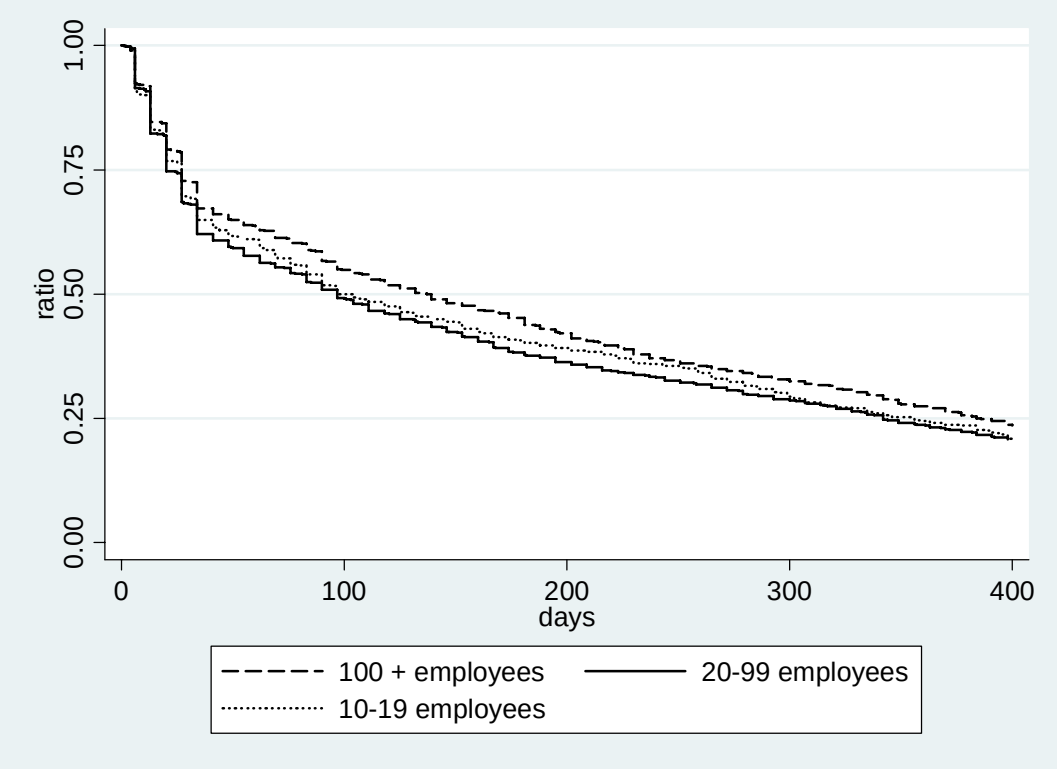
Figures

Figure 1. The Danish workforce more than 16 years of age, 1995-2001



Source: Danish register-based employer-employee panel data set from 1995 to 2001

Figure 2. The fraction of male workers with no education staying unemployed after separating from a workplace with at least 10 employees in the private sector, 1995-2001.



Source: Danish register-based employer-employee panel data set from 1995 to 2001.

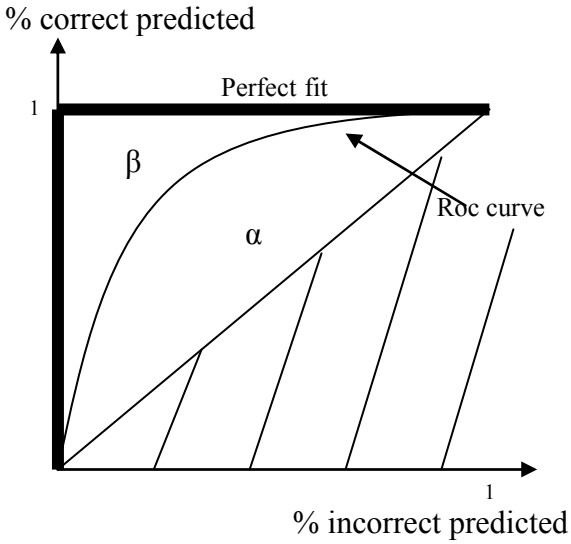
The survivor function –staying unemployed- is calculated over the full sample and evaluated at indicated days. By using the Wilcoxon (Breslow) test I test if coming from workplaces with different amounts of employees change the survivor function. The test statistics is compared with a $\chi 2$ – distribution with 2 degrees of freedom (chi2(2)=11.79 Pr>chi2=0,0027). The test rejects that the survivor function for different sized workplaces are equal.

Figure 3. The transition possibilities after being displaced

Employment[1]	⇒	Displaced[2]	⇒ New wage earner employment Self-employed Short-term unemployed Long-term unemployed [3] Education Pension/Out of labour force Unknown

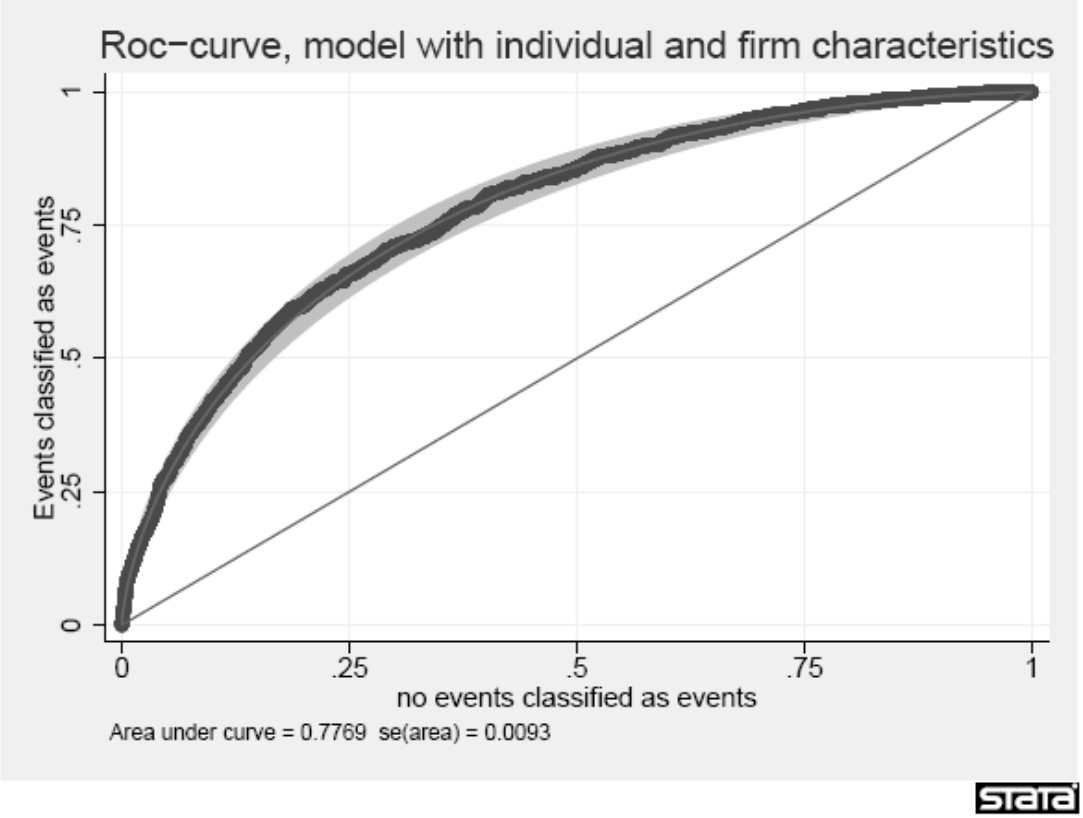
(1) Employed at least one year in a private workplace with at least 10 employees
(2) Number of employees reduced at least 30 percent from the previous year
(3) At least one year on benefits, due to unemployment, activation or educational leave

Figure 4. Illustrating the ROC-curve idea



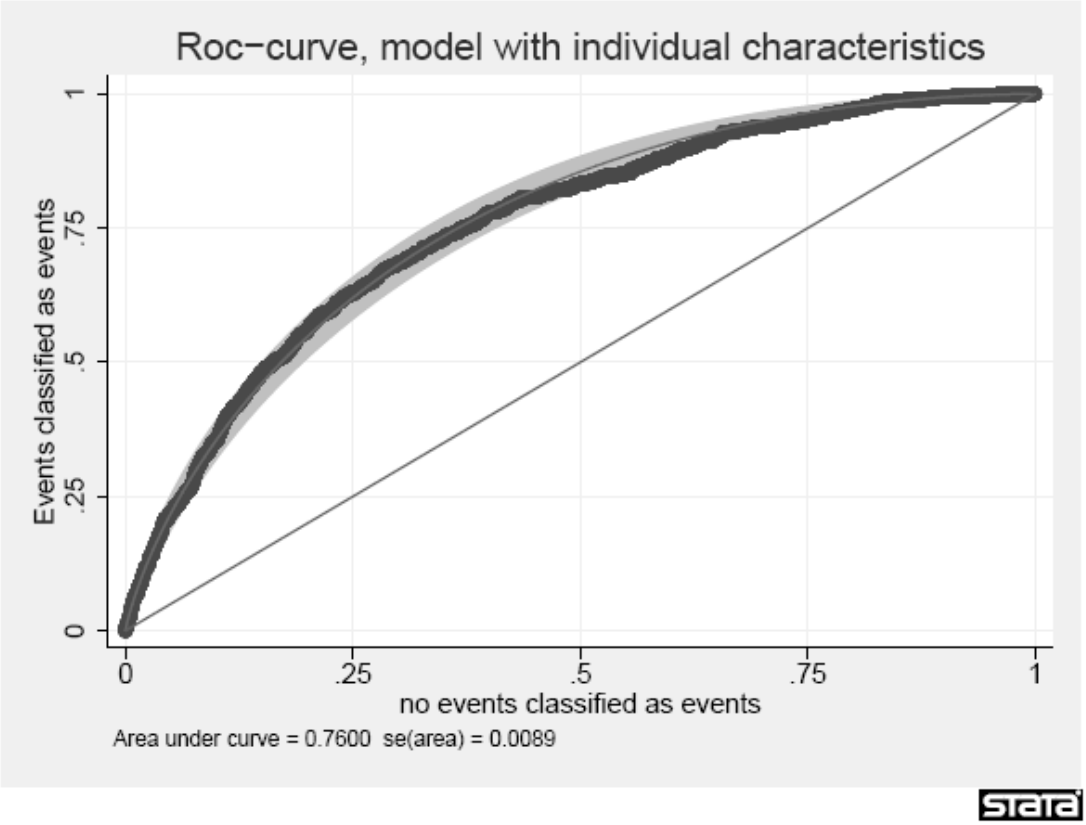
Note: The shade area is not included in the calculation of Accuracy Ratio

Figure 5. The ROC curve for the extended model



Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Figure 6. The ROC curve for the basic model



Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Tables

Table 1. Employees in the private sector with at least 10 employees, yearly averages in the period 1995-2001.

T			T -> T+1						T+1		
Employed			Separations			Firm development			Displaced once		
	Pct.	#		Pct.	#		Pct.	#		Pct.	#
Steady employment	74	544056	Separates	20	108632	Displaced	28	30779	New employ.	81,6	18631
									Self-employ.	1,3	283
									Unempl.	4,3	990
									Long-term unempl.	3,4	782
									Education	1,1	238
									Out of labour force	6,9	1576
									Without category	1,4	326
									Displaced twice	-	7953
						Other	72	77853	.	-	77853
			Stay	81	436024		-	436024		-	436024
Other employed	26	187633		-	187633		-	187633		-	187633
Total	100	732289		100	732289		100	732289		100	732289

Note: At time T I select the group of workers from private workplaces with at least 10 employees, who have been working at least one year at the workplace. If the worker is displaced from one year to the next (T-> T+1) he or she stays in the sample. The workers status at time T+1 determines the categorisation of the exit state. The final displacement sample is reduced because of exclusion of workers displaced more than ones.

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 2. Yearly percentage change in number of employees at workplaces, where workers separated from, 1995-2001.

Percentage change in numbers of employees from year to year.	Percent
Increase 30+ pct	4,52
Increase 0-30 pct	30,8
Decrease 0-30 pct	36,34
Decrease 30+ pct	13,08
Closed workplace	6,74
No employees information in the following year	8,52
Total	100

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 3. All workplaces in the private sector with at least 10 employees, 1995-2001

Variables		Private workplaces with more than 10 employees		Private workplaces with more than 10 employees, where they have displaced wage earners	
# employees	10-19 employees	17,89		21,94	
	20-99 employees	40,42		40,94	
	100-499 employees	41,69		37,11	
	Total pct.	**100**		**100**	
Industry	Hotel & restaurant	2,47		5,15	
	Manufacturing etc.	35,78		29,02	
	Electricity	0,93		1,00	
	Construction	7,53		8,24	
	Wholesale	20,09		18,94	
	Agriculture etc.	0,90		0,89	
	Transport	7,80		11,80	
	Finance	15,40		17,94	
	Service (& Int. Org)	9,10		7,02	
	Total pct.	**100**		**100**	
Reason for closing	Workplace internal absorbed	0,44		3,13	
	Workplace external absorbed	0,90		5,87	
	Other	98,66		91,00	
	Total pct.	**100**		**100**	
		Mean	**Std. dev.**	**Mean**	**Std. dev.**
Productivity	Productivity	0,654	0,247	0,610	0,254
Workforce composition	Share of unskilled workers	12,174	16,358	12,567	17,378
	Share of skilled workers	46,233	27,942	42,782	27,787
	Share of white-collar workers	17,126	17,410	18,053	19,082
	Share of managerial staff	14,745	17,147	14,139	17,160
Wage	Workplace average unskilled wage difference	-0,777	32,104	2,163	30,502
	Workplace average skilled wage difference	0,088	29,712	4,115	33,256
	Workplace average white-collar wage difference	-1,185	42,732	2,788	43,766
	Workplace average managerial wage difference	0,563	77,978	0,375	75,248
Reemployed	Reemployment rate	73,839	16,924	67,183	22,460

Note: The two groups of workplaces are tested significantly different, except with respect to "workplace average managerial wage difference".

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 4. All long-term unemployed and the displaced long-term unemployed coming from steady employment in private workplaces with at least 10 employees

Variables		All long-term unemployed		"displaced" long-term unemployed	
Individual char.	Males	38,16		51,23	
	Females	61,84		48,77	
	Total pct.	**100**		**100**	
	<31 years	16,40		15,59	
	31-50 years	48,35		45,02	
	50+ years	35,25		39,39	
	Total pct.	**100**		**100**	
	Single	29,36		27,43	
	Single with child	6,94		3,40	
	Couples	36,54		45,25	
	Couples with children	27,16		23,92	
	Total pct.	**100**		**100**	
	Basic edu.	44,15		46,42	
	Vocational edu.	38,18		40,91	
	Further edu.	17,67		12,66	
	Total pct.	**100**		**100**	
	Danes	89,04		89,45	
	Immigrants	10,56		10,55	
	New Danes	0,40		0,00	
	Total pct.	**100**		**100**	
		Mean	**Std. dev**	**Mean**	**Std. dev.**
	A-income total	147168,2	31303,45	226947,3	79321,75
	Tenure	5,403	3,584	5,634	5,230

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 5. Controls and workplace characteristics implemented empirically

Characteristics from section 2.2.	**Variables in the data**
Individual characteristics and business cycles	
Formal skills	Basic education (reference group) Vocational training Further education
Occupation	Unskilled worker Skilled worker White-collar worker Managerial worker
Tenure	Tenure at workplace (years)
Seniority	Age 15-30 (reference group) Age 31-50 Age 51+
Family Status	Single (reference group) Single parent Couple Couple and parent
Gender Ethnicity	Man (reference group) Woman Dane (reference group) Immigrant
Business cycles	Regional unemployment rate (Amt) Year dummies
Former workplace characteristics	
Many employees	Number of employees 100 + (reference group) Number of employees 20-99 Number of employees 10-19
A low rate of staff turnover	Pct. of the employees from the year before that remained
High ratio of full-time wage earners	Production in man-years devided by number of employees
High wage	The difference between the average wage given to white-collar employees in the workplace and the average wage given to white-collar employees in the industry
Industries	Hotel & Restaurant (reference group) Manufacturing etc. Electricity and gas Construction Wholesale Agricultural etc. Transport Finance Service International Organizations
Closure	Firm bought internal, externale, other or missing

Source: Weatherall (2007)

Table 6: Descriptive statistics on displaced workers, 1995-2001.

Variables		Reemployed		Long-term unemployed		χ2-test equality
Individual char.	Males	65,95		50,77		-
	Females	34,05		49,23		-
	Total pct.	**100**		**100**		reject
	<31 years	33,98		15,47		-
	31-50 years	50,51		46,68		-
	50+ years	15,51		37,85		-
	Total pct.	**100**		**100**		reject
	Single	29,98		27,62		-
	Single with child	2,40		3,58		-
	Couples	31,76		44,12		-
	Couples with children	35,86		24,68		-
	Total pct.	**100**		**100**		reject
	Basic edu.	33,36		46,68		-
	Vocational edu.	45,62		40,41		-
	Further edu.	21,02		12,92		-
	Total pct.	**100**		**100**		reject
	W-E unskilled	20,69		26,09		-
	W-E skilled	43,57		52,69		-
	W-E white collar	22,23		14,07		-
	W-E managerial	13,51		7,16		-
	Total pct.	**100**		**100**		reject
	Danes	96,08		89,00		-
	Immigrants	3,92		11,00		-
	Total pct.	**100**		**100**		Reject
		Mean	**Std. dev**	**Mean**	**Std. dev.**	**T-test equality**
	A-income total	271638,2	149884	224405,3	78745,63	reject
	Tenure	4,675	4,578	5,939	5,336	reject
	Regional unemployment	5,874	4,062	6,852	3,991	Reject
Variables						**X2-test equality**
Workplace char.	10-19 employees	20,99		23,27		-
	20-99 employees	41,40		38,49		-
	100-499 employees	37,61		38,24		-
	Total pct.	**100**		**100**		Accept
	Hotel & restaurant	4,92		4,73		-
	Manufacturing etc.	27,07		50,90		-
	Electricity	1,02		1,41		-
	Construction	8,38		3,84		-
	Wholesale	19,61		15,09		-
	Agriculture etc.	0,79		0,26		-
	Transport	12,41		7,42		-
	Finance	19,18		9,85		-
	Service (int. org)	6,61		6,52		-
	Total pct.	**100**		**100**		reject
	Workplace internal absorbed	3,17		3,07		-
	Workplace external absorbed	5,68		6,14		-
	Other	91,15		90,7		-
	Total pct.	**100**		**100**		accept
		Mean	**Std. dev.**	**Mean**	**Std. dev.**	**T-test equality**
	Ratio of full-time workers	0,619	0,220	0,612	0,225	Reject
	Share of unskilled workers	12,157	16,510	15,996	19,564	Reject
	Share of skilled workers	42,116	27,673	47,207	27,997	Reject
	Share of white-collar workers	29,120	19,610	13,887	15,280	Reject
	Share of managerial staff	14,743	17,518	10,782	14,015	Reject
	Workplace average white-collar wage difference	3,661	44,059	0,207	43,349	Reject
	Reemployment rate	67,497	22,348	71,210	20,653	reject

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 7: The extended model. New employment is reference group, 1995-2001.

	Long-term u.	Std. Err.		Short-term u.	Std. Err.		Self-employed	Std. Err.	
Man	ref.			ref.			Ref.		
Woman	0,6292017	0,0855581	***	0,3625662	0,0768522	***	-0,9642466	0,1706512	***
Age 15-30	ref.			ref.			Ref.		
Age 31-50	1,286488	0,124864	***	0,3911996	0,0907467	***	0,5306753	0,1673221	***
Age 51+	2,143585	0,1343712	***	0,7777636	0,1117496	***	0,4471771	0,2267433	**
Single	ref.			ref.			Ref.		
Single parent	0,140215	0,2195588		0,1174947	0,202787		0,6419012	0,3926778	*
Couple	-0,0888381	0,0990021		-0,0649174	0,088694		0,0899581	0,1741445	
Couple parent	-0,4503722	0,1127221	***	-0,2442556	0,0933938	***	0,1418598	0,1679637	
Basic edu.	ref.			ref.			Ref.		
Vocational edu.	-0,2188396	0,085368	***	-0,1185249	0,0750354		-0,0279949	0,1454467	
Further edu.	-0,2214746	0,1328986	*	-0,2443468	0,1231917	**	-0,229115	0,197332	
W.E. unskil.	ref.			ref.			Ref.		
W.E. skilled	0,1145267	0,1105222		0,1126629	0,0962736		-0,427517	0,185832	**
W.E. whi.col.	-0,1962046	0,1438416		-0,1708068	0,1333331		-0,1207542	0,2262666	
W.E. manager	0,2092589	0,1886425		-0,2303683	0,1787771		0,5555038	0,2299703	**
Tenure	0,0017108	0,0081465		-0,0173062	0,0083108	**	0,0102872	0,0156448	
Dane	ref.			ref.			Ref.		
Immigrant	1,128358	0,1307627	***	0,6952556	0,1292238	***	0,2250376	0,2702676	
Income(A)	-4,43E-06	5,83E-07	***	-1,82E-06	4,51E-07	***	6,14E-07	2,57E-07	**
Regional UI	0,1335611	0,0229076	***	0,0976216	0,0217294	***	0,0408993	0,0385823	
1995	ref.			ref.			Ref.		
1996	-0,4202642	0,1594369	***	-0,1559314	0,1414704		0,197608	0,2537448	
1997	-0,2418766	0,1652348		-0,0901825	0,1498598		0,2673739	0,2634232	
1998	-0,0773933	0,1811726		0,0451234	0,1612583		0,2514397	0,2896989	
1999	-0,1412385	0,1994819		0,0361004	0,1770505		-0,0042924	0,3213028	
2000	0,1801801	0,2981863		0,6419876	0,2672153	**	0,2468671	0,4832693	
No.emp 100+	ref.			ref.			Ref.		
No.emp 20-99	-0,0259092	0,0898916		0,041491	0,0805073		0,3221844	0,174288	*
No.emp 10-19	0,2538613	0,1103642	**	0,3109446	0,096381	***	1,006243	0,1804411	***
Hotel & restaurant	ref.			ref.			Ref.		
Manufacturing etc.	0,9873956	0,1952957	***	1,222574	0,1825145	***	-0,079968	0,2973544	
Electricity	0,879844	0,3759997	**	0,5553113	0,4573751		-31,36082	4886123	
Construction	-0,2470216	0,2664871		0,8724579	0,2038902	***	0,5083548	0,3157936	*
Wholesale	0,2622327	0,2042328		0,3321716	0,1904567	*	-0,1085461	0,2846017	
Agriculture etc.	-1,190963	0,7416252	*	0,8980367	0,3107682	***	0,9419132	0,4057836	**
Transport	-0,0113959	0,2310327		0,0866555	0,2163787		-0,3150583	0,340449	
Finance	0,092983	0,2224478		0,218971	0,209373		0,1960403	0,2949197	
Service	0,1322402	0,2347264		0,5414847	0,2162402	**	0,2644679	0,318429	
Ratio full-time w.e.	-0,2052592	0,2580361		0,1906102	0,1064012	*	-0,9461731	0,3935732	**
Pct.skilled emp.	0,0004273	0,0030656		-0,0016327	0,0024728		-0,0088098	0,0042155	**
Pct.manager	-0,0140028	0,0043692	***	-0,0075334	0,0036207	**	-0,0215194	0,0056862	***
Pct.unskilled emp.	0,0035019	0,0031863		0,0033127	0,0026325		-0,0149776	0,0048532	***
Pct.white collar emp.	-0,0075638	0,0039649	*	-0,0221446	0,0036641	***	-0,01412	0,0054761	***
Av.wage dif. Whi.col.	0,0044145	0,0010006	***	0,0020708	0,0009194	**	-0,0010111	0,0015359	
Pct. reemployed	0,0047603	0,0022433	**	0,0003488	0,0017424		-0,007029	0,0031045	**
Internal absorbed	ref.			ref.			Ref.		
External absorbed	0,2510562	0,26563		0,3672714	0,2349214		0,7853126	0,5019383	
Other	0,1461015	0,2201684		0,1343184	0,1971956		0,2429031	0,4570774	
Constant	-4,760801	0,4534903	***	-4,08172	0,4082536	***	-3,601324	0,7369304	***

Continued	Education	Std. Err.		Out of Labor Force	Std. Err.		No category	Std. Err.	
Man	ref.			ref.			ref.		
Woman	0,1704787	0,1437238		0,4783565	0,0610758	***	0,0134372	0,1296913	
Age 15-30	ref.			ref.			ref.		
Age 31-50	-0,6807569	0,2707217	**	-0,007639	0,0806699		0,0916532	0,1440156	
Age 51+	-30,42967	1002580		1,441145	0,0844497	***	0,289463	0,1920579	
Single	ref.			ref.			ref.		
Single parent	-0,7821477	0,7325537		0,2934434	0,1808553	*	-0,3783551	0,4044694	
Couple	0,2152844	0,1673133		0,1610802	0,072098	**	-0,3905351	0,1458981	***
Couple parent	-0,6835957	0,2721625	**	0,0650547	0,0828259		-0,620878	0,1530085	***
Basic edu.	ref.			ref.			ref.		
Vocational edu.	-0,096936	0,1834329		-0,1473733	0,0628057	**	-0,0291642	0,1352984	
Further edu.	0,3576159	0,2463856		-0,0333092	0,0912307		0,3082329	0,171965	*
W.E. unskil.	ref.			ref.			ref.		
W.E. skilled	-0,2642311	0,1743747		-0,2844845	0,0777263	***	-0,3978938	0,1646115	**
W.E. whi.col.	-0,5077023	0,3113765	*	-0,4207682	0,1008646	***	-0,5096536	0,2085771	**
W.E. manager	0,1286812	0,3468145		-0,1795232	0,125234		-0,7100157	0,2480199	***
Tenure	-0,1397693	0,05739	**	0,0147091	0,0061438	**	-0,0400038	0,0162116	**
Dane	ref.			ref.			ref.		
Immigrant	-0,7472671	0,3801065	**	0,651524	0,1088167	***	0,8177899	0,2001377	***
Income(A)	-0,0000178	1,55E-06	***	-2,35E-06	3,58E-07	***	5,95E-07	2,84E-07	**
Regional UI	0,16315	0,0421427	***	0,0663854	0,0169859	***	0,0563204	0,0366835	
1995	ref.			ref.			ref.		
1996	0,7216069	0,3089008	**	-0,0438997	0,1148285		0,5558664	0,2550139	**
1997	0,8076691	0,3212709	**	0,0695582	0,1189612		0,6718519	0,2634891	**
1998	1,218597	0,3462822	***	0,0820774	0,1314644		0,6351554	0,2876167	**
1999	1,202484	0,3768602	***	-0,0719624	0,1449653		0,2284927	0,3272717	
2000	1,901699	0,5670406	***	0,1577431	0,2157781		0,4791271	0,4759631	
No.emp 100+	ref.			ref.			ref.		
No.emp 20-99	-0,1060066	0,1824119		-0,1828563	0,0659994	***	-0,1669278	0,1345854	
No.emp 10-19	-0,0527243	0,205828		0,0107694	0,0787385		0,0682737	0,1629568	
Hotel & restaurant	ref.			ref.			ref.		
Manufacturing etc.	0,3846668	0,2708424		0,3358914	0,1292318	***	0,7032847	0,3098589	**
Electricity	-29,0853	3785772		0,2291691	0,2869659		0,633679	0,6632897	
Construction	0,5593833	0,3891464		0,0652528	0,1596751		0,2881988	0,3676216	
Wholesale	-0,009282	0,2336472		0,0499936	0,1298789		0,2216987	0,3166142	
Agriculture etc.	0,6385717	0,4839659		-0,2715438	0,339222		1,404067	0,4660323	***
Transport	0,7049859	0,2962486	**	0,1219219	0,1457853		0,5135579	0,3314301	
Finance	0,2954307	0,2512588	**	0,0021126	0,1407039		0,2629513	0,3303818	
Service	0,4098838	0,274814		0,4306703	0,141504	***	0,2136956	0,3696963	
Ratio full-time w.e.	-1,734668	0,4871071	***	-0,587107	0,1796918	***	0,1444489	0,1569085	
Pct.skilled emp.	-0,002896	0,0042993		0,001132	0,0020559		-0,0018977	0,0042765	
Pct.manager	0,0004496	0,0069108		-0,0049837	0,0027664	*	-0,0013806	0,0054192	
Pct.unskilled emp.	0,0040049	0,0049713		-0,0041201	0,0022577		-0,0119104	0,0051278	**
Pct.white collar emp.	0,0021617	0,0065304		-0,0079615	0,0026761	***	-0,009066	0,0052118	*
Av.wage dif. Whi.col.	0,0017476	0,0019284		-0,0010308	0,0007915		0,0012821	0,0013825	
Pct. reemployed	0,0033891	0,0037286		-0,0026871	0,0014944	*	0,0018499	0,0029344	
Internal absorbed	ref.			ref.			ref.		
External absorbed	1,71856	0,7557436	**	0,0067086	0,1876489		0,4988842	0,3432329	
Other	0,9940085	0,7237286		-0,1130519	0,1532054		-0,1826359	0,3009289	
constant	-3,159772	0,9534126	***	-2,090547	0,3074217	***	-4,420342	0,6711094	***

Note: No. obs = 22826 ; Log likelihood = -15578.063 ; Pseydo R2 =0.1075 *Source: Danish register-based employer-employee panel data set from 1995 to 2001.*

Table 8. The basic model

	Long-term unemployed			Short-term unemployed			Self-employed		
	Coefficient	**Std. Err.**		**Coefficient**	**Std. Err.**		**Coefficient**	**Std. Err.**	
Man	ref.			ref.			ref.		
Woman	0,516816	0,0812574	***	0,1530326	0,0724299	**	-0,8791556	0,165288	***
Age 15-30	ref.			ref.			ref.		
Age 31-50	1,32404	0,1230237	***	0,4493884	0,0901595	***	0,2880542	0,1600194	*
Age 51+	2,172798	0,1317167	***	0,8229573	0,1102284	***	0,2300145	0,2174575	
Single	ref.			ref.			ref.		
Single parent	0,1518846	0,2178361		0,2071723	0,2001838		0,5509128	0,3878797	
Couple	-0,0493018	0,0980232		-0,0187785	0,0879428		-0,0233709	0,1702971	
Couple parent	-0,372671	0,1117212	***	-0,1536185	0,0928927	*	0,0397464	0,1634922	
Basic edu.	ref.			ref.			ref.		
Vocational edu.	-0,2914432	0,0837519	***	-0,1758994	0,0737552	**	-0,0034003	0,141834	
Further edu.	-0,3200595	0,1298846	**	-0,3346931	0,1196589	***	-0,3643651	0,1950868	*
W.E. unskil.	ref.			ref.			ref.		
W.E. skilled	0,172708	0,0973647	*	0,1564604	0,0832057	*	-0,5736566	0,1609584	***
W.E. whi.col.	-0,540813	0,1312303	***	-0,7303446	0,1217856	***	-0,5464625	0,1983598	***
W.E. manager	-0,2175264	0,17508		-0,6146187	0,1658897	***	0,1201973	0,205483	
Tenure	0,0162426	0,0075656	**	-0,0080475	0,0077422		-0,0212899	0,0150907	
Dane	ref.			ref.			ref.		
Immigrant	1,199304	0,1281165	***	0,7320752	0,1269598	***	0,3239416	0,2644097	
Income(A)	-4,38E-06	5,43E-07	***	-2,07E-06	4,26E-07	***	3,50E-07	3,00E-07	
Regional UI	0,0842411	0,021862	***	0,0405804	0,020785	*	0,0409874	0,0378075	
1995	ref.			ref.			ref.		
1996	-0,3241946	0,1410861	**	-0,1908196	0,1283375		0,4351415	0,2343922	*
1997	-0,1861278	0,1451312	**	-0,2231712	0,1355039	*	0,4907071	0,2438535	**
1998	-0,0495913	0,1576006		-0,1449985	0,1453307		0,3296433	0,2713522	
1999	-0,1515008	0,1747901		-0,2257489	0,1603474		0,167514	0,3033167	
2000	-0,1241584	0,2730831		0,0311026	0,2487164	*	0,306607	0,4643412	
Cons	-3,779111	0,2863779	***	-2,707542	0,2564642	***	-4,350279	0,4655894	***

Continued	Education			Out of labour force			No category		
	Coefficient	Std. Err.		Coefficient	Std. Err.		Coefficient	Std. Err.	
Man	ref.			ref.			ref.		
Woman	0,1617905	0,1383707		0,4571623	0,0583633	***	-0,0490626	0,1250393	
Age 15-30	ref.			ref.			ref.		
Age 31-50	-0,5857854	0,2688368	**	-0,031672	0,0791606		0,1097404	0,1414675	
Age 51+	-29,4411	645737		1,405456	0,0823029	***	0,3092081	0,1903565	*
Single	ref.			ref.			ref.		
Single parent	-0,8402656	0,7300108		0,3133839	0,1797616	*	-0,3351029	0,4010183	
Couple	0,2338975	0,1661357		0,1489508	0,071387	**	-0,3900413	0,1456837	***
Couple parent	-0,6833143	0,2700364	**	0,0557666	0,0820181		-0,5988034	0,1521538	***
Basic edu.	ref.			ref.			ref.		
Vocational edu.	-0,1240868	0,1815915		-0,1753962	0,0620779	***	-0,0277172	0,134703	
Further edu.	0,3574909	0,239395		-0,0442148	0,0897818		0,3635054	0,16914	**
W.E. unskil.	ref.			ref.			ref.		
W.E. skilled	-0,4533973	0,1516159	***	-0,2234053	0,0683959	***	-0,2472066	0,1431745	*
W.E. whi.col.	-0,6134514	0,2868331	**	-0,6070304	0,0912842	***	-0,5588702	0,1852883	***
W.E. manager	0,1045638	0,31943		-0,2490325	0,1152623	**	-0,6639484	0,2238841	***
Tenure	-0,1742046	0,055236	***	0,0113883	0,0058109	**	-0,0306524	0,015276	**
Dane	ref.			ref.			ref.		
Immigrant	-0,6438074	0,3754182	*	0,6976322	0,1075297	***	0,8018431	0,1985835	***
Income(A)	-0,0000187	1,50E-06	***	-3,08E-06	3,51E-07	***	5,50E-07	2,57E-07	**
Regional UI	0,1714485	0,041037	***	0,0629897	0,0164572	***	0,0361816	0,0354665	
1995	ref.			ref.			ref.		
1996	0,7076908	0,2822609	**	0,0611365	0,1035572		0,6084813	0,2329508	***
1997	0,7900721	0,2974052	***	0,1704509	0,1069591		0,7158619	0,2390376	***
1998	1,284572	0,3189574	***	0,1756627	0,1174666		0,6990837	0,259831	***
1999	1,198878	0,3505008	***	-0,0194912	0,1308252		0,2316504	0,2980806	
2000	1,977589	0,5418573	***	0,1756084	0,2014459		0,3808592	0,4428485	
Cons	-2,315342	0,5479706	***	-2,600384	0,2073219	***	-4,327422	0,439826	***

No. obs = 22826 ; Log likelihood = -16009.901 ; Pseudo R2 =0.0828

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 9: Test of IIA, H_0: The difference in coefficient are not systematic

	Values of $\chi 2$ with 39 df	Prob > $\chi 2$
Short-term unemployed	47.65	0.1613
Long-term unemployed	52.48	0.0732
Education	$3.5\ e^{-5}$	0.0000
Out of labour force	50.81	0.0975
No category	23.75	0.9741

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 10: Test of combining categories, H_0: All coefficient except intercepts associated with given pair of outcomes are 0

		Values of $\chi 2$ with 39 df	Prob > $\chi 2$
Self-employed-	Short-term unemployed	302.127	0.0000
	Long-term unemployed	428.001	0.0000
	Education	347.520	0.0000
	Out of labour force	345.717	0.0000
	No category	159.740	0.0000
	Wage earner	253.216	0.0000
Short-term unemployed-	Long-term unemployed	204.180	0.0000
	Education	366.586	0.0000
	Out of labour force	324.864	0.0000
	No category	180.369	0.0000
	Wage earner	535.569	0.0000
Long-term unemployed-	Education	364.985	0.0000
	Out of labour force	259.161	0.0000
	No category	305.304	0.0000
	Wage earner	877.042	0.0000
Education-	Out of labour force	264.543	0.0000
	No category	314.859	0.0000
	Wage earner	401.688	0.0000
Out of labour force-	No category	244.741	0.0000
	Wage earner	1062.281	0.0000
No category	Wage earner	145.225	0.0000

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 11. Goodness of fit for the extended model

Predicted / Actual	Wage earner	Self-employed	Short-term unemployed	Long-term unemployed	Education	Out of labour force	No category
Wage earner	0.832	0.807	0.759	0.708	0.724	0.737	0.796
Self-employed	0.012	0.030	0.012	0.010	0.013	0.013	0.014
Short-term unemployed	0.040	0.042	0.067	0.069	0.045	0.053	0.049
Long-term unemployed	0.029	0.029	0.054	0.085	0.025	0.056	0.038
Education	0.010	0.011	0.008	0.004	0.091	0.010	0.010
Out of labour force	0.062	0.064	0.084	0.110	0.083	0.115	0.071
No category	0.014	0.016	0.016	0.015	0.016	0.014	0.022

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 12. Goodness of fit for the basic model

Predicted / Actual	Wage earner	Self-employed	Short-term unemployed	Long-term unemployed	Education	Out of labour force	No category
Wage earner	0.828	0.825	0.785	0.733	0.737	0.745	0.808
Self-employed	0.013	0.017	0.012	0.011	0.010	0.011	0.014
Short-term unemployed	0.041	0.040	0.053	0.058	0.048	0.053	0.044
Long-term unemployed	0.031	0.031	0.046	0.068	0.026	0.056	0.035
Education	0.010	0.010	0.008	0.004	0.082	0.010	0.010
Out of labour force	0.063	0.060	0.082	0.111	0.081	0.111	0.070
No category	0.014	0.016	0.015	0.014	0.016	0.014	0.019

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 13. Predicting long-term unemployment by the extended model

	Predicted long-term unemployed	Predicted NOT long-term unemployed
Long-term unemployed	18.67	2.89
NOT long-term unemployed	81.33	97.11
Total	782	22044

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Table 14. Predicting long-term unemployment by the basic model

	Predicted long-term unemployed	Predicted NOT long-term unemployed
Long-term unemployed	14.71	3.03
NOT long-term unemployed	85.29	96.97
Total	782	22044

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Tabel 15. ROC area for the extended model and the basic model

	Obs	**ROC area**	**Std. Err**
Extended model	22826	0.7769	0.0093
Basic model	22826	0.7600	0.0089

H_0: Area(extended model)= Area(basic model)

χ^2 (1) 27.02 Prob> =0.0000

Source: Danish register-based employer-employee panel data set from 1995 to 2001.

Chapter 2
Does job-related training increase future wages?

Abstract

In addition to the many initiatives by the Danish government to upgrade formal skills in the workforce there are also a lot of education and training initiatives at private firms. Nearly 40 percent of Danish employees received job related training (JRT) at their workplace in 1995 and the percentage has dramatically increased over the last 10 years.[16] The employee's wage return to JRT is the focus of this paper. Given a unique Danish panel data including administrative data and survey data on employers and employees the effect of JRT on wages is analysed. The information on employees' participation in JRT in 1995, wages, and historical job shifts make it possible to take individual specific effects into account and to instrument job separation. To overcome the potential endogeneity between wages and job separations by using historical job shifts as an instrument is new in the JRT literature. The results show that the OLS estimates are consistent even when job separation is included as an exogenous variable. Moreover women with vocational education who received JRT and then separated to a new job receive a high wage return. The JRT has a positive and significant effect on wage return among men and women with a vocational education. Surprisingly no wage return to JRT is found among other educational groups. Finally the overall wage return to the extensive JRT in Denmark is very small compared to international findings.

[16] Author's calculations on the JRTDS data from 1994 to 2004 described in section 5.

1. Introduction

The Danish government and many other European governments have focused on the importance of formal education and schooling. At the same time Danish firms are investing more and more money in upgrading the skills of their employees through job related training (JRT). In 1995 nearly 40 percent of Danish employees received JRT.[17] The share and the money invested in JRT has increased over the last 10 years and today Denmark has one of the highest JRT rates among OECD countries (OECD 2005). However, only one other study (AKF 2006) has briefly investigated the effect of JRT on wages in Denmark. The effect of JRT on wage return is the focus of this paper.

More than 40 years ago Shultz (1961), Mincer (1958) and Becker (1962) set up a theoretical framework explaining how investment in human capital increases future wage income. Becker (1962) extended the framework by making the distinction between two kinds of human capital. One is general human capital that is transferable between employers. Therefore the employee pays all the cost and also gets the whole pay off in terms of general skills. The other is the firm specific human capital that is not transferable among different firms. Therefore, the employers and employees share the cost and the pay off.

Some empirical results confirm that the wage return to JRT is positive such as Parent (1999). Others find that only specific kinds of JRT result in a positive wage return (AKF 2006; Bartel 1995; Loewenstein and Spletzer 1998; Lynch 1992; Regnér 2002). There also exist studies that argue that most training among employees has a big element of generality that other employers in the labor market demand and the employers still pay most of the training costs (Loewenstein and Spletzer 1998, 1999; Xu 2005). Studies that show no substantial effect of JRT on wage return and studies that show that a lot of JRT is general and firm sponsored fit the extended human capital framework that takes imperfections into account (Acemoglu and Pischke 1999). Acemoglu and Pischke's model predicts that due to compressed wage structures and distortions on the labor market the return to JRT is reduced and in some cases eliminated. Furthermore their model illustrates that (depending on the cost function of training) employers might have less incentive to finance JRT or that they might have an incentive to sponsor general training. Finally, the model predicts that employees

[17] Author's calculations on the JRTDS data from 1994 to 2004 described in section 5.

receiving JRT will separate less in a labor market with a compressed wage structure than in fully competitive labor markets.

Very few studies have looked at the wage return to JRT in Scandinavia (AKF 2006; Evertsson 2004; Regnér 2002; Schøne 2001). However, the list of JRT studies in the US and the UK is long (Bartel 1995, Blanchflower and Lynch 1992, Frazis and Loewenstein 2005, Krueger and Rouse 1998, Lengermann 1999, Loewenstein and Spletzer 1995, 1998 and 1999, Lynch 1992, Parent 1999, Veum 1995). Previous studies have shown that the effect of JRT on future wages can be empirically difficult to analyze. In most situations if an ordinary Mincer wage equation is estimated the estimates will be biased due to selection with respect to training. The problem occurs when employees with certain characteristics (e.g. high aptitude, many skills) receive JRT and the same characteristics are correlated with a high wage independent of receiving JRT or not. This results in an estimated effect of JRT that is biased. This problem has to be accounted for when evaluating JRT. The selection into JRT is always looked at from the employee's side. However the employer's preference for offering JRT is decisive for an employee to receive JRT. Therefore it is interesting to see if the employees who actually enter JRT also are the employees who employers prioritise in terms of JRT offers.

Many JRT studies have included job separations as an exogenous variable, which seems problematic when previous job mobility literature show the opposite (among others: Gibbons and Waldman 2004; Gibbons and Katz 1991; von Wachter and Bender 2006). Job mobility studies have shown that job leavers who leave voluntarily tend to benefit from a separation whereas the employees who leave involuntarily experience the opposite. Once again it depends on what kind of workers is evaluated (e.g. young or old) (von Wachter and Bender 2006).

Thus an evaluation of the effect of JRT on wage return demands a lot of data if both the training selection and the potential job separations selection are taken into account. Lack of good JRT data might be the reason why so few studies have been undertaken in Scandinavia. This paper can take into account individual fixed effects and the potential job separation selection by a first difference wage regression where job separation decisions are instrumented through job separations histories. The empirical strategy is only possible because of the availability of good Danish longitudinal micro

data (in this paper referred to as JRTDS). The JRTDS combines two surveys, one for employees and one for employers, and a panel of administrative data. The data set has four advantages. First, the detailed information on the length and cost of JRT makes it possible to clearly define JRT. Second, the existence of a reliable panel of tax recorded wages makes it possible to take individual specific effects into account. Third, the employer survey makes it possible to check if employers' preference for offering JRT corresponds with the employees who actually enter JRT. Thereby a different insight to the selection into JRT is illustrated. Fourth, the historical information on job separations within the last 5 years of the employee's career is a good indicator for an employee's likelihood of switching jobs. At the same time it is reasonable to assume that the history is orthogonal to the wage increase at the current workplace. Thus, historical job separations are a good instrument for job separations.

JRT is defined in many different ways in the literature. If an employee participated in any kind of job related education within the last year he or she is defined as a JRT participant in this paper. The JRT can take place both inside and outside the workplace, where a workplace is a legally-registered unit at a specific address. The JRT does not include the informal training that an employee receives while completing job assignments (e.g. an employee is taught to work a machine by a fellow colleague). It is possible that the employee finances some of the training costs, but in general more than 85 pct of all Danish JRT is financed by the employer. Finally JRT in Denmark includes shorter education courses and training, but not long-term formal educations (e.g. apprenticeships).

The paper is set up as follows: Section 2 explains the theories behind JRT. Section 3 gives an overview of previous empirical JRT evaluations. The Danish labor market and JRT are described in section 4. Section 5 describes the rich Danish register data. Descriptives statistics are illustrated in section 6. In section 7, the empirical strategy is discussed. Section 8 contains the empirical results on the wage return to JRT. Section 9 concludes.

2. The theory behind JRT

In labor economics it is fundamental to look at the relationships between wages and human capital. Becker's (1962) traditional human capital theory is often used to explain

the returns to JRT. However, the traditional human capital predictions are far from supported by previous empirical studies and today's labor markets also do not fit the framework perfectly. Therefore new training frameworks that question the assumptions of full information and flexible wages have come forward (Acemoglu and Pischke 1999). Due to the fact that most previous studies relate their empirical results to the traditional human capital framework that framework is first described. Afterwards the extended human capital framework with labor market distortions is applied and discussed.

Traditional general and specific human capital theory

The traditional human capital theory explains that increased experience and human capital result in an upward sloping wage curve over an employee's lifetime (Shultz 1961; Mincer 1958; Becker 1962). The conclusion is that more human capital means higher productivity and consequently higher earnings. A human capital increase in the form of JRT is therefore expected to increase the wage return.

Originally Becker (1962) analyzes the wage effect of two kinds of human capital in a perfectly competitive labor market without wage distortions and other labor market imperfections. One kind of human capital is general and transferable between firms. The transferability gives the employee the incentive to pay for all the training costs and afterwards receive all the returns to training. The other kind of human capital is assumed to be firm specific and is not transferable among different firms. The specificity gives the employers and employees the incentive to share the costs and the return to training. Becker's idea is illustrated in Figure 1. The figure clearly shows the three conclusions of the traditional model. First, if nobody invests in training then no wage increase occurs over time (see black line).[18] Second, investment in either general or firm specific training decreases the employee's wage income during the training period and increases future wages. However, the employee who receives firm specific training only receives a wage increase if he or she stays at the same firm. Third, the employees receiving general training expect the largest wage growth after training because they neither share the pay-off or the training costs with the employer (see dashed line).

[18] Wage increase due to experience is not illustrated in the figure.

JRT in a labor market with wage distortion and imperfections

Acemoglu & Pischke (1999) among others have questioned the traditional human capital theory concerning a perfectly competitive labor market without any distortions. Acemoglu & Pischke (1999) illustrate the difference between training in a perfectly competitive labour market and labor markets with imperfections and compressed wage structure by Figure 2.

Figure 2 shows that in a perfectly competitive labor market the employee is paid a wage equal to the marginal product of his skills, f(JRT) (i.e. the thin black line). f(JRT) is also the wage the employee can receive in other firms. Now suppose there exist a separation cost, C, of making a job-change. The cost could simply be due to the outside employer's lack of information regarding the employee's training experience outcome. The cost reduces the employee's wage outside the current firm to f(JRT)-C (i.e. the dashed line). The current firm can make the employee stay at the firm by paying him or her f(JRT)-C. Thus the current workplace benefits from the separation costs, because if the employee is only paid f(JRT)-C, then C is the profit of the firm. In this example the separation cost is not a function of the training. Therefore the employer has no incentive to invest in further training because the profit stays the same.

Now look at Figure 2 and the line for the compressed wage structure where the separation cost is a function of training, C(JRT). It is assumed that the separation cost increases with respect to training, thus C'(JRT)>0. The intuition is that the most skilled employees (i.e. employees who have received the most JRT) have the highest separation costs because the more courses (and maybe even more specific courses) the more difficult it is for the outside firms to evaluate the value of the courses. The employees outside wage option is then f(JRT)-C(JRT) (i.e. the thickest black line). Compared to the perfectly competitive labor market, the employer's profit increases with the employee's training level. Thus, the employer has an incentive to increase the training level of his or her employee to increase profits.

From Figure 2 three conclusions are drawn. The first conclusion that Acemoglu and Pischke (1999) draw is that a compressed wage structure allows employers to benefit from investing in JRT even when it is general. The reason is that the compressed wage structure prevents employees from receiving the full return of

general training in other firms. Therefore he or she stays at the current employer if the current firm offers the market wage (i.e. a wage below the employee's marginal productivity). Thus the employer benefits from offering general training and paying the employee the outside market wage. The second conclusion is that labor market imperfections (similar to separation cost and a compressed wage structure) reduce an employee's return to training. Finally given a separation cost, employees separate less from their current employer.

Different labor market imperfections and a compressed wage structure

In the following I discuss how different labor market distortions can cause a compressed wage structure and thereby make the set up by Acemoglu and Pischke (1999) the most realistic. A distorted labor market can be due to search frictions and the monopsony power of the employer. Suppose an employee can only search for another job by quitting the job he or she has and then search for a new job (i.e. on the job search is not possible). Then an employee's expected outside option is reduced if there exists a risk of becoming unemployed. The reduced expected return compresses the wage structure and the return to training for the employee, but it increases the profits for the current employer. Furthermore it will improve the monopsony power of his or her own employer if the employee realizes that the outside employers will not pay the full return to training because they have some kind of monopsony power. This forces the wage structure to become compressed. The search friction and monopsony power of the employer reduce the wage return of training and the preference for separation among trained employees.

Suppose that training courses are commonly accepted and well-known among employers but the employee's ability to apply the course is unknown to the outside employer. Then the outside employer is insecure of the actual marginal productivity of a newly trained employee. The employee could be a high ability trained employee who has the ability to apply the training, but the employee could also be a low ability trained employee who does not have the ability to apply the training. Thus the outside employer will then pay a return to training that takes into account the risk of getting a low ability employee who can not apply the training received earlier. Once

again, the outside wage is compressed and the current employer does not have to pay the full marginal product to keep his trained employees.

Unions often play a very important role in wage settings with respect to wages, training, tenure, experience etc. There might be strict rules about what kind of wage benefits employees can get with specific training skills on top of all the rules on promotions, hourly wage, holidays etc. In some countries the employer and employee unions agree on minimum wages or wage floors as well. A minimum wage means that the cost of training can be difficult to transfer to the employee and the wage structure is distorted. Here the unions distort the flexibility of the wages especially downwards. Again the return to training will be reduced and the employee will separate less likely. Maybe the employee even receives less training because the minimum wage makes it impossible to transfer the training cost to the employee.

Until now the compressed wage structure inside a workplace has been caused by the distortion in the outside wages. However, the compressed wages can also be a result of wage policies inside a workplace. Here the explanations are generally found in the literature on personnel economics, where employers set wages to avoid adverse selection. An example is efficiency wages where the employer does not know the employee and takes into account that some employees shirk. Thus, the wage starts at a low level but increases until there is an incentive not to shirk (Lazear 1998). Thus the employer does not pay the marginal product of training, and the wage structure is again distorted. The results are again a lower wage return to training, less job separations and maybe less training.

To sum up, the predictions of the extended human capital model are different from the traditional human capital model in four ways. First, an employee can receive general training from an employer due to the separation costs. This is not possible in the traditional model. Second, as in the traditional model the employee's wage return increases with training. However, given the separation cost the wage return is lower in the extended model than in the traditional model. Third, the employee in the extended model has a reduced incentive to separate from the current employer due to the separation costs. Fourth, the employee might receive less training in the extended model than in the traditional model because the costs of training are not transferable due to a high wage floor for example.

The existence of labor market imperfections and a compressed wage structure clearly change the traditional conclusion for training in Becker's standard human capital framework. Even though most JRT studies have used the traditional human capital framework when evaluating their empirical results as I show in section 3, the extended human capital model with labor market distortion and a compressed wage structure might be more appropriate for the evaluation on JRT in Denmark. It will be possible to see if this is true after the description of the Danish labor market and Danish JRT in section 4.

3. Literature review on JRT

Previous studies on the wage return to JRT have focused their empirical analysis on the traditional human capital theory background material. Therefore, two areas of the JRT evaluation process have been the centre of attention. First, area is the JRT definition and the separation between general and firm specific training. Second area is the biased estimate of JRT due to the correlation between individual training heterogeneity and wages (i.e. training selection). In the following I illustrate how previous studies have taken the just described areas into account and the ambiguous empirical results of the effect of JRT on wage return. Furthermore the lack of attention paid to the potential job separation endogeneity in the JRT literature is commented upon.

Different JRT definitions

The JRT literature is characterized by using different words for the same thing and then at the same time not defining JRT in exactly the same way with respect to JRT duration, costs and substance (see appendix 1). Thus JRT includes (as defined in previous literature) on-the-job training, off-the-job training, formal training, seminar training, company training, courses, and apprenticeships, etc.

A number of studies use the US National Longitudinal Survey of Youths from 1979 (NLSY79). Therefore a lot of JRT definitions are based on the information included in that data (Blanchflower and Lynch 1992, Frazis and Loewenstein 2005, Krueger and Rouse 1998, Lengermann 1999, Loewenstein and Spletzer 1995, 1998 and 1999, Lynch 1992, Parent 1999, Veum 1995). In the first studies on the NLSY data, JRT only included duration spells of at least 4 weeks (Lynch 1992; Loewenstein and

Spletzer 1999; Parent 1999). Moreover, studies from the UK and Norway did not have information on short JRT spells (Arulampalam and Boot 2001; Blundel et al. 1996; Booth et al. 2003; Evertson 2004). In an evaluation of JRT it is problematic if all short spells are treated as non-participants, because instead of evaluating the effect of JRT, one would evaluate the effect of long JRT spells compared to employees with no JRT and short JRT spells. Arulampalam and Booth (2001) also show that it is important to have each training incidence because when they look at training spells of more than 3 days it is the incidence of training and not the number or length of training spells that has an effect on the wage return.

Other JRT studies use an employee's opinion about the length of required JRT for a specific job as a proxy for the amount of JRT the employee has received (e.g. Schøne 2001). This definition is problematic to use when evaluating JRT, because it is difficult to define when JRT took place and if it took place at all. Thus making an evaluation on the wage return to actual JRT is impossible especially because before and after wage information is difficult to define (i.e. it is impossible to know which wage is received before the JRT and which wage is received after the JRT).

Especially studies from the US, the UK and Germany include apprenticeship training in their JRT definition (Lynch 1992, Parent 1999, Blundell et al. 1996). In some countries such as Denmark including apprenticeship in JRT is inappropriate because an apprenticeship education is part of the formal educational system and is generously subsidized by the Danish authorities.

Many previous studies make empirical analyses based on the traditional human capital theory described in section 2. Therefore the studies divide JRT into general and firm-specific training. The division in many cases relies on where training takes place. In US studies for example, firm-specific training (i.e. on-the-job training) takes place in the firm and general training (i.e. off-the-job training) takes place outside the firm (Lynch 1992; Blundell et al. 1996; Loewenstein and Spletzer 1999; Parent 1999; Veum 1995; Xu 2005). The geographical division between general and firm specific human capital is problematic. Suppose that big firms have more employees needing the same kind of training than small firms. Thus the big firms would probably save money by paying the cost of hiring a teacher in house instead of paying the transportation cost, maintenance cost etc. for all the trainees taking a course outside the

firm. Thus, firm size influences whether JRT takes place at the firm. The context of the JRT training doesn't influence whether JRT takes place at the firm.

Obviously the changing JRT definitions can result in different empirical results, which make it challenging to compare different study results.

Overcome the selection bias in JRT participation

Studies on the effect of JRT on wage return indicate that if employees who receive training also receive high wages due to high aptitude, then the estimated effect of JRT on wage returns in a simple Mincer wage equation becomes biased.

To combat the JRT selection previous studies have instrumented the JRT (see appendix 1 table B: Parent 1999; Xu 2005). Intuitively these studies have used variables that affect the probability of training participation, but do not affect the wages other than through their effect on JRT participation. For example Parent (1999) uses the employees deviation from the stock of training with-in job means to calculate employee job training participation risk. Whereas, Xu (2005) uses among others the spouse's training experience in estimating the employees training participation. Thus he assumes that an employee's spouse preference for JRT is correlated with an employee's preference for JRT and not correlated with the employee's wage return. Suppose JRT is necessary if the spouse chooses a certain income path and that income path certainly must influence the employee's possibilities with respect to his or her own income path. For example if a couple has children one would expect that one of them would try to work hard in order to obtain a high income and the other would try to work less (i.e. low income) in order to take care of the family. Thus it is difficult to see how the spouse's JRT decision does not influence the employee's wage return.

Others have first looked at the correlation between JRT and observable characteristics. Then they have instrumented the training risk by assuming a certain functional form for the risk of training with respect to observables (see appendix 1 table B: Lynch 1992; Veum 1995). Many studies have found the same observable socio-economic characteristics to be correlated with JRT (see appendix 1 table C). Thus studies in the Netherlands and the US find that both young women and men are more likely to receive JRT (Lynch 1992; Maximiano and Oosterbeek 2006). Furthermore studies from the US, the UK, the Netherlands and Sweden find that well educated

employees receive more JRT (Altonji and Spletzer 1991; Arulampalam and Booth 2001; Blundell et al. 1996; Evertsson 2004; Krueger and Rouse 1998; Lynch 1992; Maximiano and Oosterbeek 2006; Veum 1995). Altonji and Spletzer (1991) find that more US women than US men receive JRT. On the other hand, Lynch (1992) finds that US women are more likely to receive off-the-job training but less likely to receive on-the-job training. Finally Maxiamo and Oosterbeek (2006) find that women in the Netherlands are more willing to train than men. Even though there is a clear correlation between socio-economic observables and JRT, assuming a functional form of the observables to instrument the likelihood of receiving, JRT does not seem plausible because the identification is through the functional form.

Another way to approach the selection problem is to assume that selection into training is due to individual aptitude where aptitude is independent of time. Then looking at wage growth (i.e. wages before and after receiving JRT) for each employee would difference out the individual specific fixed effect. Several studies from the US and the UK have analyzed the effect of JRT on wage growth (see appendix 1 table A: Booth et al. 2003; Hamil-Luker 2005; Loewenstein and Spletzer 1998 and 1999; Lynch 1992; Veum 1995).

Additionally some studies have claimed that the selection into JRT is due to both a time independent person specific fixed effect and a time dependent person specific effect. Thus the studies from the US, the UK and Norway instrument or predict the selection of employees into JRT in the wage growth estimation (see appendix 1 table A: Arulampalam and Booth 2001; Krueger and Rouse 1998; Schøne 2001; Veum 1995). Whereas Krueger and Rouse (1998) use an exogenous shift in subsidy to JRT programs to estimate the training probabilities, others use the probability of entering training as an instrument (i.e. the functional form is what determines the selection). Again, choosing a random functional form does not seem like a logical way to instrument the likelihood of receiving JRT. However, an exogenous shift in subsidy is workable, but often not possible for the time periods analyzed.

Previous studies have also found a correlation between JRT and the likelihood of job separations (see appendix 1 table D: Krueger and Rouse 1998; Loewenstein and Spletzer 1997; Lynch 1991; Parent 1999). Suppose that workers who choose to separate from workplaces also try to receive more JRT because that improves

their wage bargaining situation in a new job.[19] Thus there might be a combined effect of JRT and separations (i.e. an interaction effect).

Previous studies have looked at a combined effect of receiving JRT at the previous employer or the current employer as mentioned above, but the focus has been on finding the wage return to different kinds of JRT and not the separation decision. Thus an employee's job separation has been included as an exogenous variable. However, there exists a large literature set on job mobility and wage return as well as the potential endogeneity problem with respect to job separations (among others: Gibbons and Waldman 2004; Gibbons and Katz 1991; von Wachter and Bender 2006). In a JRT framework suppose that an employee decides to quit his or her job because he or she is promised a better wage somewhere else and not the other way around where the employee separates and then receives a higher wage. Then there clearly exists an endogeneity problem with respect to job separations and wages. This potential endogeneity problem has not been taken into account in previous JRT studies.

Previous results on wage return to JRT

Most empirical studies find that JRT has a positive return no matter if the measured outcome is wage or wage growth (see appendix 1 table A and B). However, the average estimated return to training has been ambiguous. Perhaps it is because the JRT concepts and the JRT environment vary a lot from study to study. One extreme is Xu (2005) who finds a log of wage return to JRT in 1994 in China of 1 percent. The other extreme is Parent (1999) who finds that the incidence of JRT increases the log of wage return by 14 percent in the US. The training return is significantly reduced when the return is measured by wage growth (see appendix 1 table B: Booth et al. 2003; Hamil-Luker 2005; Loewenstein and Spletzer 1998 and 1999; Lynch 1992). However, some of the previous studies' results are difficult to interpret because they do not include first differences of the explanatory variables (e.g. Hamil-Luker 2005; Loewenstein and Spletzer 1998 and 1999; Lynch 1992). It is noticeable that Veum (1995) is the only study that finds no effect of JRT using the simple first difference regression model. Furthermore it is understandable that the wage return in the first difference estimations

[19] Another example of the interaction effect is a firm specific effect, where firms with high separation rates also offer a lot of JRT.

are smaller than in the simple wage regressions, because the wage growth estimations as described earlier account for individual specific effects.

Even though most studies find positive returns to JRT, the return to JRT are different with respect to kinds of JRT, the duration of JRT, the timing of JRT, job shifts and trainees personal characteristics. For example, Arulampalam and Booth (2001) in the UK and Veum (1995) in the US show that it is the training incidence that initiates a wage return and not the duration of training. This is in contrast to the traditional human capital theory model where the duration of training is positively correlated with the wage increase.

Dividing JRT into general and firm specific training is essential in the traditional human capital theory, but the empirical results are very mixed. As already mentioned most studies use the geographical situation of the JRT to separate between general and firm specific training. The empirical results of Lynch (1992) and Xu (2005) confirm the original human capital theory. Lynch (1992) finds that both off-the-job training (i.e. general training) at the previous firm and on-the-job training (i.e. firm specific training) at the current firm have positive wage effects. Furthermore on-the-job training at the previous firm has no effect. Additionally, Xu (2005) finds that only off-the-job training has a positive wage effect.

Other studies find no clear evidence of the traditional theory's division between general and specific human capital. For example Parent (1999) finds that all on-the-job training at the previous workplace has a positive wage effect too. Instead Blundell et al. (1996) show that women obtain no wage increases by taking on-the-job training.

Even though some studies show that the division influences the return to training differently, and authors claim the results thereby give an indication of the effect of general training and firm specific training as the traditional human capital theory predicts, the results are clearly ambiguous. An obvious reason why some studies do not find clear evidence on different kinds of human capital is that the assumptions about a perfectly competitive labor market without wage distortions and labor market imperfection are not valid in labor markets such as the US and Europe.

Instead some empirical findings support Acemoglu's and Piscke's (1999) extended human capital model with labor market distortions and a compressed wage

structure. Loewenstein and Spletzer (1999) actually claim that on-the-job training includes general training. Furthermore, Lowenstein and Spletzer (1998) show that most JRT is paid by the employer among young Americans, even when the training is general. However, the more general the JRT is, the less likely it is that the employer finances the JRT. These findings support the extended model's conclusion that employers earn a profit by offering general training to their employees, which is in contrast to the traditional human capital model.

As mentioned earlier some studies find no wage return to JRT, which again supports the model just described (see appendix 1 table A and B: Krueger and Rouse 1998; Loewenstein and Spletzer 1998; Lynch 1992). Finally for example Parent (1999) finds that trained employees are less likely to leave their current employer. This is in accordance with the extended human capital model prediction.

Clearly the empirical results from previous JRT studies support and reject predictions from the traditional human capital model and the extended human capital model.

4. The Danish labor market and JRT

The effect of JRT on wage return at least in theory depends on the labor market in which JRT occurs. In this section the Danish labor market is described and afterwards the extensive amount of Danish JRT taking place is illustrated.

The Danish labor market

The Danish labor marked is characterized by a so-called flexicurity model, where the labor market is flexible because it is easy to fire and hire employees (like in the US) and at the same time there exists a highly developed social security system (like in the other Nordic countries). Given the generous social security system, the influential labor market organisations, high income taxes, and a minimum wage floor, the wage structure is very compressed.[20] So overall, the Scandinavian countries have a very compressed wage structure compared to the US or the UK.

[20] Additionally the participation rate is relatively high among both genders in Denmark. Especially Danish women's participation rate is relatively high compared to other developed countries. Since 1994 the female participation rate has been at least 76 percent.

The unions play an important role in the wage determination of Danish jobs. In 1995 more than 80 percent of the workforce was a member of a union organization. Even though the percentage has decreased over the last decade the organization degree is still above 75 percent. The employee's unions and the employer's union are two of the players in the Danish tripartite labor market model. In this setting, the employer's union and employee's union discuss wage and working condition regulations. "The State" only interferes if problems occur regarding the centralised collective bargaining agreements. Until 1995 the central collective bargaining agreements occurred every second year. Since then it has become more flexible within each subdivision of employer's and employee's unions (that were involved in the original bargaining negotiations). This tripartite model and the existence of other labor market institutions clearly distort the free movement of wages and labor. Thus, the standard human capital theory assumption of perfectly competitive labor markets with fully flexible wages and no imperfections does not hold for the Danish labor market.

The share of skilled employees in the Danish work force has increased dramatically over the last decade. In 1995 around 60 percent of the adult population between 25-64 years of age had an upper secondary education (OECD 1997).[21] Thus the adult population was skilled but not as highly educated as the Czech Republic, Germany, Norway, Switzerland and the US. The US had more than 80 percent of their adult population with an upper secondary education. By 1999 the share of people with an upper secondary education in Denmark increased to 80 percent (OECD 2001). Thus Denmark entered the group of countries (Czech Republic, Germany, Japan, Norway, Switzerland and the US) with the highest skilled adult population.

Figures 3 and 4 show clearly that the share of people with tertiary education (i.e. Danish vocational education and further education) have increased since 1994.[22] Therefore the educational level among the adult Danish population has increased. The educational development follows the Danish policy-makers intention of improving the educational skills in the Danish workforce to meet globalization requirements. Therefore, formal education is generously subsidized in Denmark (for a detailed description of the formal education system see Weatherall (2007)). The formal

[21] In the Danish system upper secondary education is high school +
[22] In OECD terms tertiary education include vocational and further education.

educational framework is very different from the JRT framework that is initiated by employees and employers.

The Danish JRT

In addition to the investment in formal education, Danish policy makers have been encouraging firms to invest more in training their current employees (both with and without a subsidy from the Danish Government). This kind of education and training is what OECD calls "job-related continuing education and training". Denmark's participation rate in job-related continuing education and training is about 50 percent among 25 to 64-year-olds in 1998/1999 (OECD 2001). The Danish participation rate is the highest among OECD countries. In the workforce, the participation rate is highest among adults with a tertiary education. In Denmark, 29 percent of the population with a lower secondary education participated in job-related continuing education and training in 1998/1999. 51 percent participated among the people with an upper secondary and post-secondary non-tertiary education. Finally, 70 percent participated among the people with a tertiary education. Highly educated women in Denmark had an especially high participation rate compared to men. The mean hours women used on job-related training were high as well relative to men.[23]

Among the Danish employees the participation rates in job-related education and training are 52 percent among men and 58 percent among women, which is still the highest among all employees in OECD countries in 1998/1999. Although, the mean number of hours employed participants use on job-related education and training is relatively high (111 hours) it is lower than the mean number of hours used in other countries such as Ireland, the Netherlands, New Zealand, and Norway.

This paper focuses to a certain extent on what the OECD refers to as "job-related continuing education and training" among the employed. More precisely an employee is defined as a JRT participant if he or she has participated in any kind of job-related education or training within the last year. The JRT participant group is further described in section 6.

[23] No comparable OECD numbers are available for Denmark from 1994-1997. Instead within a 4 –week period 15 percent of Danish men (13 percent) and women (17 percent) received job-related continuing education and training.

The compressed wage structure in Denmark, the generous formal education system, and the extensive and broadly defined JRT program are important to have in mind when comparing the empirical results of this paper with empirical results on JRT in other countries.

5. Combined survey and administrative data on JRT

An analysis of the effect of JRT on wages demands a lot of data. First, very detailed information on training duration, training costs, and training contents is necessary to define JRT. The costs or the geographical situation of the JRT are especially used often to define general and firm specific training. Second, longitudinal wage information is necessary to separate the training effect and the effect of individual heterogeneity on wages. Third, job separation information that is not correlated with wages is needed to solve the potential endogeneity problem between job separations and wages. Most of the necessary information is contained in the two surveys and in the longitudinal administrative data that form the job-related training data set (JRTDS).

The JRTDS

The JRTDS comprises three data sets. The first data set is a sample of the panel data set called The Danish Work Environment Cohort Study (DWECS). DWECS uses a split panel design. The 1990 panel is a random sample of the population from 18-59 years of age as of October, 1st, 1990. This population was re-interviewed in 1995 and 2000 irrespective of participation in previous rounds. To correct for the aging and migration of the 1990 panel, additional random panels were collected in 1995 and 2000, in order to ensure that the samples in 1990, 1995 and 2000 all were representative samples of the whole population. In the 1995 survey, employees were interviewed about JRT. This information is applied in this paper. The 1995 sample consists of 5127 employees.

The employee survey contains an extensive amount of variables covering various themes such as occupational exposures, health, job specifics, industry specifics, BMI, occupations, occupational accidents, labor market status and specifics about JRT. The JRTDS is especially good because it contains precise records on training duration within a year, whereas for example the US NLSY79 in its early stages only had information on training periods for more than 4 weeks. Another advantage of the

Danish data is the detailed information on course financing and the information on courses. A minor disadvantage is that the Danish survey only has information on JRT in 1995 and the data does not include many JRT observation years. Thus it is possible to evaluate the effect of JRT in 1995 when there is precise information on the length of JRT, the cost of JRT, the employee's job situation, and the employer's characteristics.

The JRTDS also contains information about employees' historical job changes. This information is relevant when analysing the wage effect of JRT among the employees that either stay in the current firm or separate from a firm. The already mentioned potential endogeneity problem of job separations and wages can be overcome if the separation selection can be instrumented. In other words, if there is a variable that can explain the likelihood of separating from a job and the variable at the same time is not correlated with wage growth then the variable can be used as a valid instrument. The JRTDS includes employees past records on job separations. Thus, the individual risk of separating from a current job can be instrumented by the individual's history of job changes.

Another source of information is a register panel data set from Statistics Denmark from 1994 to 2004, which contains records on all employees from the DWECS. The data contains detailed information on socio-economic characteristics such as; age, family status, educational skills, personal income, wages, social transfers and unemployment histories. Additionally, there is detailed information on employees' workplaces and when employees switch workplaces. All variables are recorded annually except for unemployment history. The unemployment and activation histories are reported as spells on a daily basis. The precise unemployment histories and occupational status make the exact categorization of full time employed wage earners possible.

The detailed administrative records on wage incomes from 1994 to 2004 are also very valuable. Compared to self-reported wages in surveys, the administrative records are easier to compare across individuals because the information comes from the official tax forms across time. In surveys, employees often remember wages differently, because some remember net wages, some remember the gross wages and others remember wages including pensions and other benefits. Finally, the panel

structure makes it possible to look at wages before and after JRT and thereby take individual fixed effects into account.

The last source of data is an employer survey where employees state their workplace in 1995. The workplaces were also interviewed in the following year, which was 1996. The workplace survey contains information about management, strategies and practices related to maintaining and qualification upgrading of employees. The information on qualification upgrades includes JRT. The data is used as a supplement to illustrate how workplace preferences for offering JRT match the employees that actually receive JRT. The information on workplace investment in JRT can describe which employees are most likely to receive JRT.

Final sample of JRTDS

To evaluate the effect of JRT on wage growth only a sample of the extensive data just described is selected. Given that JRT information exists in 1995 the immediate effect of JRT can be measured by looking at wages in 1994 and 1996. Thus the panel data period is restricted to three years from 1994 to 1996. Wage growth is only possible to measure if an individual receives a wage before and after receiving JRT. Therefore only the fulltime employed in 1994, 1995 and 1996 are selected. As previously described the return to JRT is expected to change with a job separation, and therefore the employees who either stay in the same workplace for three years or switch workplace the year after potentially receiving JRT (i.e. work in a new workplace in 1996) are selected. The described selection results in a final sample consisting of 3347 employees. 1791 are men and 1556 are women.

6. Descriptives on Danish JRT, wages, and job separations

This section illustrates the distribution of JRT among full-time employees and shows that even though previous studies have separated between general and firm specific training it does not make sense in a Danish context. Furthermore I use simple statistics to see if the theoretical and empirical claim - JRT increases the wage return - holds for the Danish data. Finally, due to previous studies claim of selection problems the likelihood of receiving training and separating from a job is analysed. As an extension workplaces' JRT offers are compared to the employee's likelihood of receiving JRT,

because the selection into training is dependent on the employee's and the employer's preferences for JRT.

Given previous studies results that wages and JRT are strongly correlated with gender and educational background all the following descriptive statistics are made separately for gender and education.

JRT distribution

As described in section 5 the analysis uses a sample of Danish employees who are full-time employees from 1994 to 1996 and who either stay at the same workplace for three years or change workplaces in 1996. Table 1 shows the selected sample include employees who on average receive more JRT, have a higher wage income, but not wage growth, and who separate less from their workplaces compared to average employees.

The following description focuses on the characteristics of the selected sample of employees. Table 2 illustrates that over 55 percent of Danish men and over 60 percent of Danish women received JRT in 1995. Clearly more women receive JRT than men. The majority of women and men receive 1 to 10 days of JRT within the year. Furthermore, over 10 percent of the employees who receive JRT receive over 20 days of training within a year.

Table 2 also illustrates the positive correlation between the percentage of employees receiving training and educational skills. Thus, among the group of employees with further education, 74 percent of men and 83 percent of women receive JRT within 1995. Whereas among the non-educated only 47 percent of men and 45 percent of women received JRT.

JRT includes education and training both inside and outside the workplace, but JRT does not include the informal training that an employee receives while doing his or her job assignment. Thus, the often used geographical criterion for separating general and firm specific training is not possible with the Danish data. Instead the cost information with respect to JRT could be used as a criterion. Following the traditional human capital theory then, JRT is firm specific when the workplace pays for some of the training expenses and JRT is general if the employee pays for all the training. In the Danish sample more than 85 percent of all JRT is financed totally by the

employer.[24] At the same time most of the employees claim that there skills can be applied in other workplaces. Therefore it does not seem appropriate to separate between general and firm specific JRT by JRT expenses.

Wages and JRT

International studies in section 3 and the theoretical models in section 2 illustrated a positive correlation between JRT and wages. The simple statistics in Table 2 support to some extent the theory, because employees who receive more than one day of JRT have on average higher yearly wage income. However, it does not support the theory that the employees who receive only one day of JRT have an average yearly wage income less than the employees who receive no JRT. The correlation between JRT and average wage growth are similar to correlation between yearly wages and JRT. One exception is the low wage growth among women who receive between 11 and 20 days of JRT. Among educational groups the employees who receive JRT also on average receive a higher yearly wage except for the men with further education. The wage growth is even more puzzling, because only men and women with a vocational education and women with no education have on average a higher wage growth when receiving JRT.

Studies have shown that empirical estimates of the effect of JRT can be biased due to the correlation between individual heterogeneity and wages (i.e. training selection). In table 3 columns 2 and 3 show the likelihood of receiving JRT among men and women. The results indicate that both young women and men are more likely to receive JRT. The employees that have a further education are more likely to receive JRT. Not surprisingly working in a high paying occupational group (i.e. often the most educated) increases an employee's probability of receiving JRT, too. For women only the just mentioned characteristics influence the likelihood of receiving JRT, whereas men are influenced by more socio-economic and workplace characteristics. Clearly men, living as a couple with long employment experiences, and working in big private industry workplaces such as wholesale, finance and services are more likely to receive JRT. The characteristics that influence the likelihood of receiving JRT among Danish employees are also found in other countries. For example, the positive effects of long work experiences and big companies or departments are especially found in other JRT

[24] Previous studies found that most JRT is financed by employers (Loewenstein and Spletzer 1999)

studies (appendix 1 table C: Blundell et al. 1996; Lynch 1992; Maximiano and Oosterbeck 2006). However, many previous studies find that union membership increases the likelihood of receiving JRT. For Danish employee unions, membership has no significant effect.

The selection into training is most likely analyzed by looking at employee information. However, an employee's decision with respect to JRT and job separations depends on the decisions of their employer, too. Suppose the employee wants to receive JRT but his or her employer will not finance it. According to the above results the employee would either not take the JRT, maybe pay for the JRT himself or herself, or simply quit the job due to lack of JRT. This employee-employer interaction influences the selection into JRT and thereby also the return to JRT. Therefore it is interesting to look at the preferences for offering JRT among employers, which is the main focus in the following descriptive paragraphs.

Table 4 clearly shows that workplaces to a higher extent prioritize JRT offers to employees from skilled occupational groups. Furthermore, more skilled occupational groups are offered longer periods of JRT and more money on JRT.

A simple OLS regression among interviewed workplaces illustrates the correlation between the average number of JRT days offered and workplace specific characteristics. Table 5 contains the OLS regressions for all workplaces and separate regressions for; workplaces that offer a minimum of one day of JRT, workplaces that have all occupational groups represented in their workforce, and workplaces that offer a minimum of one day of JRT and have all occupational groups in their workforce. The results are similarly independent of the selection criteria. Workplaces with affiliates, many employees, and a high percentage of skilled employees in industries such as construction and services offer more days of training. Furthermore workplaces that take many human management initiatives such as yearly employer-employee meetings, organizational changes for helping the employees, reduction of routine job tasks, and reduction of physical and psychological hard work offer a significantly higher amount of JRT days.

Table 6 illustrates the OLS regressions of workplaces offering JRT separate for four occupational groups. The results are nearly identical to the results of JRT days offered to all employees in a workplace. Additionally, the results show that

the average wages of certain occupations are positively correlated with JRT offers among the highly skilled occupational groups (e.g. managers and high paid wage earners) but not among other occupational groups.

The analysis of JRT offers among workplaces support the findings for employees. Thus the employees who are more likely to receive JRT are also the employees that workplaces prefer to sign-up for JRT offers.

Job separation and JRT

On average, close to 30 percent of Danish employees separate from their jobs every year (Weatherall 2007). In this paper's sample of fulltime employees who have been employed for a minimum of three years the separation rates are a lot lower – 15 percent among men and 9 percent among women. It is not surprising that the job separation rate is low because the sample does not include all the employees who separate into unemployment or who have temporary contracts and change jobs frequently (i.e. more than every second year).

Table 7 shows that the average amount of JRT and wage growth are different among employees who stay in the same workplace and employees who switch workplaces. Men separate more often from their workplaces than women. Furthermore, men and women who have a vocational education have on average received more JRT before they separate. The same is true for women with further education. In contrast non-educated women who separate from their workplaces have on average received less JRT. The statistics also clearly show that men and women who separate from their workplace have on average a higher wage growth. It is not clear if it is the separation that causes the wage increase or if it is the expectations of a future wage increase that causes the separation (i.e. endogeneity problem).

The likelihood of separating from a workplace is illustrated in Table 3. Danish men are more likely to separate if they are young, unskilled and have short tenure. Furthermore working in big private companies for the hotel and restaurant industry increases the probability of separating from workplaces. In contrast very few socio-economic and workplace characteristics influence the separations for women. The results in Table 3 show surprisingly that single women are less likely to separate from a workplace than single women with kids. Furthermore women from relatively small

private companies are more likely to separate from a workplace. These findings are related to the specific sample of full time workers from 1994-1996 that is selected for the analysis.

The descriptive statistics indicate that there are correlations between wages, JRT, and job separations. It is therefore necessary to take these relationships into account and consider possible selection problems when picking an empirical strategy in the following section.

7. Empirical model – individual fixed effect and job separation endogeneity

The theoretical models in section 2 showed that investment in JRT increases the productivity of the employee and thereby the wage supposedly increases. The traditional human capital theory also shows that the division between firm specific and general training can influence the wage return to JRT for employees who separate from one firm to another. Finally, the extended human capital theory illustrated that the wage return to JRT declines and maybe even disappears in an economy with compressed wage structures. Job separations also decline with respect to JRT due to the existence of a separation cost.

The empirical model is supposed to capture the predictions from the human capital theories. However, as the literature review clearly showed the selection problem due to individual specific effects with respect to JRT and job separations are important to account for in the empirical setup, because it is the key to reducing the bias of the JRT estimates.

First consider a simple Mincer wage equation in the following way:

$$(1) \qquad \log(w_{it+1}) = \alpha_0 + \alpha_1 D96 + \beta JRT_{it} + X_{it+1}\gamma + \varepsilon_{it+1}$$

Where log of wages for employee i at time t+1 (i.e. 1996) is a function of receiving JRT, *JRT*, and some socio-economic characteristics, *X*, such as experience, tenure, occupation etc. In this setting ε is the independent error term and $\alpha_0 + \alpha_1 D96$ are the constant term consisting of a constant and a time dependent constant in 1996. The estimated β is the effect of receiving JRT in 1995 on log of wages in the following year.

Obviously employees and firms only invest or receive JRT if they expect the employee's productivity to increase, which also means that wages increase. As previously mentioned, an individual specific aptitude that is not contained in any of the *X*'s could exist. Thus the estimated effect of JRT might then capture the individual specific aptitude instead of the JRT effect (i.e. the simple Mincer equation would estimate a positive JRT effect). The estimated β becomes biased because it captures the effect of JRT. For example the high paid wage earner already has a high wage due to an individual specific effect.

As mentioned earlier a way to take care of selection that is independent of time is looking at wage growth (i.e. before and after receiving JRT). Thus, the problem in previous studies of finding an instrument is overcome. So a bias caused by an individual specific fixed effect is easily overcome by estimating the effect of JRT by using a first difference log wage equation, as follows:

$$\log(w_{it+1}) - \log(w_{it-1}) = \alpha_1 + \beta(JRT_{it}) + (X_{it+1} - X_{it-1})\gamma + (\varepsilon_{it+1} - \varepsilon_{it-1}) \quad (2)$$

In other words, in this analysis the log wage increase from 1994 to 1996 is a function of a time trend factor α_1, the JRT received between 1994 and 1996 (i.e. JRT received in 1995) and the difference in observed characteristics between 1994 and 1996 and an independent error term. Note that JRT is equal to zero in 1994.

One could argue that the selection into JRT is time dependent. Therefore it is necessary to take the selection of training into account (in the first difference estimation as well). By not taking selection into account in the wage growth equation, β would again be biased just like in the simple Mincer equation.

As described in section 3, previous studies have tried to take training selection and individual fixed effects into account simultaneously, but for two reasons it is not done here. First, it is not obvious that the selection into JRT is time dependent if one thinks about the individual specific effect as an unobserved aptitude for implementing the JRT. Second, even if it is time dependent it is difficult to find a good instrument. Assuming that a random functional form of observable characteristics can identify the likelihood of receiving JRT it is not reliable as argued in section 3. On the other hand using an instrument such as Xu(2005) (described in section 3) is feasible if

the instrument fulfils two assumptions; correlation with the endogenous variable and the orthogonality of the error term. Moreover, Kruger and Rouse's (1998) use of an exogenous change in the JRT program as an instrument is feasible. However, neither a good instrument nor an exogenous change is possible in this analysis of the Danish JRT in 1995.

In a job separation situation as earlier mentioned two things have to be taken into account. First, as previously indicated it is plausible that there is a combined effect of JRT and job separations. Therefore an interaction-term of JRT and job separation is included in the model. Second, a potential endogeneity problem exists when looking at separations and wage return.

The endogeneity problem can be overcome if the separation can be instrumented. Good instruments are characterized by satisfying two requirements. First, the instrument must be correlated with the endogenous variable and because more instruments are used it is important to see if the instruments are jointly valid. Second, the instruments must be orthogonal to the error term. In other words the instrument is not supposed to influence the outcome variable (wage return) other than through the endogenous variable (job separation). As explained in section 5 the JRTDS includes employees past records on job separations. Thus the individual risk of separating from a current job can be instrumented by the individual's history of job changes. Clearly it is necessary to instrument both the job separation variable and the interaction term between job separations and JRT. Then the wage growth regression accounting for individual fixed effects and the separation selection is estimated in the following way:

$$\begin{aligned}(3)\qquad &\log(w_{it+1}) - \log(w_{it-1}) = \alpha_1 + \beta(JRT_{it}) + \delta(JRT_{it} * separ_{it+1}) + \varphi(separ_{it+1}) \\ &+ (X_{it+1} - X_{it-1})\gamma + (\varepsilon_{it+1} - \varepsilon_{it-1})\end{aligned}$$

Where the incidence of separation is estimated in the following way:

$$\begin{aligned}&separ_{it+1} = \lambda(historsepar_{it}) + \phi(historseparsquared_{it}) + \kappa(JRT_{it}) + (X_{it+1} - X_{it-1})\vartheta \\ &+ \pi(JRT_{it} * historsepar_{it}) + \rho(JRT_{it} * historseparsquared_{it}) + \eta_{it+1}\end{aligned}$$

(4)

$$\begin{aligned}&(JRT_{it} * separ_{it+1}) = \varsigma(JRT_{it} * historsepar_{it}) + \omega(JRT_{it} * historseparsquared_{it}) \\ &+ \psi(historsepar_{it}) + \sigma(historseparsquared_{it}) + \theta(JRT_{it}) + (X_{it+1} - X_{it-1})\xi + \tau_{it+1}\end{aligned}$$

In equation (3) the change in socio-economic factors are included, $(X_{it+1} - X_{it-1})$. Compared to the simple Mincer wage equation the variables included in equation (3) are changes in the socio-economic variables, and not the level. Thus the changes affect the wage growth and not the level. Variables such as; experience, tenure, occupation group, working industries and working sector have often been included in previous studies. Here it does not make sense because first of all the sample is full-time employees working in 1994, 1995 and 1996. Therefore experience changes for all employees in the sample for two years. Second, tenure is strongly correlated with job separations, because all the employees that separate will have no tenure or negative tenure. Third, a change of occupational group could change the wage growth, but it does not really help explaining the causality of JRT and wage growth. Fourth, changing jobs from one industry or sector to another industry or sector could affect the wage growth because the different industries and sectors have different wage growth rates. However, in the sample hardly any of the employees who separate from their jobs change industry or sector. Therefore it is not relevant to include these variables. Instead, only the change in the local unemployment rate is included in the equation (3). The local unemployment rate affects the employee's job opportunities and the employers hiring opportunities as well as the wage growth in the local areas.

To sum up, this paper estimates the return to JRT by taking individual fixed effects into account and by instrumenting the employee's likelihood of job separations in order to solve the potential endogeneity problem.

8. Empirical results

The results of the empirical model just described are illustrated in Tables 8 and 9. The results contain a simple first difference wage regression on JRT, a first difference wage growth regression on JRT and job separations, and finally the first difference wage growth regression where job separation is instrumented by past job separations. As illustrated in the descriptive section 4, it is sensible to look at the results separately for gender and educational background.

Instruments

The reason for instrumenting the job separation decision stems from an assumption that an endogeneity problem exists between wage growth and job separations. If endogeneity exists then the estimated coefficients will become biased. To overcome the endogeneity problem good instruments as explained in section 7 can help.

Even though the instruments are valid it might be the case that an ordinary least square estimator of the regression without the instruments would yield consistent estimates and it is therefore reasonable to test the endogeneity assumption. The tests of the instruments and the endogeneity are illustrated in Table 10 for both men and women. Table 10 illustrates the tests for models both with and without an interaction term.

In this analysis the first requirement means that the endogenous variables (i.e. job separation and/or the interaction between job separation and JRT) have to be correlated with the instruments (i.e. number of job separations since 1991; number of job separations since 1991 squared; interaction between number of job separations since 1991 and JRT; interaction between number of job separations since 1991 squared and JRT). The relevant test statistics would be from the first stage reduced form equation of the endogenous variables on the instrument (see table 10). The test of the strength of the instruments is accepted among men with vocational education, because of the relative high F-values and first stage estimations are accepted. However, the instruments are valid for the models both with and without the interaction term. The instruments do not seem valid for all other educational groups among men and women. The number of observation is quite small for all educational groups except for men with vocational education. A small number of observations can explain why the instruments do not show up as valid for educational groups different from men with vocational education. Because the instruments only are accepted among men with a vocational education it only makes sense to look at the rest of the test statistics for these men.

In models including at least two instruments it is possible to test for overidentification. The Sargan-test tests for joint validity of the instruments (i.e. overidentification). Clearly the null hypotheses can not be rejected among men with vocational education. Thus the excluded instruments are jointly valid independently of including the interaction term.

Finally the additional endogeneity test has been performed through the Du-Wu-Hausman test. The test results show that separation is exogenous. Therefore the OLS estimator is consistent for men with vocational education. So the interpretation of the results should be based on the OLS estimates because the OLS produces lower standard errors compared to the IV estimator even if one is looking at models with the interaction term.

The largest number of observations and reliable test results are among men with vocational education. Therefore the test results from this population group are applied to the rest of the population. Thus because the test results indicate that conclusions should be drawn on the OLS estimates this is done for all men and women within all educational groups.

Job separations

The descriptive statistics indicated a positive correlation between job separations and wages. The empirical results in Tables 8, 9, 11 and 12 show a positive effect of job separation among men with vocational education. The effect is nearly 3 percent (p-value of 18 percent). Even though as the test showed conclusions should be drawn on the OLS results it is interesting to look at the IV results for men with vocational education where the instruments were valid. Both the JRT effect and the separation parameters increase when instrumenting the separation decision, but the parameters are not significant. Thus a bigger sample is needed to make further conclusions.

A job separation among men with further education also increases the wage growth significantly. Dependent on the inclusion of the interaction term or not the wage return increases between 10 to 15 percent if the employees change jobs. It is noticeable that separation affects wage growth in a group where JRT has no effect on the wage growth.

Whereas the combined effect of job separations and JRT do not show up among men and women with no education and further education. This effect is found among women with a vocational education. The wage return actually increases by nearly 17 percent if a woman receives JRT and separates from the job afterwards.

For the people without an education and women with further education, separations have no effect, but again these results could be due to the small number of observations.

The effect of JRT

The estimated wage returns to JRT with respect to educational background are illustrated in Table 8 for men and Table 9 for women. As just discussed the instrumental approach does not improve the first difference wage growth estimates. Therefore the conclusions will be drawn on the results from the OLS estimations of wage growth.

The results clearly show that only men and women with a vocational education receive a positive return to JRT. JRT has a 3 percent significant and positive effect on an employee's wage growth from 1994 to 1996. Although for women the JRT effect is between 2 and 4 percent dependent on the inclusion of the interaction between JRT and job separation.

All other educational groups among men and women are not getting any immediate wage return out of receiving JRT. Among employees with a further education or no education, JRT affects the wage return insignificantly negative. This result is surprising because section 5 showed that employers preferred offering JRT to the most skilled employees and the most skilled employees also received the most JRT. Thus, one would expect that employees with further education would also receive a high wage return from training.

All the results estimate the effect of the incidence from training. Now, suppose that the length of JRT is important for the wage return of JRT and that the vocationally trained received the longest spells of JRT, which is the reason why they receive the highest wage return. That could explain the positive results among men and women with a vocational background. However, dividing the JRT up into the length of JRT within a year, the estimation results in Tables 11 and 12 support the above findings and do not show some new surprising causalities. The men and women with vocational education receive a positive and significant wage if they receive between 1-5 days of JRT. The men's wage return to JRT is also positive if the JRT duration is 6-10 days. For all other educational groups among men and women the wage return to JRT is hardly

ever significant, but if it is significant then the return is negative. Thus the differences in wage return among educational groups are not explained by the duration of JRT.

Discussion

Even though JRT is extensive in Denmark for men and women with different educational backgrounds, it is surprising that there is only a significant wage return to JRT for employees with in a vocational education. In studies from the US and the UK, JRT has a significant effect among all employees independent of educational background.

There could be several reasons for the difference in the JRT wage return among educational groups and international results. First, because the analysis distinguishes between educational groups and gender the number of observations in each group becomes small. Therefore some of the insignificant results might become significant with more observations.

Second, as previously argued the Danish wage structure is very compressed compared to the US and the UK. Therefore the wage return to JRT is expected to be lower in Denmark. Among educational groups the wage structures could also be more or less compressed. Looking at the Danish centralised collective bargaining agreements for the time period analyzed two main wage systems agreements occurred. One was the minimal wage system, where the central bargaining agreement resulted in a minimum wage but the individual wages were determined at the local workplaces. The other system was the normal wage system, where the central bargaining agreement resulted in fixed wage structures for the whole agreement period. In its simplest form the normal wage system resulted in no wage flexibility with in a workplace except with respect to tenure and job specific assignments. The normal wage system was common for the unions for many of the workers with further education, whereas the minimal wage system was common for all other education groups. Thus the workers with further education seemed to work under a more compressed wage system than workers with vocational education and no education. This can explain why no JRT effect is observed among workers with further education and an effect is observed among the workers with vocational education. Previous studies on wages and the influence of JRT or for example motherhood show considerable variations by education

level, too (Lynch 1992; Anderson et al. 2002). Thus different wage structures might also exist among education levels in other countries as well.

Third, by looking at the union agreement for office and trade (HK: most employees with a vocational education) and the union agreement for lawyers and economists (DJØF: most employees with a further education) one might get the impression that the wage return to education and training is more formalized among the first group. Thus the formalization of the monetary value from JRT causes automatic wage return, which means it is not necessary to negotiate on a yearly basis among HK employees and employers (as is the case among DJØF employees and employers).

Finally, there could be other pay-offs to receiving JRT that is not measured in wage growth. Suppose that attending a JRT course during work hours is better than working. Maybe the course even increases your efficiency. Being more efficient may lead to improvements in family and work-life. So the employees do not improve wages, but improve their life utility (Quality of Life). This scenario could be more relevant among the highly educated (versus those with vocational education).

The results on separations and a potential endogeneity problem is no cause for alarm for all previous JRT studies that treat job separation as an exogenous variable, because the test results showed that the OLS estimates were consistent. It is not obvious how the results fit into a labor market with a compressed wage structure by looking at the separation results regarding men and women with different educational levels. However the results support the suspicion that there could be different wage structures within different educational levels.

9. Conclusion

This paper posed the question whether JRT increases future wages. Whereas most JRT studies investigate the JRT predictions of the traditional human capital theory (with no labor market distortions) this analysis focuses on the JRT and separation predictions of the extended human capital theory (with labor market distortions) because the later fits the Danish labor market framework better. The empirical results are also more in line with the predictions of the extended human capital model with a compressed wage structure.

Given a unique Danish panel data the empirical wage return model can take individual fixed effects into account. Furthermore this JRT study can (as one of the first studies) instrument the potential endogeneity between wages and job separations by the employee's historical job separation profile.

The empirical results are ambiguous within education levels and gender groups. JRT has a positive and significant effect on the wage return among men and women with a vocational education. Surprisingly no wage return to JRT is found among other educational groups. The descriptives clearly show that employees with further education participate more often in JRT and employers also prefer offering JRT to the most skilled employees. Therefore it is puzzling that employees with further education receive no wage return to JRT. Moreover the overall wage return to extensive JRT in Denmark is small compared to international findings. This result supports the extended model prediction where the wage return to JRT is expected to be small or even zero in a compressed wage setting.

The empirical results on job separations clearly show that women who have received vocational education and JRT combined with a job change receive relatively high wage increases. On the other hand, men with a vocational education who separate from jobs receive a relatively small positive wage growth. Interestingly men with further education receive a relatively large wage return to job separation. At the same time the JRT has no significant effect among these men.

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Appendix 1. Literature review

Table A: JRT studies where the outcome variable is wage growth

Author	Data and training definition	Method	Result
Arulampalam & Booth 2001	UK: NCDS longitudinal survey on 1765 young men born march 1121 and employed in 1981 and 1991. **Training** is work related training lasting minimum 3 days between 1981-1991.	IV (instrumenting training and number of training incidence through a hurdle negative binomial on personal characteristics) LS: log (gross hourly wage1991, deflated)-log(gross hourly wage1981) RS: etnicity, disability, firm type, firm size, job type, regional ui, highest qualification, training, number of training incidence.	+ training 0 number of training incidences + high education
Bartel 1995	A panel from 1986-1990 of 3800 professional employees personal records from 1 manufacturing firm. **Training** includes core training, corporate employee development program, technical programs	IV (instrumenting training through relative wage status) LS: salary growth RS: training, education, length of service in firm, change of length of service, occupation dummies, year dummies. Murphy and Topels two-step wage growth equation taking into account personal fixed effect (using wage differences) and training selection (using predicted training probability).	**Overall:** + training **Core programs:** - training
Blundell, Dearden & Meghir 1996	UK: NCDS longitudinal survey on 2781 person born in march 1958 and employed 1981. **Training** is non-government work-related training courses min 14 days or 100 hours.	Quasi-difference model (controlling for unobserved fixed effects and productivity chocks) LS: log(hourly wage1991)-log(hourly wage1981) RS: ojt (current and previous), oft (current and previous), other training, qualifications, number of job changes, regions, occupation, union membership, employer size, sector	**Men:** + oft > + ojt + job change **Women:** + oft (current) + not changing job **Education:** + ojt **(0 level men)** + oft **(0 level men)** + voc. training **(0 level women)**
Booth, Francesconi & Zoega 2003	UK: BHPS 950 men born after 1936 full time employed at the survey dates (1991-96). **Training**: Work-related training to improve or increase skills in current job (not introduction training)	First differences LS: annual growth in log (hourly wage) RS: changes in union, changes in training and interactions training-union, changes in (education, tenure, marital status residential location, firm size, current occupation, sector, employer, industry, local ui)	+ training + union + union*training
Hamil-Luker 2005	US: NLS 1977-87 3663 women + NLSY79 1988-98 4056 women **Training**: more than 20 hours	Growth model LS: log hourly wage growh rate RS: ojt, oft, race, age, education, experience, service occupation, children	**Women no high school:** + ojt
Krueger & Rouse 1998	US: Longitudinal data 2 firms 1991-94 (manufacturing 642 employees, service 239 employees) **Training** to low-skilled subsidized by the federal government	IV (instrumenting training through shift) LS: log(hourly wage 1991,92,94 or 95) RS: Training/ training(1994,95), age, tenure, year dummies.	**Manufacturing**: + train (0,005) + train (IV 0,007) **Service:** 0 train + train(IV 0,007)
Lengermann 1999	US: NLSY 1979-1993 **Training** at least 4 weeks until 1988. From 1988 any training program or ojt designed to improve job skills, help finding a job, or learn a new job -> construct training spells	Fixed effect LS: log wage yearly differences RS: years received training	+company training + formal training + long spells

Continued

Loewenstein & Spletzer 1998	US: NLSY 1988-1991 **Training:** current and previous and different kinds and duration. Any training program or ojt designed to improve job skills, help finding a job, or learn a new job	Fixed effect LS: log (wage) – log(wage) RS: employer paid training (incidence, spells, current and previous years), tenure, years, afqt, race, gender, age, marital status, children, urban city, smsa, local ui, multiple-site firm, number of previous jobs, part time work, government employment, industry, occupation	**Current year:** + formal training 0 everything else **Previous years other employer:** + seminar + business school, vocational institute etc.
Loewenstein & Spletzer 1999	US: NLSY 1992-1994 9362 person-year obs and The Employer Opportynity Pilot Project (EOPP) 1982 1527 employers) **Training** any training program or ojt designed to improve job skills, help finding a job, or learn a new job.	Log wage growth equation (NLSY employees , EOPP estimated wages, but don't show the estimates refers to Loewenstein & Spletzer (1998))	+ general skills
Lynch 1992	US: NLSY 1979-83, 3064 non-college graduates. **Training** at least 4 weeks. Training program or ojt designed to improve job skills, help finding a job, or learn a new job	First difference estimator LS: log(hourly wage1983)-log(hourly wage1980) RS: Experience, Tenure, Otj, Ofj, Apprenticeship, Job-change, Union(1983-1980)	0 otj + oft , apprenticeship + jobchange (0,07) + union (0,13) **Low skilled women**: + jobchange
Schøne 2001	Norway: FlexCSSD match employer-employee data 1995-96 (1266 firms 103418 wage earners) & NSOE survey 1989-93 1352 private employees. **Training**: predicted required training for a certain job	First difference (Firm level with firm specific effects. LS: log (hourly wage1996)-log (hourly wage1995) RS: tenure, experience, education, women share, firm size, industry	**Individual:** + training **Firm level:** 0 training
Veum 1995	US: NLSY79 1986-90. 4614 persons 21-29 years **Training:** all types of training	Fixed Effect (selection term for training) RS: Log (hourly wage1990)-log (hourly wage1986) RS: training (company, apprenticeship, oft, business, vocational, correspondence, seminar, other), change in tenure, change experience, job change, change in firm size, change in union membership, change in citysize	**All (- selection)** 0 training **All (+ selecton)** + company training + seminar training + job change Men and women similar except for seminar training

Table B: JRT studies where the outcome variable is wage

Author	Data and training definition	Method	Result
AKF	Denmark: VEU 1996 & 2000. Fulltime employed the quarter before training. **Training:** course participants in Statistic Denmark registeer	Matching Hourly wage, Yearly wage income	**Further education courses:** + training **Vocational courses:** + training **Formal courses**: 0 training / -
Bassanini 2006	EU:ECHP Employed workers 25-54 years of age in 15 countries 1994-00 **Training:** the past year	LS: Gross hourly wage RS: training, fixed effect, age, age2, tenure, tenure2, firm size, public sector, occupation, permanent contract, log hours worked, log hours worked 2, number of previous job, reason of job change	**Overall:** + training (Except training at previous employer: women, more than 35, low educated)
Booth, Francesconi & Zoega 2003	UK: BHPS 950 men born after 1936 full time employed at the survey dates (1991-96). **Training**: Work-related training to improve or increase skills in current job (not introduction training)	Fixed effect LS: log (hourly wage) RS: union, training, interactions training-union, education, experience, tenure, marital status residential location, firm size, current occupation, sector, employer, industry, local ui)	+ union + union*training

Continued

Evertsson 2004	Sweden: Swedish Survey of Living Condition 1994-98. 10721 employees (part time and fulltime same employer for 3 years receiving training). **Training** financed or arranged by employer at least 1 week.	OLS wage regression LS: annual earnings RS: industry, education, tenure, experience, experience squared, work hours, occupation, family status (insert endogenous variables:person and work specific characteristics)	**Men & Women**: + training (0,04) No major differences between different kinds of training. Men> women
Frazis & Loewenstein 2005	US: NLSY 1979-2000 12686 14-21 in 1979. EOPP Cross Sectional Data **Training** at least 4 weeks until 1988. From 1988 any training program or ojt designed to improve job skills, help finding a job, or learn a new job	LS: Log wages on training RS: dummy, linear, quadratic, cube root, log, dummy+linear, fourier series	+ employer-financed training (also when heterogeneity is included)
Loewenstein & Spletzer 1998	US: NLSY 1988-1991 **Training:** current and previous and different kinds and duration. Any training program or ojt designed to improve job skills, help finding a job, or learn a new job	LS: log (wage) RS: employer paid training (incidence, spells, current and previous years), tenure, years, afqt, race, gender, age, marital status, children, urban city, smsa, local ui, multiple-site firm, number of previous jobs, part time work, government employment, industry, occupation	**Current year:** + seminar **Number of spells same employer:** + formal company training + seminars outside work **Number of spells other employer:** + seminars + business school, vocational institute etc.
Loewenstein & Spletzer 1999	US: NLSY 1992-1994 9362 person-year obs and The Employer Opportynity Pilot Project (EOPP) 1982 1527 employers) **Training** any training program or ojt designed to improve job skills, help finding a job, or learn a new job.	LS: Log wage	Ojt is general + formal training at current employer + formal training at previous employer
Lynch 1992	US: NLSY 1979-83, 3064 non-college graduates. **Training** at least 4 weeks. Training program or ojt designed to improve job skills, help finding a job, or learn a new job	Heckmann two-stage procedure LS: Log(hourly wage1983) RS: Previous otj, oft, apprenticeship, Current complete and uncomplete ojt, oft, apprenticeship, Training selection (mills ratio), experience, tenure, school, ui-rate, SMSA, male, nonwhite, healthy, married, union, number of jobs, industry and occupation dummies	**Previous employer:** + Oft + Apprenticeship 0 Ojt **Current employer:** + Otj
Parent 1999	US: NLSY 1979-91 5649 individuals 14-21 in 1978. Employees (at least 6 month within last year) **Training** at least 4 weeks. Training program or ojt designed to improve job skills, help finding a job, or learn a new job.	IV-GLS LS: log (hourlywage 1979) RS: Previous otj, oft, apprenticeship, experience, tenure, race, gender, marital status, union, metropolitan statistical area urban/rural, health, regional ui, number of jobs, regions. Instrumenting training, experience & tenure (through deviation from job-match mean and deviation from individual means) Firm specificity (Proportional hazard rate)	**Current employer** + Training **Previous employer** + Training.
Regnér 2002	Sweden: Swedish Level of Living Survey. Employed 18-65 age. 2636 persons **Training:** Training necessary for the job. To what extent is it usable in other firms	LS: log (hourly wage) RS: training (length), tenure, tenure2, experience, experience2, married, woman, private, education, union, work environment, manager, firm size, job matching, interaction tenure-training	**All**: + general human capital + specific human capital General>Specific **Women, public sector:** + specific human capital **Men, private sector:** 0 specific human capital

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Veum 1995	US: NLSY79 1986-90. 4614 persons 21-29 years **Training:** all types of training	Two-step Heckman procedure (training risk) RS: Log (hourly wage1990) RS: training (company, apprenticeship, oft, business, vocational, correspondence, seminar, other), male, nonwhite, education, ability score (AFQT), tenure, experience, job held, firm size, union, ui, citysize, married, health	**All (without selection)** 0 training **All (with selection)** + company training + seminar training + education 0 job held **Men** + seminar training **Women** + company training + seminar training
Xu 2005	China: State and Life Chances in Urban China 1949-1994. 4073 families **Training:** Retrospective information up till 7 spells	Heckman two-stage procedure RS: log of wage. Excluding restriction: number of children & spouse's training experience	+ off-job training (0,0119) (0,0107). 0 firm specific.

Table C: JRT studies where the outcome variable is JRT

Author	Data and training definition	Method	Result
Altonji & Spletzer 1991	US: NLSH72 3181 persons **Training:** incidence, duration	Probit estimation LS: 0-1 training received RS: experience, experience2, education, aptitude, hours of subjects, occupations OLS LS: number of hours training received RS: see above	**Incidens of training:** Women>men **Duration of training:** Men > women **More training**: blacks, secondary education (aptitude minor effect).
Arulampalam & Booth 2001	UK: NCDS longitudinal survey on 1765 young men born march 1121 and employed in 1981 and 1991. **Training** is work related training lasting minimum 3 days between 1981-1991.	Negbin II hurdle LS: number of training incidences RS: married, kids, ethnicity, disability, schooling scores, highest education, occupation, private school, firm type, firm size, union membership, training prori	**Many training incidences**: married, few kids, low ui, bad reading score, high education, big/"small" company, priori training courses
Bartel 1995	A panel from 1986-1990 of 3800 professional employees personal records from 1 manufacturing firm. **Training** includes core training, corporate employee development program, technical programs	Logit (probability of training) RS: relative wage status, new starter, education, length of service in firm, tenure in job, recruiting (agency, college, write in), year dummies, occupation Tobit procedure (days of training) RS: same (taking the relative status –wage pct. in an occupation group- in to account)	**Training (core & technical)** + relative status (the stars and starters) **Training (employee development)** - relative status (the remedial)
Blundell, Dearden & Meghir 1996	UK: NCDS longitudinal survey on 2781 person born in march 1958 and employed 1981. **Training** is non-government work-related training courses min 14 days or 100 hours.	Probit (likelihood of training) RS: age, log wage first job, sector, employer size, social class, training before 1981, number of jobs, education (all the variables from 1981)	**High risk:** high educated, previously trained, high social class, public sector, uniom member, large firms. Men more likely to take employer provided training
Evertsson 2004	Sweden: Swedish Survey of Living Condition 1994-98. 10721 employees (part time and fulltime same employer for 3 years receiving training). **Training** financed or arranged by employer at least 1 week.	Logit (Risk of receiving training) Multinomial logit (Risk of receiving general / firm specific /industry specific training)	**High risk**: men, high educated /socioeconomic status high, long tenure, long experience, full time working, not a women with presence of small children, in public administration Men participate more in general specific training

Continued

Krueger & Rouse 1998	US: Longitudinal data 2 firms 1991-94 (manufacturing 642 employees, service 239 employees) **Training** to low-skilled subsidized by the federal government	Probit (likelihood of training) RS: age, female nonwhite, married, education, tenure, work shift, log hourly wage1992, production worker, department size, job bids 1992, vacation 1992, performance 1992	**Manufacturing: Highest probability of training** – more education, bidding for job openings, low wage **Service: Highest probability of training** – more than 4 years tenure, low paid, workers in first shift, large departments.
Lynch 1992	US: NLSY 1979-83, 3064 non-college graduates. **Training** at least 4 weeks. Training program or ojt designed to improve job skills, help finding a job, or learn a new job	Probit (Risk of receiving training) RS: male, nonwhite, tenure, experience, education, union, ui, married, previous training	**Low risk of Oft**: young, male, long tenure, occupations(professional and technical staff, service workers). **High risk of Ojt:** white, male, unionized with long experience, in areas with low UI, occupations (managers, ales, clerical, craft)
Maximiano & Oosterbeek 2006	Netherlands: Monitor Post-initial Education 2005. Employed 16-65. **Training:** participation within the last year	Probit (risk of participating) LS: 0-1 training participation RS: age, gender, children, immigrant, schooling, firm size, industry, job permanent, tenure, hours contract. Willingness to train RS: age, gender, children, immigrant, schooling, firm size, industry, job permanent, tenure, hours contract.	**High risk of training:** young, well educated, permanent job, more work hours, less tenure large firms, industries (education, health, financial) **Employees very willing to train**: women, with children, many years of schooling, from large firms, specific industries (education, health)
Veum 1995	US: NLSY79 1986-90. 4614 persons 21-29 years **Training:** all types of training	Probit (likelihood of training (company, apprenticeship or oft) RS: Male, nonwhite, education, ability score (AFQT), tenure, experience, job held, firm size, union, ui, citysize, married, health	**High risk of company training:** well educated, high ability, long experience, many jobs held, from big firms, union member and low local ui **High risk of oft training:** well educated, high ability, many jobs held and low local ui **High risk of apprenticeship training**: not college graduates, high ability, many jobs held, union member

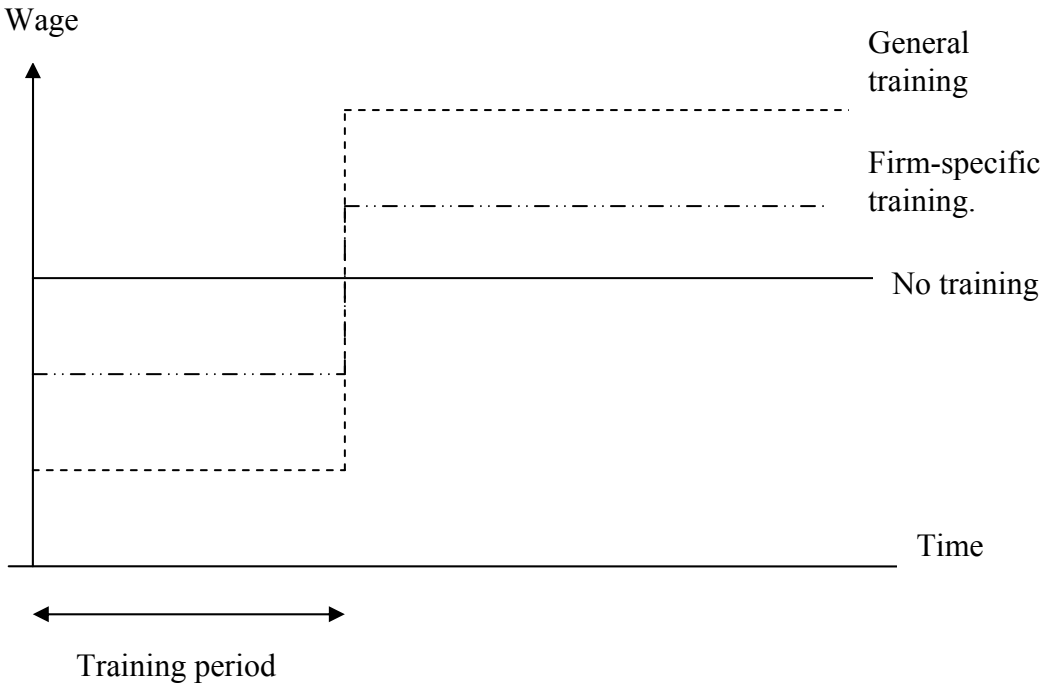
Table D: JRT studies looking at separations and promotions

Author	Data and training definition	Method	Result
Blundell, Dearden & Meghir 1996	UK: NCDS longitudinal survey on 2781 person born in march 1958 and employed 1981. **Training** is non-government work-related training courses min 14 days or 100 hours.	Quasi-difference model (fixed effects and productivity chocks) LS: log(hourly wage1991)-log(hourly wage1981) RS: promotion, tenure, ojt (current and previous), oft (current and previous), other training, qualifications, number of jobchanges, regions, occupation, union membership, employer size, sektor	**Men** + promotion + training (smaller) **Women** + promotion + training (smaller)
Evertsson 2004	Sweden: Swedish Survey of Living Condition 1994-98. 10721 employees (part time and fulltime same employer for 3 years receiving training). **Training** financed or arranged by employer at least 1 week.	Logit (Risk of receiving promotion after training)	**High risk**: men **Low risk:** women from health care industry
Krueger & Rouse 1998	US: Longitudinal data 2 firms 1991-94 (manufacturing 642 employees, service 239 employees) **Training** to low-skilled subsidized by the federal government	Probit (likelihood of turnover) LS: not observed in 1994 or 1995 RS: training, age, tenure, female, nonwhite, married, education, work shift, job bids 1992, job upgrades 1992 Ordered probit (likelihood of jobbids or jobupgrades)	**Manufacturing & Service:** training no significant effect on separations. (but less likely to be laid off) Training participants bid for more jobs and gets more upgrades
Parent 1999	US: NLSY 1979-91 5649 individuals 14-21 in 1978. Employees (at least 6 month within last year) **Training** at least 4 weeks. Training program or ojt designed to improve job skills, help finding a job, or learn a new job.	Hazard model (Cox's partial Likelihood Approach) on spells at employer RS:Previous otj, oft, apprenticeship, experience, tenure, race, gender, marital status, union, metropolitan statistical area urban/rural, health, regional ui, number of jobs, regions. Taking individual heterogeneity into account	**Less likely to leave** when training at current employer. **Increased mobility** when training at previous employer.

Source: Weatherall (2007)

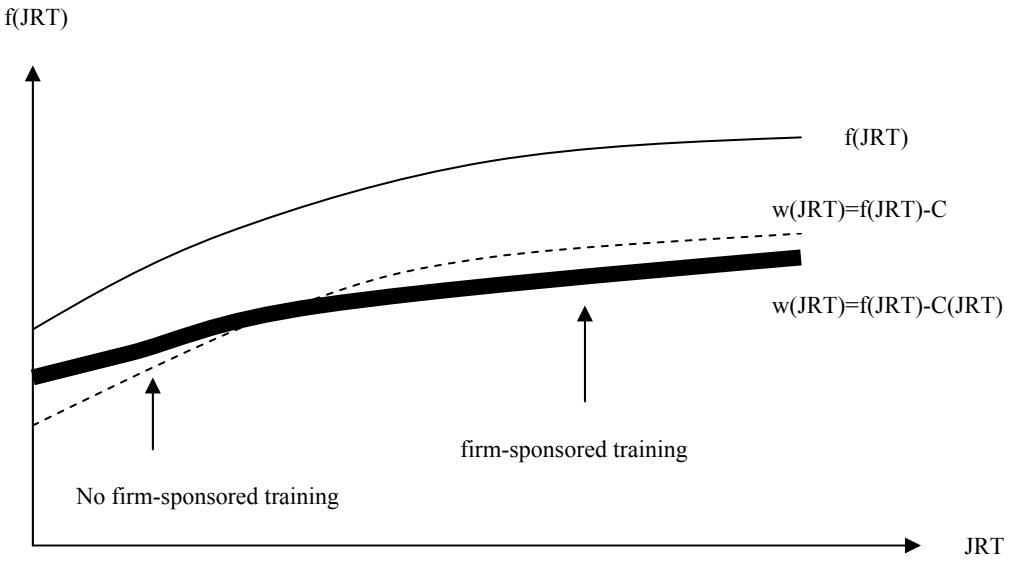
Figures

Figure 1. Illustration of the traditional human capital theory



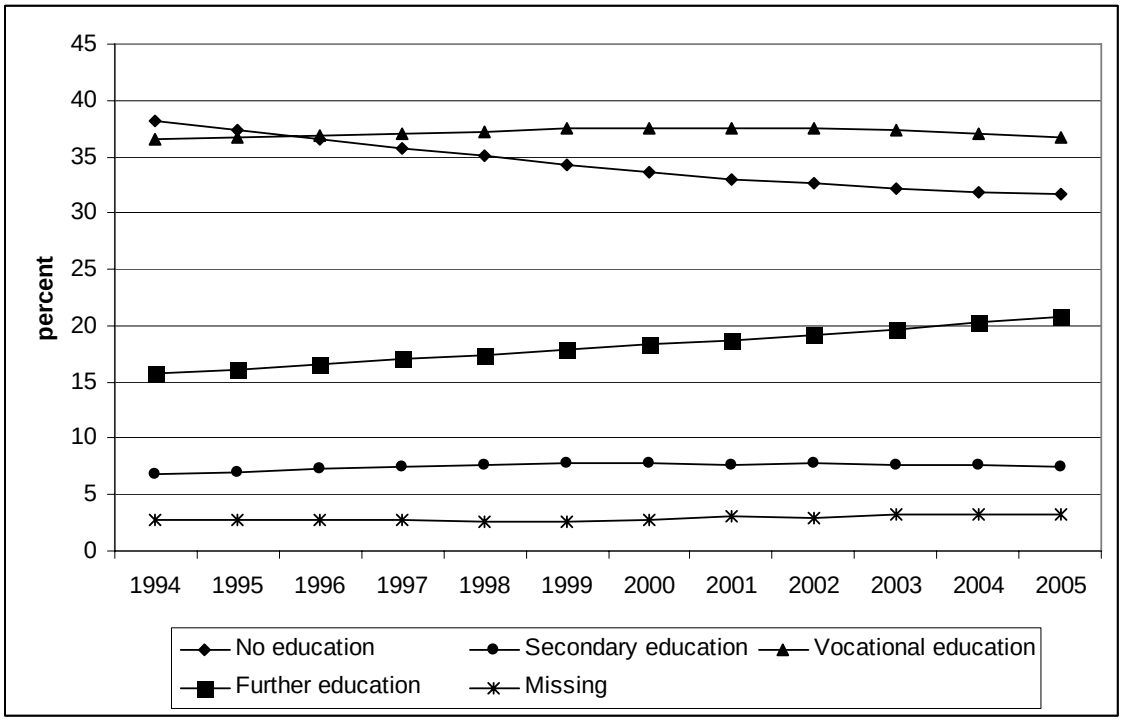
Source: Weatherall(2007)

Figure 2. Illustration of the extended human capital theory



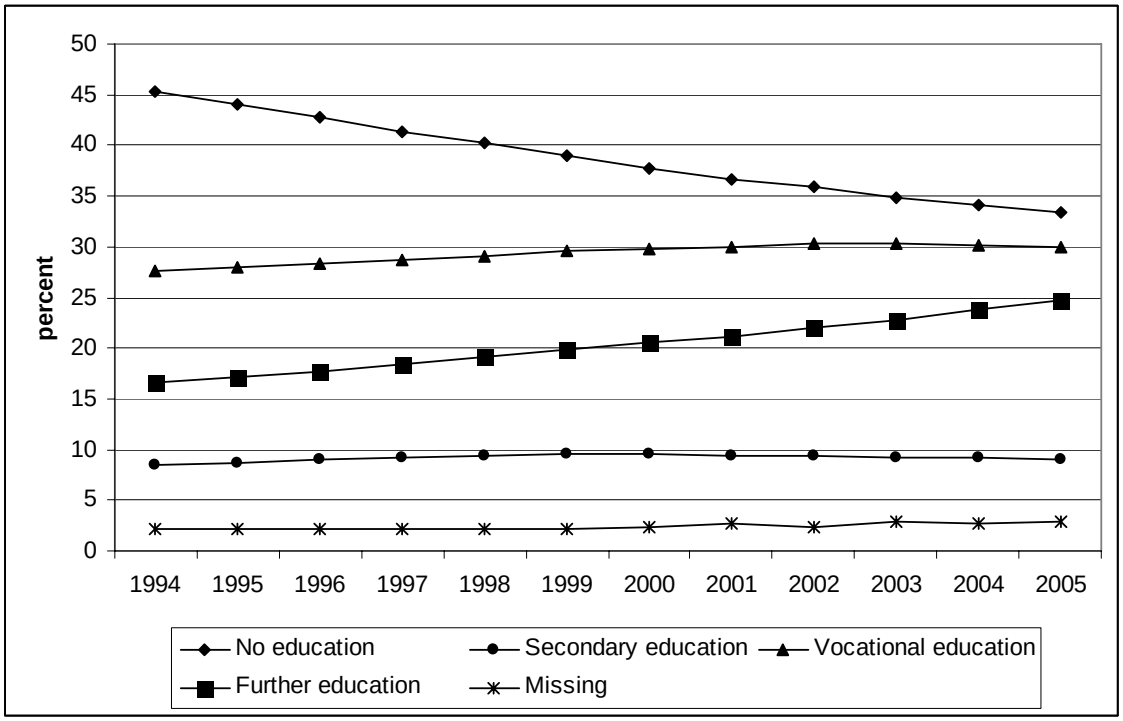
Source: Weatherall(2007)

Figure 3. Highest education among men 15 years of age or more from 1994 to 2005



Source: Statistics Denmark Data Bank (1994-2005)

Figure 4. Highest education among women 15 years of age or more from 1994 to 2005



Source: Statistics Denmark Data Bank (1994-2005)

Tables

Table 1. Comparison of final sample and full sample

	Final sample	Full sample
Percentage receiving JRT in 1995	58	49
Percentage women	45	48
Percentage job separation in 1996	12	29
Average wage in 1996 (DKK)	244917	202886
st.dev	103140	112012
Average wage growth 1994-96(DKK)	20210	23590
st.dev	40855	66597

Source: JRTDS panel data 1994-1996

Table 2. Descriptive statistics on JRT in final sample 1994-1996

		Men			Women		
		Pct	**Wage 96 (DKK)**	**Wage 94-96 (DKK)**	**Pct**	**Wage 96 (DKK)**	**Wage 94-96 (DKK)**
JRT	**0 days**	44,99	263677,2	20365,484	38,11	184322,19	15687,495
			123896,89	44327,828		61855,286	36114,304
	< 1 day	1,17	254816,89	14664,947	2,24	179482,6	12515,7
			64708,272	12243,764		54869,455	37766,373
	1-5 days	24,35	292813,26	23980,647	31,1	205783,62	17948,825
			113663,51	44759,708		55351,672	32786,035
	6-10 days	13,72	295486,5	22264,901	15,06	223334,59	21592,015
			106498,64	49075,23		67459,648	31168,448
	11-20 days	9,15	295353,31	26299,223	7,76	233305,47	14953,288
			111396,82	42296		70210,609	23845,884
	>20 days	6,61	284717,48	25737,009	5,74	233868,45	24225,104
			88374,066	60793,137		54323,109	34320,379
	Total	100	279321,39	22337,475	100	203407,99	17642,433
			116194,87	45949,927		63540,803	33536,226
Education	**Non-educated**	53,5	235493,74	168171,12		20547,346	18626,428
			77085,489	51869,182		43995,881	36235,475
	Non-educated JRT	46,5	264988,62	195114,99	44,82	19930,419	20264,097
			88286,723	54699,992		42453,31	33986,621
	Total	100					
	Voc.educated	50,37	249585,4	187286,25		16916,468	11013,377
			63175,216	62420,006		33574,265	35733,516
	Voc. educated JRT	49,63	275759,34	206015,85	59,46	24383,997	17739,421
			99242,457	60453,471		39621,047	30535,234
	Total	100					
	Fur. educated	26,44	370262,18	228182,41		32678,973	20228,886
			249510,04	68048,953		70415,994	35777,465
	Fur. Educated JRT	73,56	329885,31	234892,36	82,63	25830,304	19087,42
			118955,75	61145,036		57819,705	31776,696
	Total	100					

Source: JRTDS panel data 1994-1996

Table 3. Probit estimations for receiving JRT 1995 and job separations in 1996

	Likelihood of JRT		Likelihood of separation	
	Men	**Women**	**Men**	**Women**
Age	-0.016	-0.017	-0.017	-0.000
	[0.005]***	[0.005]***	[0.006]***	[0.007]
Danish	ref.	ref.	ref.	ref.
Immigrant	0.170	0.204	0.285	-0.086
	[0.236]	[0.249]	[0.273]	[0.322]
Single	ref.	ref.	ref.	ref.
Single parent	0.412	0.104	0.398	0.485
	[0.352]	[0.173]	[0.398]	[0.208]**
Couple	0.119	0.092	0.029	0.131
	[0.099]	[0.113]	[0.118]	[0.157]
Couple and parent	0.141	0.039	-0.020	0.012
	[0.091]	[0.110]	[0.108]	[0.153]
No education				
Vocational education	0.046	0.224	-0.090	0.042
	[0.081]	[0.090]**	[0.103]	[0.128]
Further education	0.330	0.470	0.120	0.089
	[0.113]***	[0.136]***	[0.137]	[0.176]
Unskilled w.e.				
W.e. low pay	-0.044	-5.461	-0.069	-0.091
	[0.170]	[0.463]***	[0.201]	[0.463]
W.e. middle pay	-0.213	-5.763	-0.010	-0.097
	[0.162]	[0.447]***	[0.193]	[0.449]
W.e. high pay	-0.457	-6.050	-0.137	-0.198
	[0.156]***	[0.433]***	[0.190]	[0.452]
Top manager	-0.647	-6.579	-0.372	-0.400
	[0.182]***	[0.440]***	[0.226]*	[0.479]
Experience	0.033	0.011	0.001	-0.023
	[0.013]**	[0.011]	[0.015]	[0.015]
Tenure	-0.003	0.007	-0.016	-0.026
	[0.005]	[0.006]	[0.007]**	[0.008]***
Union member	0.081	0.138	-0.075	0.132
	[0.105]	[0.149]	[0.125]	[0.204]
Private sector				
Municipality/region	0.076	0.009	-0.446	-0.477
	[0.137]	[0.142]	[0.177]**	[0.173]***
State	-0.261	-0.236	-0.264	-0.277
	[0.134]*	[0.170]	[0.165]	[0.208]

continued

Hotel and restaurant				
Manufacturing	0.137	-0.548	-0.602	-0.481
	[0.343]	[0.394]	[0.357]*	[0.458]
Construction	-0.076	-0.524	-0.228	-0.303
	[0.366]	[0.615]	[0.381]	[0.765]
Wholesale	0.606	-0.366	-0.101	-0.527
	[0.350]*	[0.400]	[0.364]	[0.470]
Transportation	0.536	0.108	-0.118	0.284
	[0.359]	[0.430]	[0.377]	[0.488]
Finance	0.703	0.069	0.052	-0.083
	[0.360]*	[0.400]	[0.372]	[0.458]
Service / international org	0.746	0.175	-0.295	-0.058
	[0.358]**	[0.403]	[0.378]	[0.460]
Electricity / agricultural	0.664	-0.063	-0.634	-0.342
	[0.412]	[0.564]	[0.495]	[0.731]
>100 employees				
20-99 employees	-0.305	-0.049	-0.208	-0.148
	[0.077]***	[0.093]	[0.098]**	[0.130]
10-20 employees	-0.481	-0.089	-0.127	-0.001
	[0.113]***	[0.125]	[0.135]	[0.162]
<10 employees	-0.489	-0.158	0.143	0.312
	[0.129]***	[0.128]	[0.147]	[0.155]**
Constant	0.188	6.463	0.420	-0.526
	[0.405]	[0.000]	[0.444]	[0.693]
Observations	1618	1341	1618	1341
log likelihood	-1002.58	-763.53	-626.87	-380.57
Pseudo R2	0.10	0.14	0.08	0.08

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Source: JRTDS panel data 1994-1996

Table 4. Employers opinion on JRT in 1996

		Unskilled	skilled/ low pay	Skilled/ midle pay	Manager/ high paid
Prioritize JRT	**A lot**	31,51	40,78	53,26	56,46
	To an extend	28,93	32,92	32,51	30,23
	Less	21,97	17,83	10,15	9,62
	Not at all	17,59	8,47	4,08	3,69
	Total	100	100	100	100
Ave. JRT days		3,05	4,3	5,25	6,49
Ave. cost JRT	**(DKK)**	1069,09	1743,33	2950,91	4329,41
Ave. wage	**(DKK)**	14158,16	17069,68	18766,8	26950,35
st.dev		2434,42	2950,618	3638,301	7042,225

Source: JRTDS the employer survey 1996

Table 5. OLS estimations on average JRT days in different workplaces 1996

	all	Offering JRT	Having all employee groups represented	Offering JRT and having all employee groups
Private	ref.	ref.	ref.	ref.
Public	0.092	0.068	-0.013	-0.379
	[0.274]	[0.291]	[0.520]	[0.557]
Main office	ref.	ref.	ref.	ref.
Affiliates	0.238	0.401	0.189	0.374
	[0.260]	[0.272]	[0.392]	[0.408]
single firm	-0.756	-0.512	-0.747	-0.415
	[0.257]***	[0.272]*	[0.397]*	[0.416]
Hotel & restaurant	ref.	ref.	ref.	ref.
Manufacture	0.730	0.518	0.384	0.689
	[0.598]	[0.731]	[0.938]	[0.991]
Electricity	1.840	1.586	1.578	2.452
	[1.089]*	[1.201]	[1.431]	[1.532]
Construction	1.495	1.680	2.183	2.566
	[0.704]**	[0.842]**	[1.082]**	[1.144]**
Wholesale	0.115	-0.186	1.047	1.208
	[0.630]	[0.761]	[1.036]	[1.089]
Agricultural	0.252	0.003	0.609	0.557
	[1.008]	[1.177]	[1.749]	[1.763]
Transport	0.996	0.737	0.134	0.629
	[0.671]	[0.811]	[1.086]	[1.169]
Finance	1.322	0.784	0.573	0.652
	[0.840]	[0.951]	[1.933]	[2.059]
Service & intern. org.	1.726	1.188	2.099	2.334
	[0.625]***	[0.757]	[1.039]**	[1.102]**
Other	2.107	1.489	1.718	2.060
	[0.945]**	[1.049]	[1.475]	[1.562]
no. employees	-0.0007	-0.0003	-0.0006	-0.00004
	[0.0003]**	[0.0003]	[0.0004]*	[0.00039]
(no. employees)^2	0.00000007	0.00000002	0.00000006	0.00000001
	[0.00000004]*	[0.00000004]	[0.00000004]	[0.00000005]
% managers	2.546	2.642	-0.603	-0.534
	[0.498]***	[0.534]***	[1.082]	[1.114]
% skilled/midle pay	2.468	2.590	0.660	1.232
	[0.379]***	[0.413]***	[0.746]	[0.782]
% skilled/low pay	2.385	2.532	1.793	2.183
	[0.465]***	[0.515]***	[0.762]**	[0.800]***
Human management	0.438	0.242	0.535	0.385
	[0.058]***	[0.065]***	[0.101]***	[0.111]***
Good economy	0.115	0.078	0.125	0.018
	[0.087]	[0.094]	[0.137]	[0.146]
Constant	0.518	2.060	1.261	1.995
	[0.711]	[0.873]**	[1.223]	[1.301]
Observations	2883	2467	1056	931
R-squared	0.08	0.05	0.06	0.05

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%
Source: JRTDS the employer survey 1996

Table 6. OLS estimations on average JRT days in workplaces offering JRT within different employment groups 1996

	Manager/ high pay	Skilled/ middle pay	Skilled/ low pay	Unskilled
Private	ref.	ref.	ref.	ref.
Public	0.162	0.313	0.137	0.210
	[0.326]	[0.305]	[0.461]	[0.363]
Main office	ref.	ref.	ref.	ref.
Affiliates	0.298	0.268	0.170	-0.017
	[0.293]	[0.281]	[0.370]	[0.317]
single firm	-0.670	-0.767	-0.758	-1.004
	[0.293]**	[0.280]***	[0.363]**	[0.312]***
Hotel & restaurant	ref.	ref.	ref.	ref.
Manufacture	0.339	0.496	0.536	0.439
	[0.852]	[0.736]	[0.738]	[0.640]
Electricity	1.598	1.885	1.186	1.368
	[1.293]	[1.237]	[1.263]	[1.220]
Construction	1.628	1.604	1.345	1.623
	[0.979]*	[0.863]*	[0.831]	[0.804]**
Wholesale	-0.082	0.092	0.458	0.093
	[0.885]	[0.767]	[0.823]	[0.690]
Agricultural	0.687	0.042	0.366	0.159
	[1.338]	[1.293]	[1.223]	[1.117]
Transport	0.738	0.858	0.446	0.373
	[0.930]	[0.813]	[0.890]	[0.743]
Finance	0.793	1.223	0.239	1.580
	[1.082]	[0.972]	[2.129]	[1.411]
Service & intern. org.	1.614	1.719	1.600	1.803
	[0.884]*	[0.766]**	[0.838]*	[0.697]***
Other	2.160	2.144	2.414	1.876
	[1.212]*	[1.063]**	[1.401]*	[1.154]
no. employees	-0.0010	-0.0007	-0.0006	-0.0006
	[0.0003]***	[0.0003]**	[0.0004]	[0.0004]*
(no. employees)^2	0.00000010	0.00000006	0.00000006	0.00000006
	[0.00000004]**	[0.00000004]	[0.00000004]	[0.00000004]
% managers	2.273	1.899	2.118	1.334
	[0.585]***	[0.578]***	[0.928]**	[0.696]*
% skilled/midle pay	2.476	2.098	2.117	2.128
	[0.472]***	[0.446]***	[0.630]***	[0.485]***
% skilled/low pay	2.688	2.444	2.841	2.110
	[0.593]***	[0.562]***	[0.595]***	[0.584]***
Human management	0.382	0.367	0.594	0.395
	[0.070]***	[0.066]***	[0.086]***	[0.073]***
Good economy	0.063	0.131	0.068	0.162
	[0.101]	[0.096]	[0.121]	[0.105]

continued				
Wage manager/high pay	0.00004			
	[0.00002]**			
Wage skilled/middle pay		0.00009		
		[0.00003]***		
Wage skilled/ low pay			-0.00003	
			[0.00004]	
Wage unskilled				0.00006
				[0.00004]
Constant	0.18652	-0.68938	0.81076	0.19155
	[1.04836]	[0.99926]	[1.20127]	[1.00777]
Observations	2283	2434	1332	1915
R-squared	0.07	0.07	0.08	0.08

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Source: JRTDS the employer survey 1996

Table 7. JRT and separations among employees in final sample from 1994-96

	Men				Women			
	%	**% JRT**	**Wage 96 (DKK)**	**Wage 94-96 (DKK)**	**%**	**% JRT**	**Wage 96 (DKK)**	**Wage 94-96 (DKK)**
Non- educ.	85,25	46,62	250209,49	19519,114	91,08	45,5	180522,62	18518,659
			86092,191	42037,418			55122,702	33740,326
Non- educ.	14,75	45,76	243425,56	24545,288	8,92	37,84	177433,05	27960,027
Separate			68467,615	49766,929			51537,641	47477,672
Voc.educ.	86,16	47,9	262276,49	19378,658	90,63	58,86	196494,2	14874,951
			80962,507	36049,251			59507,897	30183,872
Voc. Educ.	13,84	60,36	264429,38	28364,243	9,37	65,31	217089,06	16348,347
Separate			101466,61	40928,476			79771,496	52666,235
Fur. Educ.	82,45	73,18	342038,97	22206,397	90,57	82,19	233229,33	18410,079
			173227,98	50873,189			62689,317	31395,074
Fur. Educ.	17,55	75,34	333621,62	53177,616	9,43	86,84	238505,79	27696,158
Separate			115869,41	92874,934			59709,45	40899,471

Source: JRTDS panel data 1994-1996

Table 8 Estimation of wage growth among men in final sample 1994-96

Method	**Ols**	**ols**	**ols**	**2sls**	**2sls**
Endogenous				**separate**	**separate JRT*sep**
Instrument				**js1991 js1991^2**	**js1991 js1991^2 JRT*js1991 JRT*js1991^2**
No education					
JRT	-0,023	-0,024	-0,024	-0,019	0,402
	[0.022]	[0.022]	[0.022]	[0.057]	[0.375]
separation		0,042	0,041	1,508	2,661
		[0.040]	[0.060]	[1.006]#	[2.289]
local ui		0,006	0,006	0,012	-0,025
		[0.007]	[0.007]	[0.026]	[0.043]
JRT*separation			0,002		-2,812
			[0.077]		[2.431]
Constant	0,111	0,124	0,124	-0,075	-0,372
	[0.017]***	[0.031]***	[0.030]***	[0.165]	[0.442]
Observations	400	400	400	400	400
R2	0	0,01	0,01	-5,47	-9,49
Vocational education					
JRT	0,031	0,031	0,029	0,024	0,066
	[0.015]**	[0.015]**	[0.016]*	[0.017]#	[0.046]#
separation		0,026	0,018	0,137	0,353
		[0.019]#	[0.026]	[0.144]	[0.318]
local ui		-0,016	-0,016	-0,016	-0,015
		[0.009]*	[0.009]*	[0.007]**	[0.007]**
JRT*separation			0,013		-0,326
			[0.037]		[0.354]
Constant	0,068	0,012	0,013	-0,001	-0,02
	[0.008]***	[0.031]	[0.031]	[0.031]	[0.041]
Observations	802	802	802	802	802
R2	0,01	0,01	0,01	-0,02	-0,11
Further education					
JRT	-0,014	-0,016	-0,006	-0,014	-0,03
	[0.029]	[0.028]	[0.028]	[0.025]	[0.090]
separation		0,103	0,146	0,004	-0,149
		[0.037]***	[0.092]#	[0.455]	[0.396]
local ui		0,005	0,005	0,002	-0,001
		[0.013]	[0.012]	[0.015]	[0.012]
JRT*separation			-0,057		0,101
			[0.101]		[0.508]
Constant	0,102	0,099	0,093	0,107	0,124
	[0.027]***	[0.036]***	[0.039]**	[0.050]**	[0.065]*
Observations	416	416	416	416	416
R2	0	0,03	0,04	0	-0,08

Robust standard errors in brackets

significant at 20%; * significant at 10%; ** significant at 5%; *** significant at 1%

Source: JRTDS panel data 1994-1996

Table 9. Estimation of wage growth among women in final sample 1994-96

Method	**ols**	**ols**	**ols**	**2sls**	**2sls**
Endogenous				**separate**	**separate JRT*sep**
Instrument				**js1991 js1991^2**	**js1991 js1991^2 JRT*js1991 JRT*js1991^2**
No education					
JRT	-0,03	-0,031	-0,027	-0,019	0,074
	[0.033]	[0.033]	[0.035]	[0.038]	[0.139]
separation		0,066	0,086	0,567	0,784
		[0.064]	[0.094]	[0.435]#	[0.395]**
local ui		0,032	0,032	0,041	0,045
		[0.017]*	[0.017]*	[0.021]*	[0.022]**
JRT*separation			-0,051		-1,177
			[0.116]		[1.749]
Constant	0,147	0,248	0,248	0,231	0,221
	[0.026]***	[0.065]***	[0.065]***	[0.073]***	[0.075]***
Observations	415	415	415	415	415
R2	0	0,01	0,01	-0,16	-0,24
Vocational education					
JRT	0,04	0,038	0,023	0,016	0,031
	[0.017]**	[0.017]**	[0.017]#	[0.039]	[0.126]
separation		0,021	-0,088	1,083	0,437
		[0.044]	[0.075]	[1.046]	[0.752]
local ui		-0,011	-0,012	0,011	-0,003
		[0.009]	[0.009]#	[0.028]	[0.017]
JRT*separation			0,168		-0,009
			[0.091]*		[1.306]
Constant	0,062	0,022	0,029	0,011	0,018
	[0.012]***	[0.034]	[0.034]	[0.064]	[0.062]
Observations	523	523	523	523	523
R2	0,01	0,01	0,03	-2,37	-0,34
Further education					
JRT	-0,01	-0,011	-0,015	-0,019	-0,039
	[0.022]	[0.022]	[0.024]	[0.026]	[0.064]
separation		0,041	0	0,31	0,186
		[0.035]	[0.055]	[0.311]	[0.363]
local ui		0,012	0,012	0,018	0,02
		[0.011]	[0.011]	[0.012]#	[0.014]#
JRT*separation			0,047		0,24
			[0.068]		[0.648]
Constant	0,097	0,136	0,138	0,135	0,149
	[0.020]***	[0.039]***	[0.039]***	[0.040]***	[0.058]**
Observations	403	403	403	403	403
R2	0	0,01	0,01	-0,21	-0,38

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Source: JRTDS panel data 1994-1996

Table 10. Test of instruments and endogeneity of job separations on wage return

	No education			
	Men		Women	
Method	2sls	2sls	2sls	2sls
Endogenous	Separate	separate JRT*sep	separate	separate JRT*sep
Instrument	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2
Observations	400	400	415	415
R2 (first stage)	0,007	0,015 ; 0,114	0,028	0,036 ; 0,047
F (df)	[2, 395]	[4, 393]	[2, 410]	[4, 408]
F-value	[1,26]	[1,49 ; 2,94]	[4,63]	[3,17 ; 0,41]
Sargan				
(χ2 df)	[1]	[2]	[1]	[2]
P(Sargan)	0,78	0,77	0,7	0,88
Du-Wu-Ha				
(χ2 df)	[1]	[2]	[1]	[2]
P(Du-Wu-Ha)	0	0	0,205	0,12
	Vocational education			
	Men		Women	
Method	2sls	2sls	2sls	2sls
Endogenous	Separate	separate JRT*sep	separate	separate JRT*sep
Instrument	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2
Observations	802	802	523	523
R2 (first stage)	0,03	0,033 ; 0,125	0,008	0,009 ; 0,048
F (df)	[2, 797]	[4, 795]	[2, 518]	[4, 516]
F-value	[9,13]	[5,22 ; 7,22]	[0,73]	[0,44 ; 0,23]
Sargan				
(χ2 df)	[1]	[2]	[1]	[2]
P(Sargan)	0,56	0,7	0,06	0
Du-Wu-Ha				
(χ2 df)	[1]	[2]	[1]	[2]
P(Du-Wu-Ha)	0,425	0,527	0,059	0,648
	Further education			
	Men		Women	
Method	2sls	2sls	2sls	2sls
Endogenous	separate	separate JRT*sep	separate	separate JRT*sep
Instrument	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2	js1991 js1991^2	js1991 js1991^2 JRT*js1991 JRT*js1991^2
Observations	416	416	403	403
R2 (first stage)	0,011	0,019 ; 0,067	0,016	0,019 ; 0,027
F (df)	[2, 411]	[2, 409]	[2, 398]	[4, 396]
F-value	[0,77]	[1,21 ; 0,97]	[2,06]	[1,33 ; 0,48]
Sargan				
(χ2 df)	[1]	[2]	[1]	[2]
P(Sargan)	0,21	0,4	0,78	0,9
Du-Wu-Ha				
(χ2 df)	[1]	[2]	[1]	[2]
P(Du-Wu-Ha)	0,822	0,676	0,334	0,553

Source: JRTDS panel data 1994-1996

Table 11. Estimation of wage growth among men in final sample 1994-96

	No education	Vocational education	Further education
0 JRT	ref.	ref.	ref.
< 1 day	-0.077	0.013	-0.032
	[0.026]***	[0.019]	[0.040]
1-5 days	-0.024	0.047	-0.015
	[0.028]	[0.030]+	[0.030]
6-10 days	-0.006	0.024	-0.009
	[0.035]	[0.014]*	[0.036]
11-20 days	-0.053	0.013	0.023
	[0.028]*	[0.016]	[0.036]
> 20 days	-0.014	-0.001	-0.006
	[0.042]	[0.018]	[0.046]
Job separation	0.041	0.018	0.146
	[0.060]	[0.026]	[0.092]
Regional ui	0.006	-0.016	0.005
	[0.007]	[0.009]*	[0.013]
JRT* job separation	0.004	0.016	-0.055
	[0.078]	[0.037]	[0.101]
Constant	0.125	0.011	0.094
	[0.030]***	[0.031]	[0.039]**
Observations	400	802	416
R2	0.01	0.02	0.04

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1% + significant at 16%

Source: JRTDS panel data 1994-1996

Table 12. Estimation of wage growth among women in final sample 1994-96

	No education	Vocational education	Further education
0 JRT	ref.	ref.	ref.
< 1 day	-0.276	0.088	0.069
	[0.153]*	[0.100]	[0.092]
1-5 days	-0.048	0.035	-0.019
	[0.040]	[0.021]*	[0.027]
6-10 days	0.061	-0.011	-0.010
	[0.051]	[0.024]	[0.026]
11-20 days	-0.008	-0.002	-0.037
	[0.048]	[0.026]	[0.026]+
> 20 days	-0.023	0.053	-0.001
	[0.052]	[0.053]	[0.028]
Job separation	0.086	-0.088	0.000
	[0.095]	[0.075]	[0.056]
Regional ui	0.031	-0.013	0.013
	[0.016]*	[0.009]	[0.011]
JRT* job separation	-0.070	0.163	0.046
	[0.114]	[0.089]*	[0.068]
Constant	0.243	0.027	0.140
	[0.063]***	[0.033]	[0.039]***
Observations	415	523	403
R2	0.03	0.04	0.02

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1% + significant at 16%

Source: JRTDS panel data 1994-1996

Chapter 3
Does subsidized adult apprenticeship improve the aggregate level of education?

Abstract

Denmark introduced a very generous apprenticeship subsidy for adults over 25 years of age in 1997 to address the challenges of globalization and the increased demand for skills. The aim of the adult apprenticeship subsidy (AAS) was to increase vocational skills levels among the non-educated in order to fill job vacancies (i.e. prevent bottlenecks). The purpose of this paper is to evaluate the effect of the AAS on the attendance rate into vocational education from 1996 to 2003. Through a simple theoretical human capital model, I show that AAS is likely to influence education decisions in the whole population. Additionally, a simulation of the model illustrates the difficulties of finding an empirical strategy capable of evaluating the effect of a subsidy in the absence of an obvious control group. This paper empirically examines the effect of the subsidy, given the exogeneous shift in AAS in 1997, among the unskilled by the difference-in-difference estimator used in international educational evaluation studies on a rich panel data. The results show that the AAS has a significant positive effect on the vocational attendance rate among 25-year-old men in 1998. However 25-year-old unskilled women were not affected by the subsidy. Additionally, the AAS has no significant effect on the attendance rate after 1998. Thus, the results do not unambiguously find that a generous AAS increases the attendance rate among the non-educated.

1. Introduction

Due to increased globalization and international competition, developed countries compete by upgrading the skills of their labor force (OECD 2006). One method of development is to upgrade skills through adult apprenticeship programs for non-educated adults, a particularly vulnerable group with high unemployment risk. The focus of this paper is on skill upgrading through an adult apprenticeship subsidy (AAS).

Denmark and many other OECD countries have initiated many programs to increase the education level in the population. In contrast to other countries nearly all people involved in vocational or further education in Denmark are entitled to a very generous subsidy or wage. Furthermore vocational education in Denmark is mainly obtained through an apprenticeship. The apprenticeship program is a common way of receiving vocational skills in Denmark, Germany, Austria and Switzerland. On the other hand, in countries such as the UK, the US and various EU countries, apprenticeship programs play a rather small role.

The Danish AAS was introduced in 1997. To my knowledge, no other OECD country has ever introduced such a generous AAS. The AAS increase on average an apprentice's income by more than 30 percent. The Danish AAS is offered mainly to unskilled people over 25 years old, independent of their family background or income, so that they can receive a vocational education, which helps prevent bottlenecks in the labor market. An AAS is only available to people starting an apprenticeship in an industry listed as a "bottleneck" industry by the regional labor market board. In all regions (see sample list in appendix A) vocational fields with a high proportion of female students (e.g. office and trade, food and domestic production and healthcare) rarely make the list. This paper conducts separate empirical analyses for women and men because of the clear gender differences in potential subsidy areas.

The history of the AAS program is that the subsidy became permanent after a successful one-year tryout period. Figure 1 shows that, except for 1997, at least 2500 persons start an apprenticeship with an AAS every year.[25] About 10 percent of all people over 25 in an apprenticeship position receive AAS. More than 70 percent of all subsidized apprenticeships in 1997 and 1998 are men. Although, the gender distribution has become more balanced over the years, in 2004 men still comprise the majority.

[25] A late approval date in 1997 may explain the highest number of new starters in 1998, as section 2 will discuss in detail.

Moreover, Figure 2 shows that the overall number and share of new apprentices increased slightly in 1997-1999. That a reasonable share of the apprenticeships were subsidized and that the number of people starting apprenticeships increased in 1997 and 1998 led the authorities to claim that the AAS was a success (PLS Consult, 1999).

This paper questions the "success claim" after 1998 by examining whether the AAS increases apprenticeships among people over 25 years old at the expense of apprenticeships among people under 25 years old. Figure 3 supports the claim by showing that the apprenticeship attendance rate among unskilled men increases for 25-year-olds but not for 24-year-olds in 1998, the year in which the subsidy was expected to be fully introduced. Figure 4 shows that age does not appear to influence the attendance rate for 24-year-old and 25-year-old women to the same extent as for men.

Another obvious question to ask is whether it is a 'success' if the AAS made people who would have continued their academic education change to a vocational education instead. By simulating a simple human capital model with and without an AAS, this paper shows the relevance of both questions. The simulation results can also illustrate the complications arising from an empirical evaluation of a subsidy, given the lack of a perfect or obvious control group.

Previous empirical studies have evaluated the incentives and the returns to apprenticeships in Germany (Harhoff and Kane 1997), the Netherlands (Smits and Zwick 2004), Austria (Soshice 1994), Switzerland (Wolter and Mühlemann 2006), the US and the UK (Elbaum and Singh 1995). However, due to the non-existence of AAS in other countries, no studies have ever evaluated the effect of an AAS on the enrolment into vocational education. Instead, several international studies have looked at the effect of subsidies to college students. In a Danish study an increase in student aid increases the demand for college, but the increase is a lot less than found in other studies (Nielsen et al 2006). For example, US studies show that generous college subsidies to disadvantaged families increase enrolment into colleges significantly (Dynarski 1999, Manski & Wise 1983, Angrist 1993). These positive results of educational subsidies on college enrolment suggest that a generous AAS would increase the enrolment into vocational training. Introducing an AAS is expected to influence not only the individual's choice of skill level but also government spending, income and wage

distribution, which should also indirectly affect firm demand for labor. [26] However, this paper will only empirically evaluate if the AAS initiative increases the attendance rate of vocational education among non-educated adults (i.e. the direct effect).

The empirical analysis applies the different eligibilities among age groups and the introduction of the subsidy in 1997 on a rich Danish register panel from 1995 to 2004. The age-specific eligibility means that an AAS applicant has to be at least 25 years old. Groups above and below 25 years of age are represented in the Danish data. The AAS was first available in 1997, and the Danish data includes information on people receiving AAS and people not receiving AAS before and after 1997. Combining the facts and the available data makes it possible to identify the effect of AAS, by comparing the unskilled 25-year-olds who are eligible for an AAS with the non-eligible unskilled 24-year-olds from 1996 to 1998. One could argue that comparing all age groups above and below 25 years of age would capture the AAS effect better. However, doing so would jeopardize the identification strategy because the 20-year-olds are not obviously comparable to the 30-year-olds. Thus the empirical analysis focuses on the subsidy effect among the group of unskilled (who have delayed their studies), who are suddenly exposed to the possibility of a subsidy. Therefore the results revolve around an empirical partial analysis and should not be confused with the total effect of an AAS.

Countries such as the US, the UK and Germany treat apprenticeship programs as a good way of improving the skills of the non-educated. The question is if an AAS is the best way to improve vocational skills among the non-educated adults. As the results of this paper's empirical AAS evaluation are vital to the evidence on whether or not the AAS works in Denmark, this paper contributes to the international public debate on using subsidies to improve vocational skills in developed countries for the purpose of increasing future employment.

The paper is set up as follows: Section 2 describes Danish students financing possibilities when they take vocational or further education (i.e. short and long continued academic pursuits). Section 3 uses an extended theoretical human capital model to illustrate the effect of an AAS. In section 4, I simulate the theoretical model to

[26] The literature shows that introducing a subsidy can be optimal for society. Suppose individuals have less information about the future than the government and therefore individuals find it risky to start an education. Because the government has more information it intervenes (e.g. subsidy to education). The intervention removes individual uncertainty indirectly. Thus the individuals make educational choices that are optimal for society (Manski 1989; Dynarski 1999)

show that a subsidy influences all people's education decisions and that a proper control group is difficult to find. Section 5 discusses the implications of using the difference-in-differences estimator as a possible empirical strategy. Section 6 describes the rich Danish register panel data. The empirical results of the effect of an AAS on the attendance rate into vocational education are analysed in section 7. Section 8 concludes.

2. Institutional framework

This section describes the generous Danish educational system showing that non-educated people in Denmark have several financially supported education options for increasing skills. I put different educational financing possibilities into perspective through an example comparing a carpenter in a traditional apprenticeship with a carpenter in an apprenticeship with AAS and an economist taking further education.

In Denmark the share of skilled adults has increased drastically over the last decade. In 1995 around 60 percent of the adult population between 25-64 years of age had an upper secondary education and about 25 percent of the population between 25-34 years of age had no education (OECD 1997). By 1999 the share of Danish adults with an upper secondary education increased to 80 percent, while less than 15 percent of these 25 to 34 years of age had no education (OECD 2001). Thus, Denmark became part of a select group of countries (the Czech Republic, Germany, Japan, Norway, Switzerland and the US) with the highest skilled adult population. The higher skill level in the Danish population is a result of more people taking vocational and further education. In 1997 about 118.000 were registered in vocational education (including apprentices and apprentices with an AAS) and 173.000 students were taking a short or long term further education. In 2004, vocational education and further education increased to about 121.000 and 202.000, respectively (Statistical Yearbook 2006).

Although Denmark didn't have the highest skilled adult population in 1995, the government spent more than 6,5 percent of GDP on educational institutions. The share of government spending was one of the highest among OECD countries and still is today (OECD 2006). Furthermore, expenditure per student increased by 10 percent simultaneously with the increase in skill level among the Danish adult population from 1995-1999.

Overall, the Danish educational system is very generous. Tuition at Danish public and most private educational institutions is free for Danish students and for all EU/EEA[27] students, as well as for students participating in exchange programmes.[28] Nearly all trainees and students receive either a wage decided by a union agreement or a student state grant at some point during their education. Although there are multiple education possibilities in Denmark, for simplicity this paper will categorize education in three groups chosen to illustrate the differences in individual opportunities for financing education: apprenticeship (vocational education), apprenticeship with an AAS (vocational education) and further education.

The normal procedure for starting an apprenticeship is to apply at a vocational institution. For some education types the enrollment acceptance rate is every 5 weeks; for other types it is 10 weeks, depending on the availability. The education is divided in two parts. The first part is the introduction, consisting of course work, which lasts for a maximum of 20 weeks. The second part, the main education, takes place primarily at a workplace (if the apprentice finds a spot) and for a short period every year in an educational institution. An apprenticeship takes 3,5 years on average. Depending on the vocational field, the shortest education period is 2,5 years and the longest is 5,5 years.[29] The workplace pays the apprentice a wage agreed upon by the unions. When the apprentice is at school, although he or she still receives a wage, the employer is reimbursed from the Employers Reimbursement Fund during the schooling period. The wage increases dramatically over the education period even though the wage normally does not reach the minimum wage level.

Although the traditional apprenticeship system has been functioning for many years, subsidized adult apprenticeship only began in 1997. As mentioned earlier, AAS is offered by the authorities primarily to unskilled unemployed and employed people over 25 years old who want to take a vocational education in a bottleneck industry. The idea is to subsidize the employers so that the apprentices receive a regular wage, not a student wage. The aim is to create a win-win situation in which the employer gets a more qualified employee and the employee receives better future wages

[27] European Economic Area
[28] From 2006 all other students have to pay a tuition fee
[29] Including the basic education that takes between 5 and 20 weeks

and employment possibilities − with neither the employer nor the student suffering economically.[30]

In 1997, the first AAS to an employer hiring a previously unemployed apprentice was 40 DKK (Danish kroner) per hour. The AAS for hiring employed apprentices was 35 DKK per hour. The employer received the AAS for the first 2.5 years of employing the apprentice. Furthermore, the employer received a higher reimbursement when the subsidized apprentice attended school than when an ordinary apprentice attended school. In 1998 and 1999 minor changes were made to the regulation about complaints and details in the wage bargaining agreement. In 2003, the AAS was changed so that all employers received a wage subsidy of 35 DKK an hour no matter which kind of apprentice they employed. In 2005 a minor change mandated the Danish Labor Market Board to pinpoint the bottleneck industries.

An apprentice with or without an AAS obviously is very dependent on the current labor market situation and the availability of apprenticeship openings. An AAS application may possibly be rejected even if a workplace offers an apprentice a spot in a bottleneck industry. Table 8 shows that even though certain educational fields were specified as bottleneck areas in Greater Copenhagen in 2004, some of the apprentices did not have an AAS in these fields. The reason why not all apprentices over 25 years of age in bottleneck industries receive an AAS is that the local labor market authorities evaluate each individual application at the time they receive it. Thus, the labor market situation in which an apprentice negotiates a contract with a workplace might differ from when the authorities evaluate an application. Additionally, if someone in an apprenticeship program has no workplace connection in certain bottleneck industries, AAS application will be denied. Despite these rejections, however the majority of applications are accepted (e.g. Greater Copenhagen Area accepted about 65 percent of the applications between 1997 and 2004).

Although the AAS was a new initiative in 1997, in the early 1990s the unskilled workers unions were already encouraging their members to start an apprenticeship even without a subsidy. The motivation derived from the fact that a lot of members were working as unskilled workers in fields that were transforming into skilled labor occupations. Thus, the unions helped their members analyze their abilities

[30] The consequences for the state are debatable (i.e. it is not necessarily a socially optimal solution to subsidize apprenticeships). The debate is not included in this paper.

and skills, and found ways to help them begin further vocational education in the workplace. Additionally, the unions helped their members negotiate a reasonable wage during their studies. The unions also worked to get the employees' work experience to count in the education and thus shorten the educational period. Due to the union efforts, even in the early 1990s, the attendance rate among older people with work experience was expected to increase. But these union initiatives didn't stop in 1997. Therefore, one might think that these initiatives, and not the AAS, caused the increase in the vocational education attendance rate for people over 25 years old. If this is true, then the overall effect of an AAS would be overestimated. However, because the union initiatives have no age restriction, I expect that both 24-year-olds (not eligible for an AAS) and 25-year-olds (eligible for an AAS) are affected equally by these initiatives.

In early 1996, the unions knew about the upcoming AAS reform. They heavily promoted the reform to their members and to workplaces. Additionally, the unemployment offices informed the unemployed about the introduction of the new AAS. Thus workplaces, the unskilled employed, and the unemployed were well informed about the generous AAS. Although all the people eligible for a subsidy appeared well informed, there still existed a delay in applications and acceptances. For example, the Greater Copenhagen Area authority received its first application on April 21, 1997, and the application was not approved until June 19, 1997. Furthermore, the area received nearly four times as many applications in 1998 as in 1997. This paper evaluates the effect of AAS in 1998 because it is the first year with no obvious application and approval delays.

Compared to the apprenticeship system (with and without AAS) the structure and the financing of further education are very different. First of all, students apply once a year for further education and the enrolment occurs either once or twice a year. Every Danish resident over the age of 18 is entitled to public support for his or her further education. The support for students' living costs is awarded by the State Educational Grant and Loan Scheme. The subsidy system is managed by the State Educational Grant and Loan Scheme Agency in collaboration with educational institutions and is under the supervision of the Danish Ministry of Education.[31] Every student enrolled in a higher education course is entitled to a number of monthly grants

[31] The annual budget amounts to over 11 billion DKK, around 0.8 per cent of GNP.

corresponding to the prescribed duration of the chosen study, plus 12 months.[32] In combination with student grants, students are offered supplementary state loans with very favourable interest rates.[33]

To illustrate the financial differences among educations, I compare the financial situation of a Danish carpenter apprentice with and without an AAS and a Danish economics student (see table 2). Each column represents an average apprentice or student. The example in Table 2 shows that it is very beneficial for a person to postpone apprenticeship until he or she is 25. The reason is that taking an apprenticeship with an AAS increases the apprentice's pay for the entire education period to nearly 40 percent. Compared to the economics student, the apprentice with an AAS earns double in a shorter period. Thus, the carpenter apprentice with an AAS is financially better off than a carpenter apprentice without AAS or an economics student. In this example, even the employer is financially better off by hiring a carpenter apprentice with an AAS rather than a traditional apprentice. Furthermore, the employer might see an advantage in having a more mature apprentice (more than 25 years of age) who can finish his or her education more quickly due to work experience.

Overall, this example shows that during an education period an apprenticeship with an AAS is beneficial for the apprentice and his or her employer. However, choosing an education demands taking three other factors into account. First, the return on education is important. The example makes clear that the starting wage for an economist is dramatically higher than the starting wage for a carpenter. Second, the opportunity cost of postponing education due to the AAS can be quite extensive and can reduce life time earnings. Third, the personal cost of becoming a carpenter instead of an economist might be very extensive depending on a person's aptitudes. Taking a choice against one's aptitudes could reduce the lifetime earnings of a carpenter. The financial and personal costs and the lifetime earnings are included in the theoretical human capital model that follows.

[32] Within a maximum of 70 monthly grants students can change from one course to another. Further extension is possible due to sickness or childbirth. Students living at home with their parents or working extensively have reduced grants.

[33] The grants take up 2/3 of the total support and loans 1/3. The interest rate for the loans is set by Parliament. Students must start paying back state loans no later than one year after the end of the year in which they graduate or give up their studies. The loan must be repaid within 15 years. About half of all students make use of state loans.

3. The theoretical model for subsidized adult apprenticeship

By extending the traditional human capital theory (Becker 1962), this section shows the expected theoretical effect of introducing an AAS in Denmark. In Becker's traditional human capital framework, an individual maximizes discounted lifetime earnings net of schooling cost. Then the optimal choice of schooling occurs when the marginal cost of schooling equals the marginal benefit of schooling. Introducing a schooling subsidy reduces the cost of schooling, thereby increasing the demand for schooling.

Extending the human capital framework by introducing a subsidy to only one type of education and not to others makes the schooling decision framework more complicated. The basis for the model is that an AAS is given to non-educated individuals over 25 years of age taking an apprenticeship. The theoretical model assumes that individuals have information about the AAS from day one, meaning that they decide on education paths from day one.[34] Thus the model is static. Furthermore, all workers are assumed to receive jobs that match their skills. Although, Albrecht et al. (2006) point out that assuming all workers can get jobs is not realistic. Therefore this model is applied strictly for illustrative purposes. Thus, no need exists here for complicating the set-up of the model.

In this extended model, a person's life is divided into six discrete time periods that fit the real education-work life decision framework (i.e. t=1 18-24 years of age; t=2 25-31 years of age; t=3 32-38 years of age; t=4 39-45 years of age; t=5 46-52 years of age; t=6 53-59 years of age). The assumption is that a person who takes an education in the first time period of his or her career has five time periods in which to receive the return of the educational skills he or she obtained in the first period. If the person instead takes an education in the second time period, it is assumed that he or she has only four time periods in which to receive the return of the obtained educational skills. More specifically, the assumption is that the first period, $t = 1$, it is possible to work or study but without an AAS. In the second period, $t = 2$, it is possible to work or study and receive an AAS while being an apprentice. From the third to the sixth period, $t = 3,4,5,6$ it is only possible to work. Assuming at least four periods of work following

[34] This assumption is in contrast to recent dynamic human capital literature, which includes dynamics in the wage setting (i.e. the schooling decisions change over the lifecycle) (Wolpin & Keane 1997).

education is necessary in a model for the Danish labor market with high unskilled wages, because else no one would study.

Individual i can choose among five occupational alternatives: take an apprenticeship, s_{vs}, attend further education, s_{fs}, work as an unskilled employee, e_{ns}, work as an employee with vocational skills, e_{vs}, or work in high-skilled job, e_{fs}.[35] It is not possible to combine the different occupation alternatives in same time period, and not all alternatives are available at each period. Diagram 1 illustrates the different choices an individual can take at different time periods. The diagram shows that five different lifetime paths existing this extended human capital model. A person can obtain a job requiring vocational or further educational skills only if that person has previously taken the specific education. In other words, a person can only receive a vocational skilled wage if he or she has taken an apprenticeship in one of the previous periods.

The individual's maximization problem is to choose the lifetime path that maximizes lifetime income. Suppose $d_{k,t,i} = 1$ when individual i chooses occupation k in period t. The lifetime reward (benefits minus costs), R_i, for individual i can be written as

$$R_i = \sum_{t=1}^{6} R_{k,t,i} d_{k,t,i} \tag{1}$$

In this simple model the work and education reward is clearly separated because only the cost of education is a function of individual characteristics. For simplicity the reward of working ($d_{k,t} = 1$ when $k = e_{ns}, e_{vs}, e_{fs}$) is only a function of the wage related to a certain education level. [36] Thus the wage equation is a function of a constant term, see equation (2).

$$R_k = w_k \qquad \text{when } k = e_{ns}, e_{vs}, e_{fs} \tag{2}$$

[35] Unemployment is not included as a state. But in this model they could be a small group under the group working as non-skilled.

[36] Traditional human capital theory: Wage is a function of skill accumulation and years of experience in a certain occupation (often in a quadratic form)

The reward of attending school ($d_{k,t}=1$ when $k=s_{vs},s_{fs}$) is extended from the traditional human capital investment idea by including a fixed direct cost of schooling and an indirect cost of schooling. The direct cost is student fees, c_k, which do not change over time. The direct cost is reduced if the student can receive an AAS. In this case the AAS, $aid_{k,t}$, is both age- and education-specific. Thus only apprentices who start in the second time period (above 25 years of age) receive a subsidy. Therefore only $aid_{vs,2}>0$.[37] The indirect cost is divided into two parts. The first part captures an individual's initial aptitude for a certain education, which is education and individual specific, $ic1_{k,i}$. The second part of the indirect cost captures the individual's readiness for starting school, which is time- and individual-specific, $ic2_{t,i}$. Thus, if an individual's ability for studying varies with respect to the education stream, then the cost of studying different types of education varies, too. A key assumption is that the cost of education varies with time based on the idea that costs change when people mature or change social and economic status over time. For example, a person could mature over the years − thereby reducing the cost of studying − and then decide to take an education over a number of years. Then the reward of attending education for individual *i* in period *t* can be written:

(3) $$R_{k,t,i}=-c_k+aid_{k,t}+ic1_{k,t}+ic2_{t,i} \qquad \text{when } k=vs,fs$$

In Denmark one can think of $c_{ve}=c_{fe}<0$ because all students receive a fixed subsidy to live on while studying. Therefore the cost might be negative. Now it is possible to express an individual's choice of education over a lifetime by the following value function:

(4) $$V_i=\max_{d_{k,t,i}}\left[\sum_{t=1}^{6}\delta^{t-1}\sum_{k}^{K}R_{d,t,i}d_{k,t,i}\right]$$

[37] The students have to have an agreement with a workplace; this restriction is not taking into account in the simple framework.

More specifically, an individual *i*'s education-employment choice has to maximize the utility over life time. This means that the individual has a static optimization problem and thereby has to decide between the values discounted by δ of the five lifetime paths illustrated in diagram 1.

This analysis focuses on the optimization problem where the pathway including apprenticeship with an AAS is compared with the other four pathways, because this paper looks at the effect of an AAS. First, an analysis of the amount of subsidy that is necessary to make an individual indifferent between taking an apprenticeship with AAS or taking no education at all:

$$\begin{aligned} & w_{ns} + \delta(-c_{vs} - ic1_{vs,i} - ic2_{2,i} + aid_{vs,2}) + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{vs} \\ & = w_{ns} + \delta w_{ns} + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{ns} \end{aligned} \tag{5}$$

$$<=> \quad aid^*_{vs,2} = w_{ns} + (\delta + \delta^2 + \delta^3 + \delta^4)(w_{ns} - w_{vs}) + c_{vs} + ic1_{vs,i} + ic2_{2,i}$$

$$=> \quad d^*_{k,t} = \begin{cases} e_{ns}, e_{ns}, e_{ns}, e_{ns}, e_{ns}, e_{ns} \\ e_{ns}, s_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs} \end{cases} \quad \text{if} \quad \begin{matrix} aid_{vc,2} \le aid^*_{vc,2} \\ aid_{vc,2} > aid^*_{vc,2} \end{matrix}$$

Similar to the traditional human capital framework, this simple extended model shows that an individual is indifferent to taking an apprenticeship with an AAS or taking no education if the return to a vocational education is equal to the opportunity cost of taking a vocational education. Clearly the small wage differences between no schooling and apprenticeship and the high costs of apprenticeship increase the amount of AAS necessary for making an individual choose apprenticeship over no education at all.

Second, an individual is indifferent to taking an apprenticeship as an adult with an AAS and taking an apprenticeship without an AAS earlier in the lifetime path when:

$$\begin{aligned} & w_{ns} + \delta(-c_{vs} - ic1_{vs,i} - ic2_{2,i} + aid_{vs,2}) + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{vs} \\ & = -c_{vs} - ic1_{vs,i} - ic2_{1,i} + \delta w_{vs} + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{vs} \end{aligned} \tag{6}$$

$$<=> \quad aid^{**}_{vs,2} = -\frac{1}{\delta}w_{ns} - \frac{1}{\delta}ic2_{1,i} + ic2_{2,i} + \frac{\delta - 1}{\delta}(c_{vs} + ic1_{vs,i}) + w_{vs}$$

$$\Rightarrow \qquad d_{k,t}^{**} = \begin{cases} s_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs} \\ e_{ns}, s_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs} \end{cases} \qquad \text{if} \qquad \begin{matrix} aid_{vc,2} \le aid_{vc,2}^{**} \\ aid_{vc,2} > aid_{vc,2}^{**} \end{matrix}$$

This result indicates that if the costs of vocational education increase a lot over the lifetime, then the AAS for delayed education has to be comparably large for an individual to delay education rather than starting vocational education early. Furthermore, the higher the unskilled wage, the less money through an AAS is necessary for making an individual choose delayed apprenticeship − the opposite from the scenario of subsidized apprenticeship with no education at all.

The third scenario in which an individual is indifferent to taking an apprenticeship with an AAS or taking further education at an early age is the following:

$$(7) \qquad \begin{aligned} & w_{ns} + \delta(-c_{vs} - ic1_{vs,i} - ic2_{2,i} + aid_{vs,2}) + (\delta^2 + \delta^3 + \delta^4 + \delta^5) w_{vs} \\ & = -c_{fs} - ic1_{fs,i} - ic2_{1,i} + \delta w_{fs} + (\delta^2 + \delta^3 + \delta^4 + \delta^5) w_{fs} \end{aligned}$$

$$\Leftrightarrow \qquad \begin{aligned} aid_{vs,2}^{***} &= -\frac{1}{\delta} w_{ns} + w_{fs} + (\delta + \delta^2 + \delta^3 + \delta^4)(w_{fs} - w_{vs}) \\ &+ \frac{1}{\delta}(-c_{fs} - ic1_{fs,i} - ic2_{1,i}) + c_{vs} + ic1_{vs,i} + ic2_{2,i} \end{aligned}$$

$$\Rightarrow \qquad d_{k,t}^{***} = \begin{cases} s_{fs}, e_{fs}, e_{fs}, e_{fs}, e_{fs}, e_{fs} \\ e_{ns}, s_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs} \end{cases} \qquad \text{if} \qquad \begin{matrix} aid_{vc,2} \le aid_{vc,2}^{***} \\ aid_{vc,2} > aid_{vc,2}^{***} \end{matrix}$$

If the return to further education is relatively high and the cost concerning further education is relatively small, then the AAS has to be relatively high to make the individual indifferent to apprenticeship with an AAS or taking further education early in life.

This discussion leads to the final scenario, in which the individual is indifferent to delayed apprenticeship with an AAS or delayed further education.

$$
\begin{aligned}
(8) \qquad & w_{ns} + \delta(-c_{vs} - ic1_{vs,i} - ic2_{2,i} + aid_{vs,2}) + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{vs} \\
& = w_{ns} + \delta(-c_{fs} - ic1_{fs,i} - ic2_{2,i}) + (\delta^2 + \delta^3 + \delta^4 + \delta^5)w_{fs}
\end{aligned}
$$

$$
<=> \qquad aid^{****}_{vs,2} = (\delta + \delta^2 + \delta^3 + \delta^4)(w_{fs} - w_{vs}) + c_{vs} - c_{fs} + ic1_{vs,i} - ic1_{fs,i}
$$

$$
=> \qquad d^{****}_{k,t} = \begin{cases} e_{ns}, s_{fs}, e_{fs}, e_{fs}, e_{fs}, e_{fs} & \text{if} \quad aid_{vc,2} \le aid^{****}_{vc,2} \\ e_{ns}, s_{vs}, e_{vs}, e_{vs}, e_{vs}, e_{vs} & \text{if} \quad aid_{vc,2} > aid^{****}_{vc,2} \end{cases}
$$

Clearly, if the return to further education is relatively large compared to the return to vocational education, then the AAS has to be very large to make the individual indifferent to a lifetime path including an apprenticeship with an AAS or a lifetime path including delayed further education.

The discussion of the different scenarios makes evident that an introduction of an AAS affects the choice among all the different educational paths. The AAS actually increase the demand for attending apprenticeships, given an income effect, a substitution effect or a postponement effect.

An income effect is defined as the increase in demand for apprenticeships among the non-educated due to the indirect increase in the return to vocational education. The return increases because the AAS reduces the cost of taking the education, not because the wage of vocational skills increases. In this setting the income effect occurs when the introduction of an AAS makes a non-educated individual become an apprentice. The Danish authorities seem to have introduced the AAS because they believed that the income effect would be strong among the non-educated, thereby increasing their demand for vocational skills.

Another effect that has to be considered is the substitution effect. The substitution effect is defined as the increase in demand for subsidized apprenticeships among those who otherwise take further education. Again, the return of a delayed apprenticeship increases because the AAS reduces the cost of vocational education while the cost of further education remains the same. Thus, a person who previously wanted to delay further education prefers taking a delayed apprenticeship with an AAS instead. Actually, the person substitutes a further education with an apprenticeship at

the expense of a decreased demand for further education. The substitution effect increases the demand for vocational education.

Finally the introduction of an AAS can result in a postponement effect. The postponement effect is defined as the increase in demand for delayed apprenticeships with an AAS among young people who normally would have started education earlier in their life (i.e. before 25). The young person receives a higher return by postponing his or her studies because the AAS decreases the cost of taking an apprenticeship later. Thus, at the expense of a decreased demand for education among young people, the demand for vocational education increases among adults as a result of the postponement effect.

This analysis of the extended human capital model makes clear that the introduction of an AAS increases the attendance rate to vocational education among adults more than 25 years of age. The goal of the AAS is exactly to increase the demand for vocational skills. The problem is that the analysis also shows that the increased demand for vocational skills among adults is to some extent the result of a decreased demand for other education types. This result contradicts the general goal of trying to improve the overall skill of the workforce. The size of the partial effect and the total effect of introducing an AAS is better illustrated in a simulated theoretical model using "real" life numbers from Denmark. This is the focus of the next section.

4. A simulation of the introduction of the Danish AAS

The effect of an AAS can be difficult to analyze empirically because the total effect consists of different partial effects. Simulating the previous theoretical model with and without an AAS allows us to illustrate some of the different effects that occur. Thus, the simulated model can illustrate the increase in demand for vocational education due to substitution, income and postponement effects.

In the extended human capital model just described in section 3, individuals' heterogeneous preferences, costs and abilities are captured in the cost setup. As is common in the literature, this paper does not contain any information about each individual's cost function with respect to a certain education. Instead, I create and use different possible cost functions in the simulated model. Two cost scenarios illustrate the effect of an AAS on the educational attendance rate. In both cost scenarios the

assumption is that the costs of education, c, vary across j educations and t time periods as follows:

(9) $c_{j,t,i} = \alpha_{j,t,i} + \beta_{j,t,i}$ where $j = vs, fs$ $t = 1,2$

where the costs are a function of an individual's initial aptitude for a certain education, α, and time cost for taking a certain education, β. Thus, α and β are comparable to $ic1$ and $ic2$ in the theoretical model just described. The first simulation assumes that the costs of studying vary independently within and across education streams and time in the following way:

(9a) $c_{vs,1,i} = \alpha_{vs,1,i} + \beta_{vs,1,i}$

(9b) $c_{vs,2,i} = \alpha_{vs,2,i} + \beta_{vs,2,i}$

(9c) $c_{fs,1,i} = \alpha_{fs,1,i} + \beta_{fs,1,i}$

(9d) $c_{fs,2,i} = \alpha_{fs,2,i} + \beta_{fs,2,i}$

The independence assumption means that the aptitude-cost of an apprenticeship in the first time period is uncorrelated with the aptitude-cost of an apprenticeship in the second period. Furthermore, the cost of delaying the apprenticeship is uncorrelated with the cost of delaying further education. Even though the assumptions are simple, the simulated model predictions follow the results from the theoretical model described in section 3.

The second cost scenario assumes that costs vary across education streams and time but not within education and time:

(10a) $c_{vs1,i} = \alpha_{vs,i} + \beta_{1,i}$

(10b) $c_{vs2,i} = \alpha_{vs,i} + \beta_{2,i}$

(10c) $c_{fs1,i} = \alpha_{fs,i} + \beta_{1,i}$

(10d) $c_{fs2,i} = \alpha_{fs,i} + \beta_{2,i}$

In other words, it is assumed that a person who has high vocational aptitude when young also has high vocational aptitude as an adult. The same is true for the cost of time. Thus if it is costly to postpone vocational education, it is also costly to postpone further education. The second cost scenario might seem more realistic than the first, and the simulated models predictions will show the expected results as well.

As an illustration the changes in the educational distribution caused by introducing an AAS in Denmark, the simulated model includes some realistic numbers. Table 3 presents the actual wages and educational distribution from 1996 applied in the simulation. Table 3 shows that the non-educated are on average paid the least, and that employees with further education are paid the most. Employees with vocational skills in Denmark have on average not even earned 20 percent more per hour than non-educated employees. Table 3 also shows that approximately 37 percent of 30-year-olds have taken a vocational education before they turn 25, whereas not even 4 percent take one after they turn 25. However, among 30-year-olds who take a further education, the percentages are 20 and 7, respectively. Among 30-year-olds, more than 30 percent had no education at all.

As mentioned earlier, information on individuals' cost functions are missing. To make up for missing information, I create the two cost scenarios to fit the distributional education in 1996. Table 3 presents the distributional assumptions concerning the cost function in the two scenarios. For simplicity, the discount rate is assumed to be constant, but it is possible that it varies across persons and over time. Finally, I use the hourly wages and costs in Table 3 to calibrate wages and costs for the aggregated six time periods described in section 3.

It is expected that the educational distribution in table 3 changes when an AAS is introduced into the simulated model, because that is the prediction of the theoretical model in section 3. It is also expected that the size of the educational changes depends on the size of AAS. In the carpenter apprenticeship example in section 2, apprenticeship income during an apprenticeship increases by approximately 40 percent when an AAS is introduced. The income increase can also be interpreted as a cost reduction of 40 percent during studies. Therefore the model is simulated with an AAS that on average reduces costs by 40 percent. An AAS that reduces costs by 40 percent is very extensive, so the effect is expected to be extensive too. To test the consistency of

the results, I simulate the model using an AAS that reduces education costs on average only by 10 percent. Finally I simulate the model where an AAS that reduces costs by 40 percent is introduced after the first period. Thus, only the people who did not study in the first period can change their educational choice due to the sudden introduction of an AAS.

Tables 4 and 5 illustrate the simulation results, which show quite clearly that an AAS increases the attendance rate to vocational education among the adults regardless of cost structure. The elasticity of the demand for adult vocational education with respect to an AAS depends on the cost assumption and the time at which an AAS is introduced. For the independent cost scenario the elasticity is 1,32 when an AAS is introduced before period 1 and 0,41 when an AAS is introduced in period 1. The latter elasticity is the short-term effect and the first elasticity is the long-term effect. For the scenario with dependent costs, attendance is more elastic with an elasticity of 1,92. The large effect is mainly due to all the people who prefer to delay their apprenticeship when an AAS will later be possible.

Table 5 shows the mobility changes between educational paths when an AAS reduces the education cost by 40 percent in a scenario with dependent costs. Not surprisingly, all the people who choose a delayed apprenticeship without an AAS also choose a delayed apprenticeship with an AAS. Likewise interesting is that the new group of people choosing an adult apprenticeship with an AAS include not only the non-educated. Some of the new starters are people who previously would have chosen further education in the same period, further education in the previous period or vocational education in the previous period. Thus, the simulation results show that introducing a subsidy will make all individuals re-evaluate their education decision. Although one might argue that the results are due to the simulation of a simple static model, the results for the educational distribution changes in the whole population when introducing a subsidy are in line with Wolpin's & Keane's (1997) dynamic setting results.

In the scenarios with dependent and independent costs, the increase in the vocational attendance rate among adults can be divided into the income effect, the substitution effect and the postponement effect. In the dependent cost setting 12 percent of the increase is due to the income effect, where people prefer apprenticeship with an

AAS to no education. Eighty five percent of the increase is due to the postponement effect, where people postpone their vocational or further education. Finally, 3 percent of the increase is caused by the substitution effect, because people substitute delayed further education with AAS apprenticeships.

The simple exercise of simulating the theoretical model with an AAS illustrates two factors. First, the simulation results show that an AAS increases the attendance rate into vocational education among adults exactly as in the theoretical model. This result is not surprising, because the simulated model is set up as the theoretical model so an educational cost reduction is expected to increase the demand for education.

Second, the results show that the demand increase for delayed apprenticeship results from people deciding to start an apprenticeship, delay education, or change education. In other words, the "new attendees" come from *all* the different lifecycle educational pathways. This result is important for conducting an empirical AAS evaluation, because it illustrates the challenge of finding an obvious control group when the whole population is affected by AAS. This aspect is discussed further in the following sections on the empirical model and the empirical data at hand.

5. The challenges of an empirical evaluation model

The results of the simulated model show that an AAS influences the educational choices in the whole population. Therefore, the best empirical strategy for evaluating the total effect of an AAS is to split the population group into two, and to treat one half with an AAS and not the other half. Observing the two presumably homogeneous groups before and after the introduction of an AAS illustrates the true effect of an AAS if every other important factor is constant or on average has developed equally for the two groups. Thus a straightforward difference-in-difference estimator can find the effect of an AAS as follows:

$$(11) \qquad effect = (attend_{t=1}^{treat} - attend_{t=0}^{treat}) - (attend_{t=1}^{control} - attend_{t=0}^{control})$$

Where $attend_t$ is the vocational attendance rate among the treated (eligible for an AAS) or the controlled (not eligible for an AAS) at time t. Time period 0 is before the

introduction of an AAS and time period 1 follows the introduction of an AAS. This simple experimental method is not possible in this paper because the AAS was introduced in 1997 to everyone in the population over 25 years of age who fulfilled the conditions for receiving an AAS. Thus the empirical evaluation has to focus on partial effects, not on the whole *"true"* effect of an AAS in the Danish population. Even though the apprenticeship system is quite extensive in countries such as Denmark, Germany, Switzerland and Austria, Denmark is the only country to introduce an AAS. Therefore, no international studies have evaluated the effect of a subsidy on adult apprenticeships. In Addition, no empirical model is commonly used to evaluate apprenticeship subsidies.

In contrast, there are a great many studies evaluating the effect of different student aid programs on college attendance in the US. Some of the early studies by Manski and Wise (1983) focus on cross-sectional variation in aid and individual characteristics. These studies are vulnerable to bias induced by correlation between aid and unobserved propensity to attend college. Most of the first studies find no effect of aid on college attendance for young people. More recent studies such as Angrist (1993) focus on the GI Bill (veteran benefits) and find a positive effect on school completion by exploiting the change in benefit over time. More recently, Dynarski (1999) examined the effect of a shift in the federal financial aid policy. By using the exogenous shift in aid and eligibility of social security student benefits (death, disability or retirement of a parent), Dynarski finds a positive effect of aid on school attendance using a difference-in-differences estimator.

Inspired by the work of Dynarski and others, I use a difference-in-differences estimator to evaluate the effect of an AAS within a minor group of the Danish population, because the Danish data include both the period in which the exogenous change of an AAS occurs and comparable age groups around the age of 25. The data is a panel of a 10 percent sample of the Danish population over 16 years of age from 1995 to 2004. This data is described in detail in section 6. The age-specific eligibility means that an AAS applicant has to be at least 25 years of age. Both the age groups above and below 25 years of age are represented in the Danish data, which include information on AAS applicants and non-applicants before and after 1997, where the AAS was introduced. Combining these facts makes it possible to identify the effect

of an AAS, because comparing the unskilled 25-year-olds who are eligible for an AAS with the ineligible unskilled 24-year-olds from 1996 to 1998 is possible. I use 1998 instead of 1997 because, as previously mentioned, in 1998 the AAS was fully introduced. One could argue that comparing all age groups above and below 25 years of age would capture the AAS effect better. Unfortunately, such a comparison would jeopardize the identification strategy, which relies on comparable age groups.

Assuming no difference in covariates and time trends among the treatment group (25-year-olds) as well as the control group (24-year-olds) means that the effect of an AAS can be estimated by the following simple difference-in-differences estimation:

$$(12) \qquad attendvs_i = \alpha + \beta(age25_i * year98_i) + \delta age25_i + \lambda year98_i + \varepsilon_i$$

Where $attendvs_i$ is 1 if individual i starts an apprenticeship while $age25_i$ and $year98_i$ are dummies for eligibility for an AAS and the year for the full introduction of the subsidy, respectively. The effect of the AAS eligibility is captured by β. Equation (12) can be estimated by both OLS and probability models, depending on the assumption of a linear trend or a non-linear trend.

Suppose now that the covariates and time trends among the treatment and control groups are different. If so, then the AAS effect is not unambiguous and might indicate a heterogeneous time trend. Therefore, it is wise to include factors that pick up the different time trends in the difference-in-differences estimation.

$$(13) \qquad attendvs_i = \alpha + \beta(age25_i * year98_i) + \delta age25_i + \lambda year98_i + X_i\gamma + \varepsilon_i$$

Controlling for different sources of heterogeneous time trends, *X*, improves the β estimate. In other words, taking the individuals heterogeneous costs into account is important. These costs can vary due to observable characteristics and non-observable characteristics. An observable characteristic could be family situation, whereas an unobservable characteristic could be the ability to study. This paper takes only

observable characteristics into account.[38] The β still captures the estimated effect of an AAS among the 24-year-olds and 25-year-olds. As it is a reduced form estimate, the β is the total effect within the selected group, and the estimate is therefore the sum of the income effect, postponement effect, and substitution effect.

Although the treatment and control group in this analysis are narrowly defined, it is precisely among this group that one would expect to find a positive effect of an adult subsidy, since the 24-year-olds and the 25-year-olds are so similar. If an AAS doesn't increase the vocational attendance rate in this population it is difficult to claim that the AAS has an overall positive effect in society. To sum up, the difference-in-differences estimator used for this paper is chosen because it is the most sensible method for the rich data available.

6. Data and descriptive statistics on Danish education

The rich data at hand is a major reason why it is possible to use a difference-in-difference estimator to evaluate the effect of an AAS. The data, which comprises three data sources, is very informative about individual educational decisions.

The first source of data is register panel data from Statistics Denmark, including a 10 percent random sample of the population aged 16 years and over from 1995 to 2004. The data includes very detailed information on socio-economic individual characteristics, such as age, family status, educational skills, personal income, and unemployment history. All variables are measured annually except for the unemployment history variables. The unemployment and activation histories are reported as spells. The precise unemployment histories and occupation status allow us to identify precisely when individuals start apprenticeships or other education forms. The panel structure allows us to look at the population and the educational structure before the introduction of the AAS. Thus we can follow the individual's later educational choices. The panel data period is dictated by two incidences: First, it is important to have post and ex post data for 1997, when the AAS was introduced. Second, as the most reliable occupation information exists after 1995, I decided not to use information pre-dating 1995 (i.e. the unemployment information is best after 1995).

[38]Previous studies have attempted to take observable and unobservable heterogeneity into account as well (Grilliches 1977, Dynarski 1999, Angrist 1993, Manski & Wise 1983)

The second source of data includes records on all apprenticeships receiving an AAS from 1997 to 2005. The AAS is recorded in the DREAM register and collected by the National Labor Market Authority. The weekly observations are transformed into continuous spells to control for the length of the apprenticeships. The purpose of using this data is threefold. First, the data maps out an exact picture of all apprentices receiving an AAS from 1997 to 2005. Second, the data illustrates the relationship between application and approval rates in the Greater Copenhagen Area. Finally, the most important use of the DREAM register is to point out the apprentices with an AAS in the 10 percent population data because the population data does not have reliable information about the AAS before 2001. Thus, the DREAM register is both a complement and a support to the population register data.

The third data source is "The Databank of Statistics Denmark". From this data I obtained the macro-climate and education attendance rates (especially before 1995). Furthermore, the data helps to illustrate the comparability of the control group and treatment group for the difference-in-differences estimator.

Educational distribution and AAS in Denmark

In Denmark the educational distribution has changed from 1996 to 2004. Figures 5 and 6 show the educational distribution among 30-year-olds over time with respect to gender. The figures also illustrate when 30-year-olds start taking vocational or further education. The figures clearly show that the skill level improves over time among 30-year-olds, even though from 1996 to 2004 relatively few 30-years-olds started a vocational education before turning 25. In contrast relatively more 30-years-olds started a vocational education after turning 25. Finally, the percentage of students in further education increases for all age groups.

As a comparison to Figures 5 and 6, the overall picture among cohorts of the non-educated is that the vocational attendance rates decrease over a lifetime (see figures 7, 8, 9 and 10). A closer look gives the impression that dividing the cohorts in two groups is possible. One group is all the young people under 25 years of age in 1997 (cohort 1973 +). The second group of cohorts is the unskilled over 25 years of age in 1997, who in theory are eligible for an AAS. For the unskilled men under 25 years old in 1997, the attendance rate either increases or stops decreasing when they turn 25. For

the second cohort group, two tendencies occur. One tendency is that a decreasing attendance rate in 1997 is followed by an increasing rate in 1998 and a decreasing rate thereafter. The other tendency is an increased attendance rate in 1997 and 1998, followed by a decreasing rate thereafter. Both tendencies support the view that 1998 is the year when the AAS was fully implemented. For unskilled women, the figures are similar, except for small differences with respect to the 1975 and 1968 cohorts. No obvious reason for these exceptions exists.

If we now look more specifically at the AAS apprenticeships, Figure 11 and Figure 12 show that most men in an AAS apprenticeship participate in education periods within the fields of building and construction and iron steel and metal production.[39] The women were mostly in trade and office and food and domestic production. In addition, the entry into health increases for women, whereas the entry into building and construction decreases. Among men the attendance rate for entry into iron, steel and metal decreases. The distributional share is to some extent in line with the bottleneck list for subsidized educational fields (see appendix A).

A look at the AAS population by region shows that some differences occur, but in general the building and construction, iron, steel and metal production fields are the most subsidized for men (see table 6). For women, although the regional differences are bigger, the trade and office and food and domestic production fields are the most subsidized, whereas the building and construction fields are only popular in some regions (see table 7). Overall, a lot of regions subsidize many different educational fields. Only education, health, and services − which are typically female vocational education fields − are not subsidized. The lists of bottleneck areas from the Greater Copenhagen Area and the other regions in 2006 (from appendix A) support the impression that a lot of apprenticeship fields are subsidized. More specifically, the most populated areas (such as Greater Copenhagen, Århus, Fyn, Frederiksborg and Roskilde) are the regions with the most bottleneck areas.

Comparing apprentices with an AAS against apprentices without an AAS reveals some interesting characteristics. Table 1 shows that far from all apprentices over 25 years of age in the Greater Copenhagen Area receive an AAS, despite the fact their

[39] The information on field of education is taken from the year of entry and the year after entry. The entries for 2004 have a different distribution because there are many missing entries in the educational field. Thus the entries for 2004 are excluded.

educational fields are on the bottleneck list.[40] Compared to the theoretical set-up, where all adult apprenticeships are subsidized, the empirical data show that not all apprentices in bottleneck fields are subsidized. The unions and unemployment offices give several reasons for cases in which AAS is not received. First, caseworkers stress that lack of information about an otherwise favorable AAS can not explain why people enter a bottleneck education field without an AAS. Second, caseworkers point out that the lists of bottleneck areas are guidelines that change every three month. Therefore, within a person's application period, the list of subsidized fields could have changed. Third, caseworkers stress that in most cases the students in subsidized education fields can receive an AAS only if all the apprentices in the region have workplace connections. Finally caseworkers stress that the regional authorities have a budget limiting the number of students who can receive an AAS. Thus a denied application could simply be the result of a lack of financial resources.

Furthermore, one might expect that most employers and students make an agreement on apprenticeship with an AAS before the application is finally accepted. If they receive a rejection for the reasons just mentioned they probably still continue with the agreement without the AAS. Additionally, many of the applicants already work at the workplace where they make the educational agreement. Thus, the employees and employers are both mentally and economically already involved, and therefore they continue the educational agreement even without an AAS.

It is obvious that the subsidized apprentices are on average older than apprentices without an AAS because of the age restriction in the AAS regulation. Figure 13 illustrates the difference in age distribution among the subsidized and the non-subsidized apprentices. The descriptive statistics in Table 8 show the differences between the newly started apprentices with and without an AAS. The majority of subsidized apprentices are between 25-30 years of age, and a large proportion is older when starting an apprenticeship with AAS. Instead, among the non-subsidized apprentices, almost 85 percent of men and 65 percent of women are under 25 years old when they enroll. Due to the big differences in age distribution between apprentices with an AAS and those without an AAS, one would expect to observe other socioeconomic differences as well.

[40] Even more detailed educational categories show the same result.

Table 8 clearly shows socioeconomic differences exist between the two apprentice groups. There is an overrepresentation of men among the subsidized apprentices compared to the non-subsidized apprentices. Furthermore, it is common for persons in couples with children and without children to take a vocational education with an AAS. However, among the traditional apprentices, more than 50 percent of men and 40 percent of women are single. Surprisingly, no ethnic differences are apparent.

The apprentices work in all regions in Denmark and are distributed similarly with respect to gender and AAS. The major educational fields that the non-subsidized men enter include office and trade, building and construction and iron, steel, and metal. The majority of subsidized men mainly work in sectors like building and construction and iron, steel, and metal. In contrast, the non-subsidized women enter apprenticeships in fields such as trade and office and health, whereas the subsidized women are more diverse. The latter probably results from the authorities not including typically female educational fields on the bottleneck list.

The previous occupation of a new apprentice also differs among subsidized and non-subsidized men and women. The majority of all apprentices with an AAS are wage earners, but among the non-subsidized apprentices, a lot begin apprenticing directly after school. Therefore, both men and women who receive AAS have on average a previous income or wage significantly higher than the non-subsidized apprentices. Furthermore and not surprisingly the subsidized apprentices have remarkably longer work experience than the non-subsidized apprentices.

Control group versus treatment group

The difference-in-differences estimator explained in section 5 is appropriate for evaluating the AAS if a suitable control group and treatment group exists. Due to the age restriction, comparing people over 25 years of age with people under 25 years of age that have the same characteristics makes good sense. As previously illustrated, because age is correlated with a lot of other characteristics, those above and those below 25 is comparable as long as the actual ages are not too far apart (e.g. comparing the 24- and 25-year-olds, not comparing the 20- and 35-year-olds). The reason is that two age groups such as 20-year-olds and 35-year-olds are different with respect to family status, work experience, health conditions, etc. Most importantly, 35-year-olds have been

influenced by more than the 20-year-olds with respect to different exogenous business cycle shocks and changes in legal regulations. Thus comparing two similar age groups is a better idea.

Therefore, I narrow the control group and treatment group tremendously, using the 24-year-olds as a control group for the 25-year-old treatment group. Furthermore, employees who already have a vocational education are excluded because they do not have an obvious economic incentive for choosing a new vocational education. By contrast, the people who most likely are receiving an educational subsidy already are expected to have some economic incentive to start a new education because they receive a higher wage. They are therefore included in the sample. However, those who already had an apprenticeship position before the introduction of AAS are not included. Actually a maximum of 2.5 percent of the new apprentices were involved in other kinds of education the year before they became apprentices (see table 8).

The assumption that the unskilled 24-year-olds are a good control group for the unskilled 25-year-olds is valid if the two groups are identical with respect to attendance rates before the AAS was introduced and if they react in the same way to macro-shocks. Figures 14 and 15 demonstrate that apprentices attendance rates among 24- and 25-year-old men is split into two time periods: before and after the introduction of the AAS. The first period is 1991-1996, when the two age group attendance rates are parallel. In the second period, from 1997-2003, the attendance rates generally went in opposite directions − except for 2000 and 2003. Given the similar trend in attendance rates in the period before 1997, the 24-year-olds seem like a good control group for the 25-year-olds who are eligible for an AAS.

For the women, the vocational attendance rates among the relevant age groups are split into three time periods. In the first period, from 1991-1993, the attendance rate increases for the 24-year-olds whereas the rate decreases among the 25-year-olds. In the second period, from 1994-1997, the attendance rates are parallel for the two age groups. The last period, from 1998-2003, is characterized by the attendance rates going in opposite directions. The picture among women is more ambiguous than for the men because the rates do not exactly follow each other through the whole period before the 1997 introduction of the AAS. Furthermore, the difference in attendance rates after 1997 is puzzling, because both rates increased in 1997 (although relatively more

for the 25-year-olds). Thereafter, the attendance rate among the 25-year-olds actually decreases. Later the attendance rate increases again but relatively less than among the 24-year-old women. In this paper, the 24-year-olds are still used as a possible control group to the 25-year-olds women because the attendance rates of the two age groups are parallel before 1997. Obviously, the difference-in-differences estimation results for women is expected to be different from men because of the unexpected development in attendance rates after 1997 and the gender skewness in subsidized bottleneck fields. Therefore a slight skepticism about the results for women is advised because the identification criterion is to a certain extent questionable for women.

As section 5 describes, taking an individual's heterogeneous observable characteristics and non-observable characteristics into account can be important because these characteristics can influence the cost of taking a vocational education. Thus, the personal characteristics can be correlated with the vocational education attendance rate. Tables 9 and 10 show that on average the 24- and 25-year-olds starting apprenticeships do not differ significantly with respect to socioeconomic characteristics before the introduction of the AAS. Moreover, after the introduction of the AAS, there is no significant difference between them, although both the 25-year-old and the 24-year-old new apprentices seem to be exposed to a minor time trend from 1996 to 1998. Tables 9 and 10 to some extent support the assumption, that the 24-year-olds are a good control group for the 25-year-olds.

Even though the difference-in-differences estimator is a well-recognized estimator in the evaluation literature, we have to use it cautiously in evaluating the AAS. The reason is that the control group becomes the treatment group as well. To be more specific, when the 24-year-olds know they might be able to get a subsidy when they turn 25, some will behave accordingly, by delaying their apprenticeship for one year. The incidence of delayed studies might explain why a decrease in attendance rates among the 24-year-old men is observed right after 1997. In previous literature, this incidence is called the Ashenfelder's dip (Ashenfelder 1978). Therefore, a positive effect of AAS might not result from an increase in the apprenticeship attendance rates among the 25-year-olds, but rather from a decline in attendance rates among the 24-year-olds. This effect is referred to as the postponement effect in sections 4 and 5 of this paper. In other educational evaluations, geographical areas are often used as a control

group. Again, the problem of the control group becoming the treatment group exists, unless it is assumed that the people living in the control region can not move to the treated region. The same problem exists if an educational subsidy depends on household income, because people can work less and reduce their income to qualify for an educational subsidy. If they do so, they would end up in the treatment group. Thus, many studies suffer from the postponement effect, a condition important to keep in mind when interpreting the results.

Data sample for estimations

Even though the data at hand is rich in information about the entire Danish population, this paper uses only a minor sample for the final estimation. This choice is due to the importance of having a trustworthy control group and treatment group for the difference-in-differences estimation method. As previously argued, the unskilled 24-year-olds not already taking an apprenticeship make a good control group for the treatment group consisting of the unskilled 25-year-olds not yet apprenticed. The immediate analysis comes from looking at the effect from 1996 to 1998 among the unskilled 24- and 25-year-olds. For the men the immediate effect is estimated by the difference-in-differences method with 7687 observations. The sample for women has 9006 observations. For the delayed effect, all years are used. Thus, the sample for the men consists of 27571 observations and for the women there are 32787 observations.

To sum up, the rich Danish panel data on the non-educated 24-year-olds and the 25-year-olds and the exogenous introduction of the AAS in 1997 make it possible to evaluate the effect of the AAS by a difference-in-difference estimator for men and women. Section 7 describes the results.

7. Empirical results on the effect of an AAS

Using the difference-in-differences estimator from section 5 this section illustrates the effect of an AAS on the probability of attending apprenticeship. The discussion of the results and the use of methods concentrate around the results for men. The reason is that the difference-in-differences method seems more suitable for men than for women, given the preponderance of typical male-dominated education fields pinpointed for AAS subsidies.

The immediate effect of an AAS

Tables 11 and 13 present the results of the difference-in-differences estimator, illustrating the immediate effect of the AAS on the attendance rate. Table 11 shows the average probability of attending an apprenticeship as a 24-year-old and as a 25-year-old in 1996 and 1998. Among the unskilled 24-year-old men, 3.28 percent started an apprenticeship in 1996 before the AAS was introduced. The attendance rate increased a little in 1998 to 3.34 percent. By contrast, among the unskilled the attendance rate among 25-year-old men increased dramatically from 1.82 in 1996 to 4.39 in 1998. Here the 25-year-olds are eligible for an AAS in 1998 because they fulfill the age restriction, whereas the 24-year-olds are not. If there were no time trends and changes in socioeconomic factors, then the effect of the AAS would be the difference between the attendance rates over time between the 24-year-olds and the 25-year-olds. The difference for men is 2.51 percent, which is quite high considering the original attendance rate of 1.82 percent.

The difference-in-differences estimate can also be estimated through a simple OLS equation, as illustrated in equation 12 (see column 1 in table 13). The OLS estimate − 2.51 percent for the men − is highly significant. If the time trends among the 24-year-olds and the 25-year-olds are different, then the effect is a time trend instead of an AAS effect. I therefore include variables that pick up the time trends in the difference-in-differences estimation (see column 2 and 3 in table 13). The AAS still increases the probability of attending apprenticeship by 2.54 percent. Thus, including the socioeconomic variables does not change the subsidy effect, but it does increase the adjusted R^2.

The outcome variable regarding apprenticeship attendance is discrete rather than continuous, making a probit model more appropriate. Table 13 states the marginal effect of the probit model in column 4. The AAS effect of 2.7 percent is a bit bigger than the effect from the OLS estimates, but not significantly different. Although, the estimated subsidy effect does not change significantly, the adjusted R^2 increase significantly when the probit model is used. Thus the probit model fits the attendance decision better.

The first results for the immediate effect of an AAS for the unskilled 25-year-old men's attendance rate in 1998 compared to 1996 are significantly high as expected. The results are in line with Figures 3 and 8, where the 25-year-old men's attendance rate increased in 1998.

The delayed effect of an AAS

As Figure 8 shows, the apprentice attendance rate among unskilled men over 25 increases in 1998 and decreases thereafter. Therefore, one might expect the delayed effect of an AAS to be negative. The delayed effect of the AAS is thus estimated by the difference-in-differences estimator (see table 15-18). Table 15 shows the results for the effect of the AAS in 2002, whereas Table 17 illustrates the results for all years.

The 2002 result shows that the AAS effect is between 0.005-0.008 percent and insignificant. Thus, the vocational attendance rate among the unskilled 25-year-olds does not increase significantly compared to the attendance rate in 1996, before the introduction of AAS. Very small and insignificant AAS effects are also found for all other years after 1998 (see table 17). Once again, the probit models have the highest adjusted R^2. Therefore the difference-in-differences results from the probit models are the most reliable.

Gender differences regarding AAS

The results for men show that among unskilled 25-year-old men the effect of an AAS is strong and significant in 1998 but insignificant over the years. By contrast for the unskilled 25-year-old women the results in Tables 12, 14, 16, and 18 show that the AAS effect is very small and insignificant in all years.

It is not surprising that an AAS affects men and women differently. As discussed in section 2, the majority of educational fields that are on the regional bottleneck lists are traditionally male dominated. Therefore, many women probably do not see the AAS as being as attractive as the men do because these women want to study in educational fields not on the list.

Given the scarcity of typically female educational fields on the bottleneck lists, the difference-in-differences estimation method is of questionable value for women. One might think that the eligibility criterion needs to be narrowed if the

difference-in-differences method should be correctly used. Unfortunately, with the data at hand, creating better eligibility criteria is not possible. Instead, the conclusion is that the AAS has no measurable effect among the unskilled 25-year-old women.

The interpretation of the covariates in the attendance rate results

The results of the previous subsection clarify that the AAS has an immediate positive effect in 1998 among men but not in the rest of the observed time periods. Due to the generosity of the AAS, the high effect in 1998 is not surprising. The finding that men who are out of the labor force or studying (but not apprentices) have less risk of entering a vocational education than men working as wage earners or men who are self-employed is not surprising either. As an apprentice has to have an agreement with an employer to obtain an AAS, this agreement is easier to get for those who already have an employer. Therefore, the wage earners have a higher probability of entering an apprenticeship. The fact that a high income reduces the risk of becoming an apprentice is understandable because of the reduced economic incentive for starting an education.

Less easy to explain is the finding that long work experience increases the likelihood of becoming an apprentice. As long work experience is normally correlated with higher wages, the incentive to study would therefore be expected to be reduced. However, the group of people under analysis comprises unskilled and relatively young men. If an unskilled 25-year-old man has a lot of work experience, he might have already reached the top level of what an unskilled wage earner can earn. Therefore, the only way he can earn more money is to increase his skills. An unskilled man with many years of work experience might also have decreased his work ability through the effect of years of hard physical work. Therefore, he would need to get new skills to find another job with less physical pressure. Thus, the economic incentive to get an AAS apprenticeship exists among young unskilled people who take lifetime income into account.

Personal characteristics such as ethnicity and family background are apparently not significantly important in the schooling decision among the unskilled young men. However, a few geographical areas have a significant, but small positive effect on the vocational education attendance rate compared to the Greater Copenhagen

Area, a finding that of course is to some extent correlated with the labor market situation in these areas.

For the women, the probability of getting an apprenticeship increases if they have the same characteristics as the young men just described. Additionally, unemployed women have a reduced likelihood of becoming an apprentice compared to female wage earners.

The income, substitution and postpone effect regarding AAS

Although in section 3 and 4, the effect of an AAS was split into substitution, income and postponement effects, the results of the difference-in-differences estimations can not be split into these three different effects. The effects are summed up in the total empirical effect of an AAS.

The increase in the attendance rate among unskilled 25-year-old men can result from 24-year-olds postponing their education because of their expectation of a future AAS or from the companies where the 24-year-olds work advising them to wait until they are 25-year-old. This postponement effect is expected to occur among all age groups below 25 years of age, but the effect should be the strongest among the 24-year-olds because they lose a maximum of one year of salary as a skilled employee by delaying their apprenticeship for one year, while the younger age groups lose more.

The substitution effect occurs when the 25-year-olds decide to take an apprenticeship instead of further education due to the AAS. Comparing apprenticeships and further education is very difficult for a number of reasons. For example, the aptitudes necessary for being a good carpenter are very different from those necessary for being a good economist. Thus, the possibility of switching education might not be possible, as the human capital theory predicted. Furthermore, Table 2 shows taking an apprenticeship even without a subsidy is financially a better idea than taking a further education during the study period. Therefore, one would think that strong preferences for further education and future income is more important than the income one receives while studying, in deciding on further education. Thus introducing an AAS is not expected to influence most young people who prefer further education without a subsidy.

The income effect exists if unskilled 25-year-olds who decide not to take an education due to high education costs suddenly decide to take an apprenticeship due to an AAS. This effect seems very possible, especially among the 25-year-olds, because they have had enough work experience as unskilled workers to see that education might be necessary for sustaining a future income. Additionally, if they decide to take an education under favorable economic conditions, they still have plenty of years to receive a better income from working as a skilled employee.

Even though it is not possible to separate the three effects of the AAS results, I have argued that the income effect and the postponement effect probably occur within the two age groups analyzed for 1998. However, because the AAS had no effect after 1998 it might be the case that none of the three effects occur after 1998.

Sensitivity analysis and elasticity with respect to AAS

The AAS effect from these results is true for the narrowly defined treatment and control group. As shown in the simple human capital simulation model in section 4, the whole population's education decisions are affected by the AAS. Unfortunately, the results of the AAS can not be transferred to the whole population immediately. Instead, I expand the control group to 23- and 24-year-olds and the treatment group to 25- and 26-year-olds. Tables 19 and 20 show the results. As expected, there is an immediate effect of the AAS in 1998 for men but not over the rest of the period. Interestingly the effect is smaller than the effect found among only 25-year-olds − a finding also expected because the older one gets, the less economic incentive one has for getting an education. Thus the 26-year-olds reduce the effect of an AAS. Furthermore the 23-year-olds have a higher cost than the 24-year-olds in postponing their education, which again reduces the effect of an AAS.

Because all people over 25 in theory could start an apprenticeship with an AAS if they wanted to, the control and treatment group includes students. As Table 8 shows few people start an education and then switch to an apprenticeship with a new subsidy. The analysis is therefore conducted on the unskilled 24- and 25-year-olds who have not participated in any studies in the previous year. The results in Tables 21 and 22 illustrate an even bigger effect of the AAS among men in 1998 as opposed to earlier than among men already studying in another educational field.

In the literature, when education subsidies are evaluated, researchers compare either elasticities or US$ 1000 increases or reductions. In this paper, the elasticity and a US$ 1000 change is only worth looking at for unskilled 25-year-old men in 1998 because for women and for all other years the AAS effect was insignificant.

Although the elasticity with respect to the AAS can be calculated in different ways, this paper uses the average numbers illustrated in Table 23. The average numbers from Table 23 show that the vocational attendance rate among 25-year-old unskilled men is highly elastic to an AAS. Thus, the elasticity is 4.64.[41] However the AAS is also quite extensive in Denmark. On average the AAS increased the apprenticeship income by 32 percent or US$ 23880 in the subsample of 24- and 25-year-olds. Given the estimated AAS result, a US$ 1000 increase would increase the vocational education attendance rate among unskilled 25-year-old men by 0.11 percent.[42] This percentage is quite low compared to other educational subsidy effects found in the literature (e.g. Dynarski 1999; Manski and Wise 1983; Angrist 1993).

As mentioned earlier, previous studies on subsidizing education have mainly looked at college attendance in the US. It is therefore very difficult to compare previous results with the results of this paper especially because the previous subsidies often have been reserved for certain social classes or for people with previous military experience. Still compared to other studies, this paper shows that the effect of an AAS has an immediate high and significant effect on unskilled men. However, the amount of AAS is also quite extensive compared to subsidies given in other countries. Compared to other international education evaluations, it is puzzling that this study finds no measurable effect of the very generous AAS subsidy after 1998.

8. Conclusion

This paper posed the question whether the AAS improves the aggregate education level in the population. By simulating an extended human capital model, this paper shows that all population groups reconsider their education decision when an AAS is introduced. The simulation results show that the level of vocational skills among adults increases with an AAS. However, because substitution, income and postponement effects occur when the subsidy is introduced, the increase in vocational skills among

[41] e= (147,8/31,84)
[42] Increase = (2,69/23880)*1000) = 0,11 confidence interval 0,03-0,20

adults (i.e. more than 25 years of age) is to some extent caused by a decrease of skills in other population groups.

Even though the simulation illustrates the difficulty of finding an optimal empirical strategy capable of evaluating the total effect of an AAS, because of the absence of an obvious control group, this paper makes a partial empirical evaluation. Using the difference-in-difference estimator this paper examines the effect of the AAS among the unskilled who delayed studying. The rich panel data and the exogenous shift in the AAS in 1997 as well as the specific age-eligibility criteria make the evaluation possible.

The empirical results show that the AAS had a clear positive effect on vocational education attendance rates among non-educated 25-year-old men in 1998. However, 25-year-old women were not affected by the subsidy. Additionally the AAS had no significant effect on the vocational education attendance rate after 1998, regardless of gender. The immediate elasticity of attendance with respect to AAS for men was very high and significant in 1998.

The results are important for Denmark and for other countries that want to invest in improving the skills of their adult workforce. First, they need to know that a generous AAS suitable for a certain population group (e.g. non-educated men over 25 years old) increases the skill level immediately within the specific population group. Second, they should be aware of the fact that there seems to be no long run effect of the very generous AAS. Thus, an economic incentive (e.g. an AAS) for a specific education (e.g. vocational education) might not permanently improve the skill level of the population as a whole.

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Appendix A

Number of bottleneck areas within the major industry categories in Danish regions, 2006 4th quarter.

	St.K.	Fred.b.	Rosk.	Vestsj.	Storstr.	Bornh.	Fyn
Office& Trade	2	2	0	2	1	1	2
Building & construction	18	18	12	12	8	10	12
Industrial engin. & other	0	1	1	1	1	1	0
Service	1	1	1	1	0	0	0
Food & domestic agricultural & fishing	4	4	3	4	4	3	3
Transportation	1	1	0	0	0	0	1
Health	2	3	4	1	3	4	1

	Sønderj.*	Ribe*	Vejle	Ringk.	Århus	Viborg*	Nordj.*
Office& Trade	0	0	1	1	1	0	0
Building & construction	7	8	15	13	13	4	5
Iron,steel & metal	0	1	1	9	5	3	1
Industrial engin. & other	0	1	1	1	1	0	0
Service	0	0	4	0	1	0	0
Food & domestic agricultural & fishing	0	0	0	4	3	0	0
Transportation	1	0	4	1	0	0	0
Health	0	1	3	4	4	0	1
				1	1	0	0

Note: No detailed list available

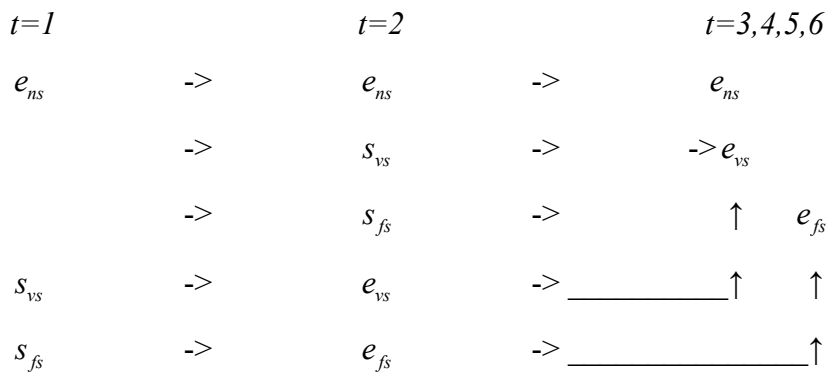
Source: Regional AF HomePages: www. af.dk.

New subsidized apprentices devided into industries in 1997 & 2005

	1997		2005	
	Men Pct	Women Pct	Men Pct	Women Pct
Total	100	100	100	100
Educational	0	0	0,62	2,47
Office and trade	24,7	39,98	18,77	32,13
Building and construction	20,31	2,1	29,33	4,66
Iron,steel and metal	24,85	1,63	23,43	3,49
Graphics	1,15	0,61	2,19	0,8
Industrial engin. And other	1,07	2,14	1,77	1,79
Service	0,47	3,25	1,7	7,93
Food and domestic	7,48	10,3	7,89	9,88
agricultural and fishing	12,03	4,72	6,44	4,26
Transportation	5,43	0,41	5,05	0,56
Health	2,5	34,86	2,77	32,03
Safety/security	0	0	0,04	0,01

Source: Dream register on AAS 1997-2005

Diagram 1: Educational pathways



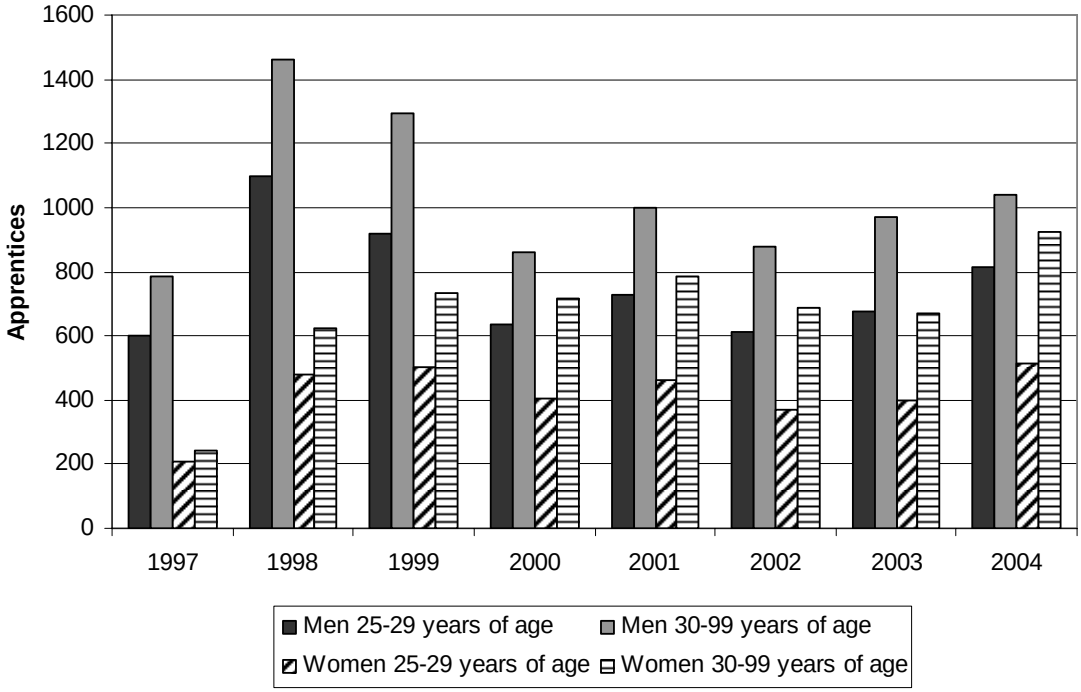
Alternative illustration

Path	**Reward at** $t=1$	**Reward at** $t=2$ **Discounted:δ**	**Reward at** $t=3,4,5,6$ **Discounted: δ² +δ³ +δ⁴+δ⁵**
Unskilled	w_{ns}	w_{ns}	w_{ns}
Apprentice (<25)	$-c_{vs}-ic1_{vs,i}-ic2_{1,i}$	w_{vs}	w_{vs}
Apprentice with AAS (>25)	w_{ns}	$-c_{vs}-ic1_{vs,i}-ic2_{2,i}+aid_{vs,2}$	w_{vs}
Further education (<25)	$-c_{fs}-ic1_{fs,i}-ic2_{1,i}$	w_{fs}	w_{fs}
Further education (>25)	w_{ns}	$-c_{fs}-ic1_{fs,i}-ic2_{2,i}$	w_{fs}

Source: Weatherall (2007)

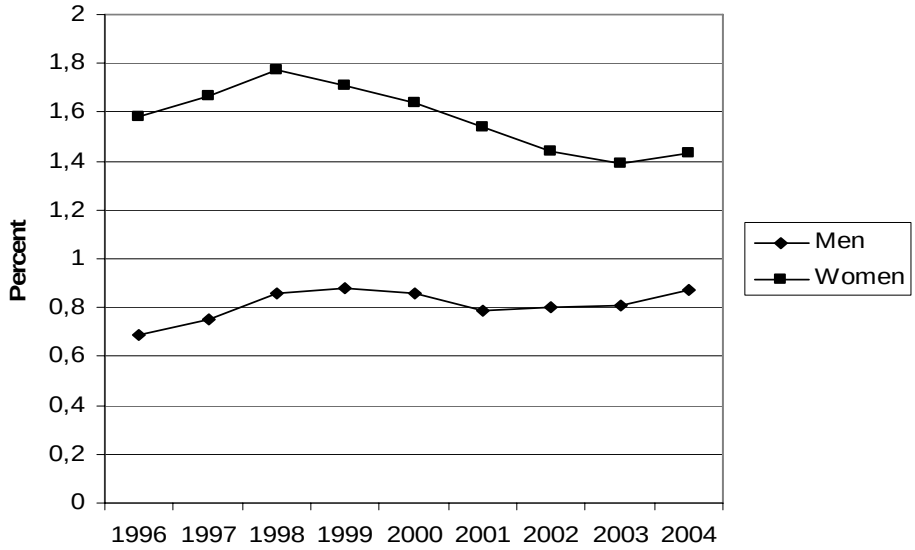
Figures

Figure 1: Persons participating in subsidized apprenticeship from 1997-2004.



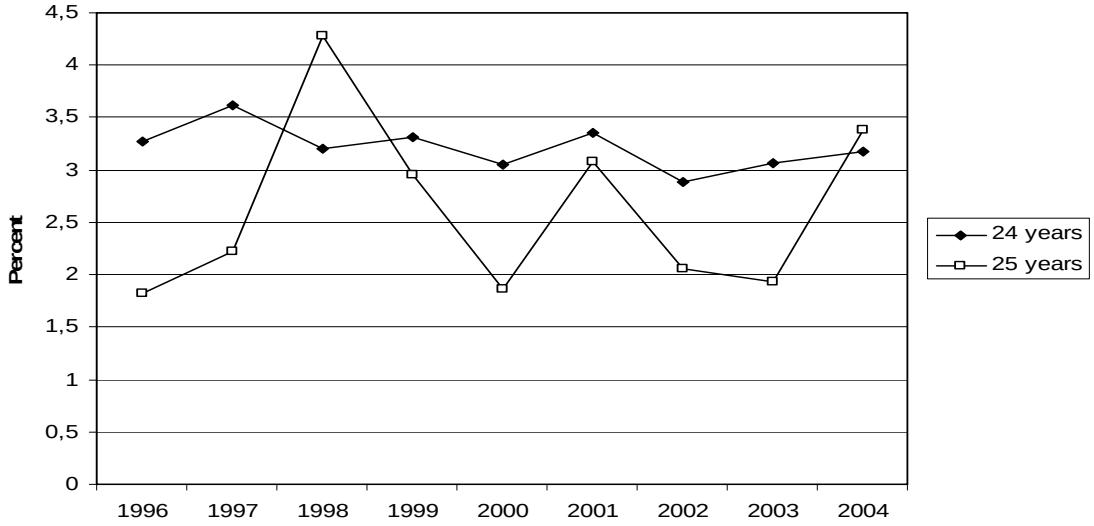
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 2: Persons starting apprenticeship out of the population between 25-39 years of age from 1996-2004.



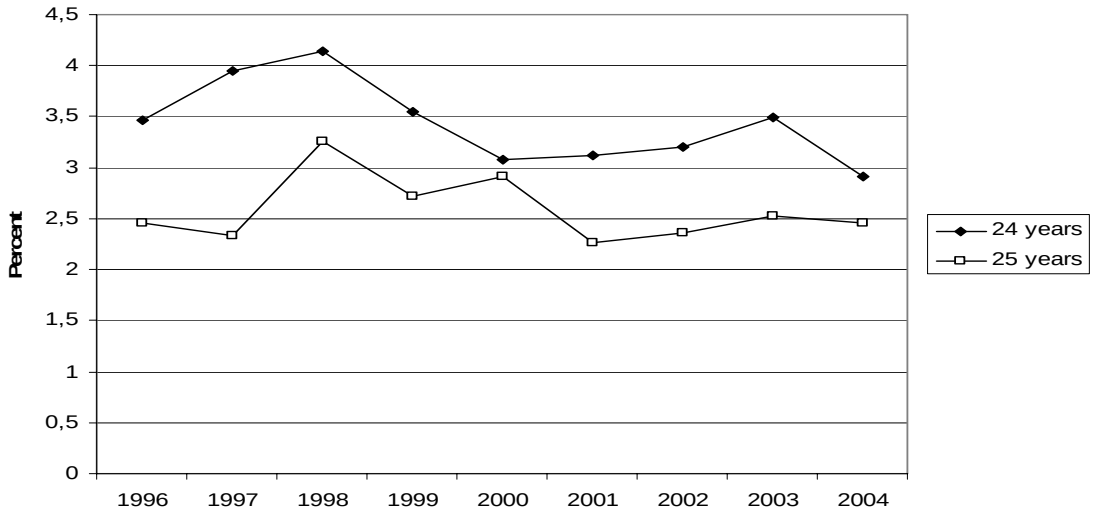
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 3. New male apprentices among people not already in education or have not finished an education by age from 1996-2004.



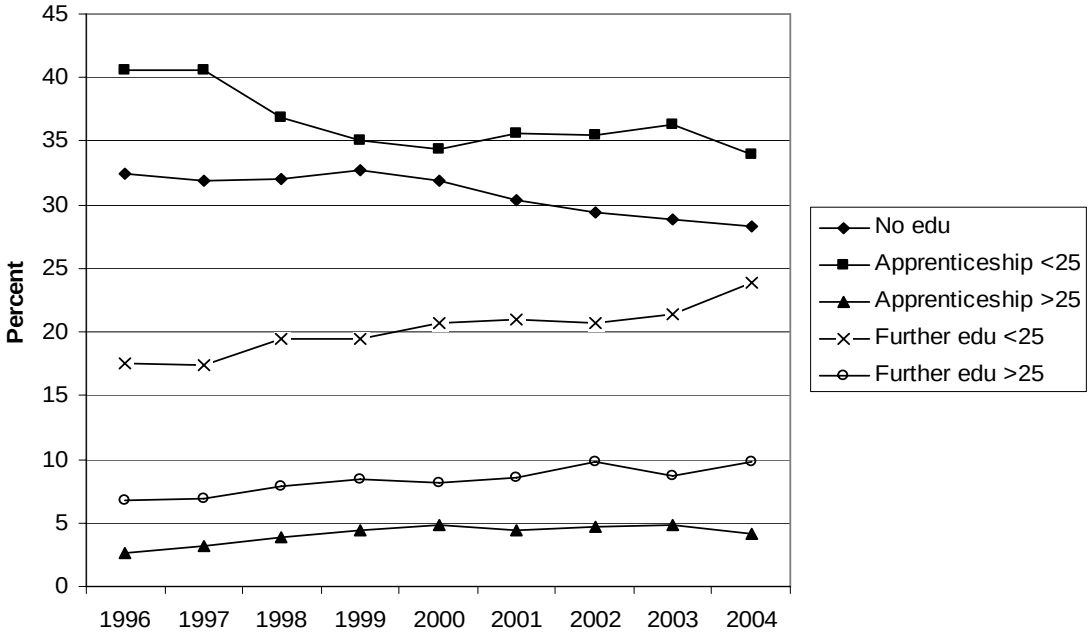
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 4. New female apprentices among people not already in education or have not finished an education by age from 1996-2004.



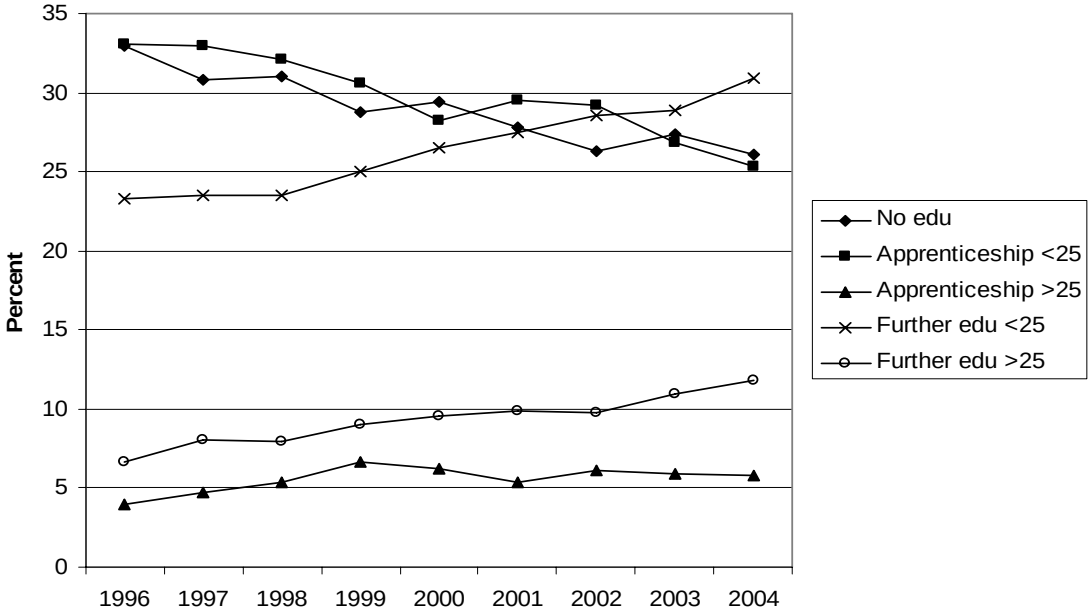
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 5. Educational distribution in Denmark for men over 30 years of age from 1996-2004



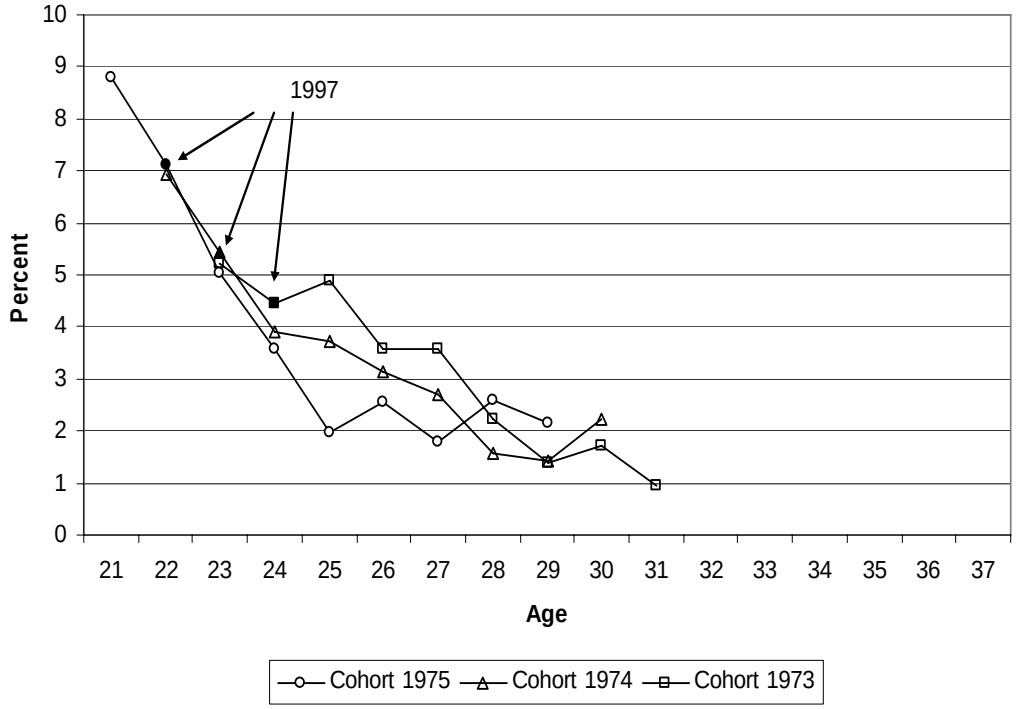
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 6. Educational distribution in Denmark for women over 30 years of age from 1996-2004



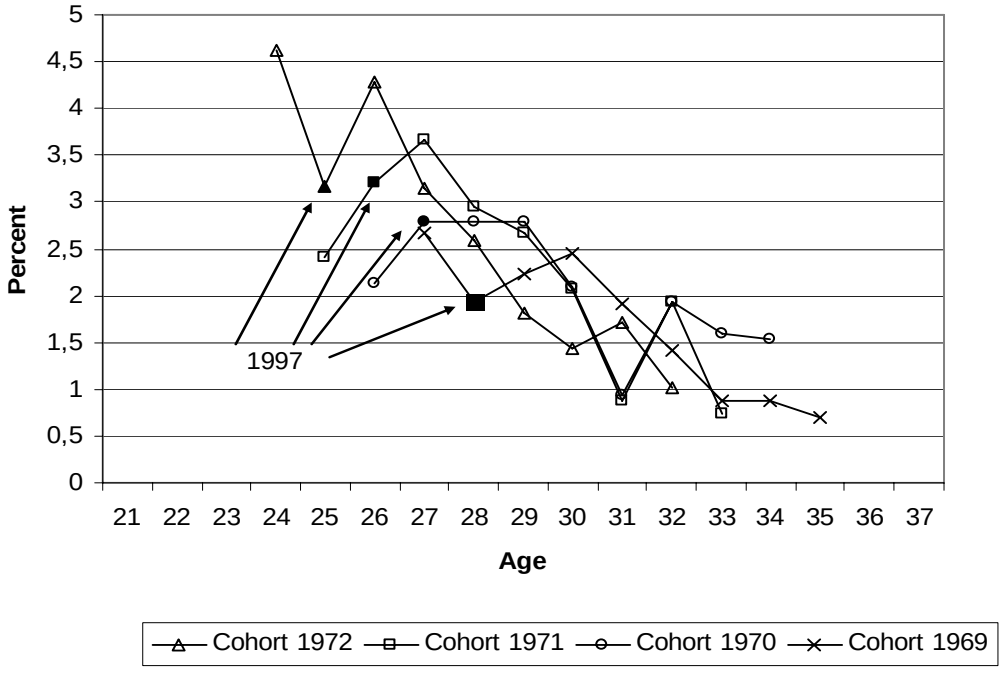
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 7. Cohort 1973-1975 vocational attendance rates for men from 1996-2004



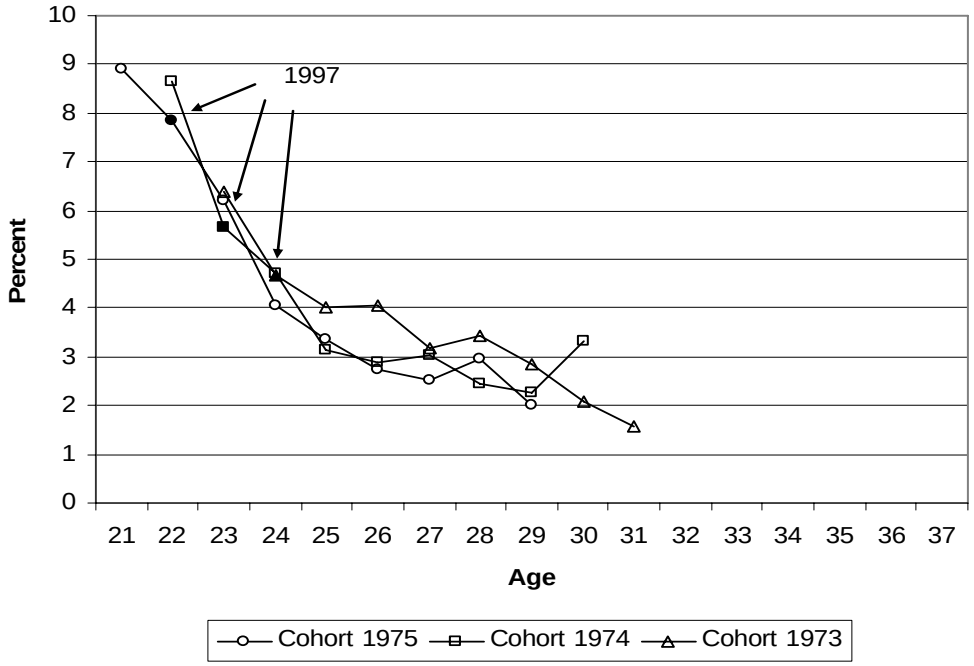
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figur 8. Cohort 1967-1972 vocational attendance rates for men from 1996-2004



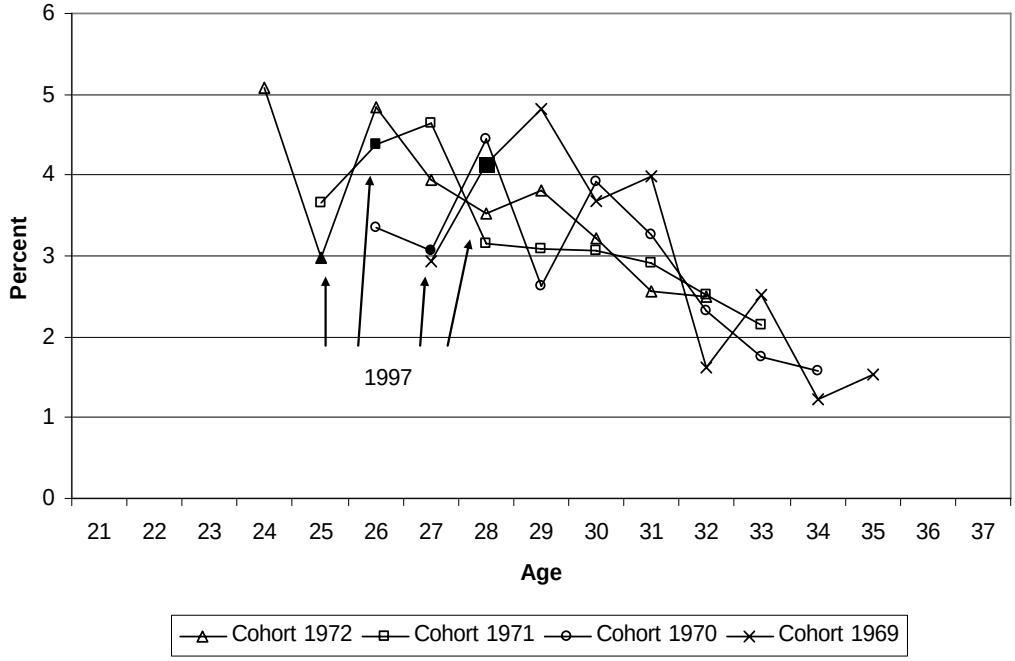
Source: Statistics Denmark register panel data from 1995 to 2004

Figure 9. Cohort 1973-1975 vocational attendance rates for women from 1996-2004



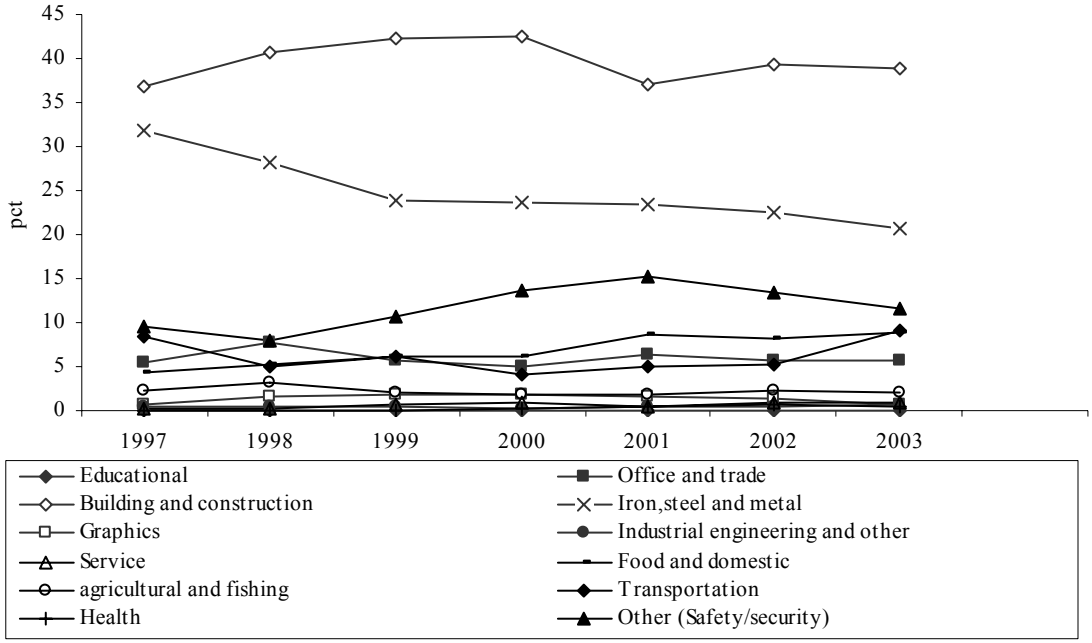
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figur 10. Cohort 1967-1972 vocational attendance rates for women from 1996-2004



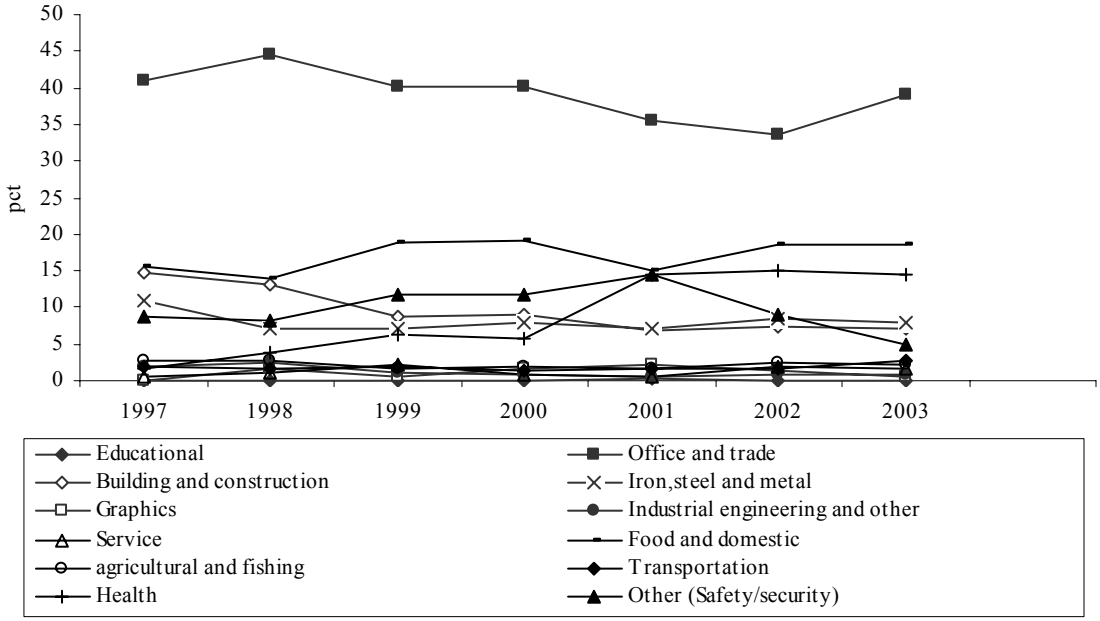
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 11. Subsidized apprenticeship by educational field for men from 1997-2003



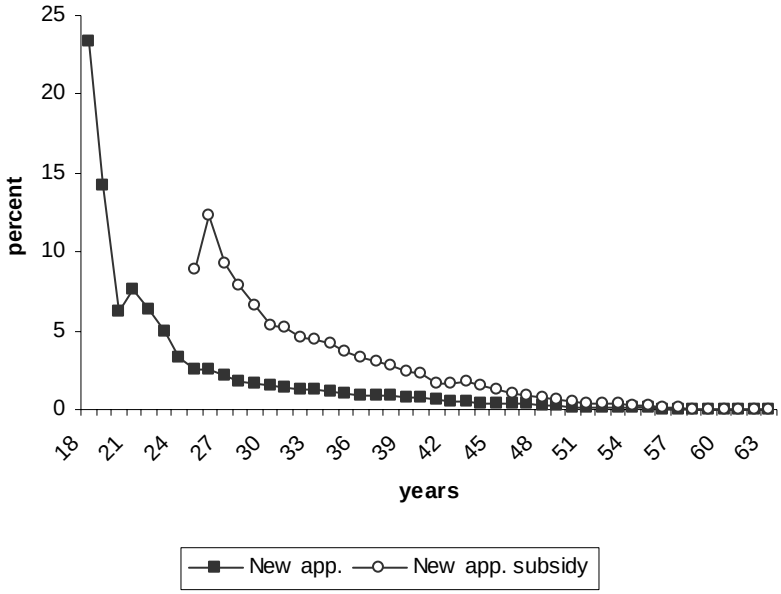
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 12. Subsidized apprenticeship by educational field for women from 1997-2003



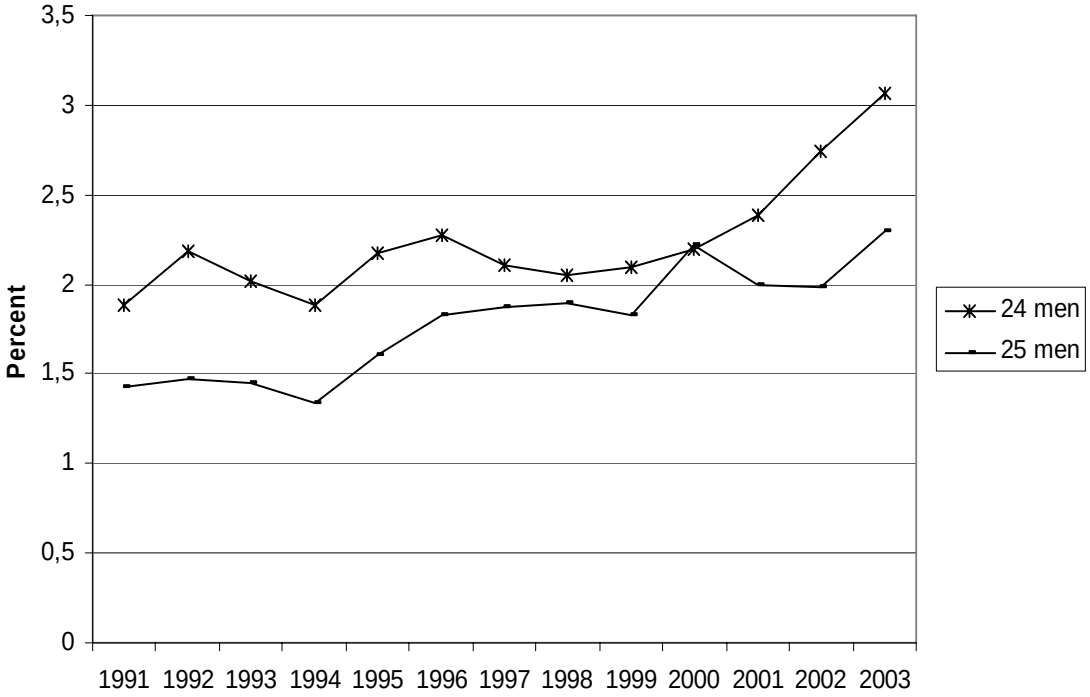
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figure 13. AAS Age distribution



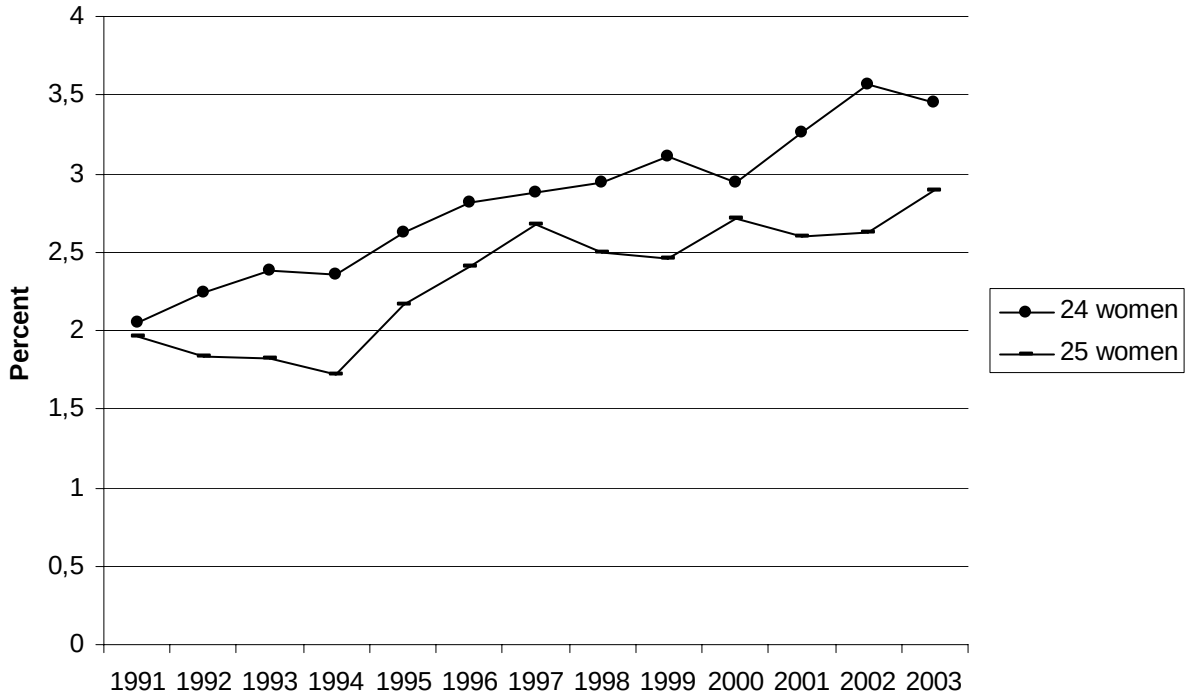
Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Figur 14. Attendance rate for men between 24-25 years of age from 1991-2003



Source: Statistics Denmark Data Bank (1991-2003)

Figur 15. Attendance rate for women between 24-25 years of age from 1991-2003



Source: Statistics Denmark Data Bank (1991-2003)

Tables

Table 1. Pct. of new apprenticeships for adults over 25 years of age that is subsidized/ not subsidized in Greater Copenhagen 2004.

New starters	Not "bottleneck"	"bottleneck"	"bottleneck" (main category)	No info on edu.field	Pct
Apprenticeship	22,16	75,62	2,22	0	100
Apprenticeship with AAS	8,86	60,76	2,53	27,85	100
Observations	87	321	10	22	440

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 2: Student pay and apprentice pay in gross values for 2006

	Carpenter	Carpenter with AAS	Economist
Average length	3,5 years	3,5 years	5 years
Hourly pay*		101,40 Dkr	
1. period (26 weeks)**	49,55 Dkr		
2. period (52 weeks)	59,90 Dkr		
3. period (52 weeks)	68,20 Dkr		
4. period (52 weeks)	82,30 Dkr		
Average monthly pay (37,5h pr week)	10.919 Dkr	15.210 Dkr	4.852 Dkr
Average pay for the whole education period	458.591 Dkr	638.820 Dkr	291.120 Dkr
Hourly minimum wage when education finished *	101,40 Dkr	101,40 Dkr	153,60 Dkr
Average reimbursement pr m pr trainee in school*	8.500 Dkr	14.640 Dkr	
Hourly subsidy to employer (max 2,5 years)		35 Dkr	
Minimum age	16 years	25 years	18 years
Other conditions		only local bottlenecks industries	Extra funding 12m
Average employer cost taking subsidy (2,5 years) and reimbursement (6 m of schooling) into account	407.591 Dkr	393.480 Dkr	

** 26 is set by the author. Schooling time is between 6 and 11 month including introduction courses. The introduction course is not subsidized, thus in that period the vocational trainee and the adult vocational trainee are paid the same. Thus the introduction period is not included in this example.

*Source: Vocational education: Carpenter, Dansk Byggeri Bygningsoverenskomst. Further education: Economist, DJØF, Monthly wage (basistrin 3) in the public sector inclusive pension 23033,50/(37,5*4)*

Table 3. Relationship between "real life" numbers and scenarios with different costs/no subsidy

Important parameters		Related "real life" numbers for 1996*	Scenario with independent costs	Scenario with dependent cost
Wage	no education	160 Dkr/hour	160 Dkr/hour	160 Dkr/hour
	voc.education	190 Dkr/hour	190 Dkr/hour	190 Dkr/hour
	fur.education	260 Dkr/hour	260 Dkr/hour	260 Dkr/hour
Discount rate			0,9	0,9
Educational cost¨ Distribution	voc. period 1		N(0,170)	N(40,700)+N(0,90)
	voc. period 2		N(40,70)	N(40,700)+N(70,70)
	fur. period 1		N(220,30)	N(290,340)+N(0,90)
	fur. period 2		N(260,90)	N(290,340)+N(70,70)
Educational distribution	No education	32,68 pct	33,41 pct	31,77 pct
	voc. period 1	36,93 pct	36,17 pct	35,72 pct
	voc. period 2	3,27 pct	3,33 pct	4,44 pct
	fur. period 1	20,37 pct	20,51 pct	25,94 pct
	fur. period 2	6,75 pct	6,59 pct	2,14 pct

* Wage: hourly average wage from private sector 2000 (because no number available from 1996). The relative relationship between has not changed drasticly. Educational distribution: Is the education distribution among the 30 years old in 1996.

Source: Statistics Denmark Data Bank (1991-2003) & Weatherall(2007)

Table 4. The result of an adult vocational education subsidy in different cost scenarios

Education	Independent cost, full info t=0			Independent cost, info t=1	Dependent cost, full info t=0		
	No subsidy	Subsidy 10pct	Subsidy 40pct	Subsidy 40pct	No subsidy	Subsidy 10pct	Subsidy 40pct
No edu	33,41	33,18	32,27	32,92	31,77	31,68	31,36
Vocational period1	36,17	36,13	36,02	36,17	35,72	35,17	32,90
Vocational period2	3,33	3,73	5,09	3,87	4,44	5,13	7,85
Further period1	20,51	20,41	20,14	20,51	25,94	25,92	25,84
Further period2	6,59	6,56	6,48	6,53	2,14	2,10	2,05
no. obs	30000	30000	30000	30000	30000	30000	30000

Source: Weatherall (2007)

Table 5. The "mobility" changes due to a subsidy in a scenario with dependent costs

Subsidy 40pct / no subsidy	no edu	voc period1	Voc period2	Fur period1	fur period2	Total obs
No edu	98,71	0	1,29	0	0	9530
voc period1	0	92,12	7,88	0	0	10715
voc period2	0	0	100,00	0	0	1332
Fur period1	0	0	0,40	99,60	0	7782
Fur period2	0	0	4,06	0	95,94	641
Total pct	31,36	32,90	7,85	25,84	2,05	30000 / 100

Source: Weatherall (2007)

Table 6. Subsidized apprenticeship by region and educational field for men in 1998

	education	off. & tra.	build. & const.	iron etc.	graph.	Indu. eng.
Stor Kbh	0	8,59	45,4	17,79	1,23	0
Frederiksborg	0	6,82	44,7	20,45	0,76	0,76
Roskilde	0	12,09	45,05	12,09	2,2	0
Vestsjælland	0	10,71	40,71	25,71	1,43	0
Storstrøm	0	4,82	40,96	21,08	1,81	0,6
Bornholm	0	5,56	22,22	27,78	0	0
Fyn	0	11,36	39,55	25,45	1,36	0,45
Sønderjylland	0	7,1	34,91	36,09	1,18	0
Ribe	0	4,48	32,09	37,31	2,24	1,49
Vejle	0	5,06	28,09	42,7	2,81	0,56
Ringkøbing	0	0	43,68	36,21	0	0
Århus	0	6,18	41,01	35,39	1,69	0
Viborg	0	0,89	55,36	34,82	0	0
Nordjylland	0	9,43	40,38	29,81	2,26	1,13
Kbh & Fredriksb	0	15,51	43,67	11,84	2,45	0,82

Continued

	service	food & dom.	agri. & fish.	transport	health	other
Stor Kbh	0	2,45	9,2	7,98	0,61	6,75
Frederiksborg	0	6,82	6,82	1,52	0	11,36
Roskilde	0	4,4	10,99	2,2	0	10,99
Vestsjælland	0	2,14	5	6,43	0	7,86
Storstrøm	0	4,82	1,81	18,67	0	5,42
Bornholm	0	27,78	0	0	0	16,67
Fyn	1,36	6,36	1,82	6,36	0	5,91
Sønderjylland	0	2,96	0	6,51	0	11,24
Ribe	0	3,73	0,75	5,97	0	11,94
Vejle	0	6,18	1,12	1,12	0	12,36
Ringkøbing	0	5,17	2,87	3,45	0	8,62
Århus	0,28	4,49	3,09	1,69	0	6,18
Viborg	0	3,57	0	1,79	0	3,57
Nordjylland	0	6,42	2,26	0	0	8,3
Kbh & Fredriksb	0,41	7,76	3,27	8,16	0,41	5,71

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 7. Subsidized apprenticeship by region and educational field for women in 1998

	education	off. & tra.	build. & const.	iron etc.	graph.	Ind. eng.
Stor Kbh	0	59,42	7,25	1,45	0	4,35
Frederiksborg	0	48,08	7,69	5,77	5,77	5,77
Roskilde	0	59,52	7,14	0	0	0
Vestsjælland	0	59,02	4,92	6,56	4,92	1,64
Storstrøm	0	40,58	7,25	10,14	0	1,45
Bornholm	0	38,24	5,88	0	0	0
Fyn	0	43,1	14,66	10,34	0,86	2,59
Sønderjylland	0	42,5	16,25	7,5	1,25	0
Ribe	0	17,02	27,66	2,13	4,26	10,64
Vejle	0	33,82	14,71	23,53	0	1,47
Ringkøbing	0	0	42,42	15,15	0	6,06
Århus	0	41,48	17,78	8,89	2,22	1,48
Viborg	0	23,08	30,77	12,82	0	5,13
Nordjylland	0	57,36	11,63	5,43	1,55	0
Kbh & Fredriksb	0	53,17	3,17	0	2,38	2,38

continued

	service	food & dom.	agri. & fish.	transport	health	other
Stor Kbh	2,9	11,59	2,9	2,9	1,45	5,8
Frederiksborg	0	19,23	1,92	1,92	3,85	0
Roskilde	2,38	14,29	4,76	2,38	2,38	7,14
Vestsjælland	0	4,92	1,64	3,28	4,92	8,2
Storstrøm	0	14,49	5,8	8,7	4,35	7,25
Bornholm	0	2,94	0	0	0	52,94
Fyn	0,86	11,21	1,72	2,59	8,62	3,45
Sønderjylland	1,25	18,75	1,25	1,25	2,5	7,5
Ribe	0	34,04	0	2,13	0	2,13
Vejle	4,41	8,82	2,94	0	4,41	5,88
Ringkøbing	0	24,24	0	0	6,06	6,06
Århus	0,74	10,37	4,44	0	0,74	11,85
Viborg	0	12,82	5,13	0	0	10,26
Nordjylland	0	12,4	1,55	0	3,1	6,98
Kbh & Fredriksb	3,17	17,46	4,76	0	7,14	6,35

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 8. Descriptive statistics for subsidized and non-subsidized apprenticeships between 1997-2004

		Men		Women	
		Not subsidized	Subsidized	Not subsidized	Subsidized
Years	**1997**	16,5	13,15	15,62	7,4
	1998	15,53	23,37	16,02	15,89
	1999	14,94	18,65	14,61	17,81
	2000	14,23	10,6	14,63	15,89
	2001	14,82	12,9	14,36	16,99
	2002	11,26	10,47	12,6	11,78
	2003	12,72	10,86	12,15	14,25
Age	**<25 years**	84,81	0	64,44	0
	25-31 years	9,05	54,15	14,82	52,6
	32-51 years	5,9	44,83	19,21	45,75
	51+ years	0,25	1,02	1,53	1,64
Family Status	**Single**	52,51	41	39,06	18,63
	Single parent	6,48	1,02	10,49	15,62
	Couple	7,06	22,61	19,82	22,47
	Couple with children	32,77	35,38	29,57	43,29
	Child not at home	1,18	0	1,06	0
Ethnicity	**Danish**	93,53	94	94,08	95,89
	Immigrant	6,47	6	5,92	4,11
Education prev. y.	**No education**	98,34	97,19	97,59	97,53
	Short further edu	0,49	0,89	0,42	0
	Middle further edu	1,02	1,4	1,86	2,19
	Long further edu	0,14	0,51	0,13	0,27
Occupation prev. y.	**Employer & self emp.**	0,54	2,43	0,3	0,27
	Wage earner	51,99	72,54	53,73	53,15
	Unemployed	1,38	9,83	2,49	18,36
	Out of labour market	3,1	10,73	7,87	18,08
	Student (basic)	42,98	4,47	35,61	10,14
Geographical area	**Stor Kbh**	9,22	6,77	9,26	6,58
	Frederiksborg	5,52	4,34	5,34	2,74
	Roskilde	4,22	2,68	4,04	1,37
	Vestsjælland	5,93	4,09	6,64	6,03
	Storstrøm	5,56	5,62	5,22	6,58
	Bornholm	0,98	0,77	0,94	0,27
	Fyn	10,35	9,96	9,51	13,7
	Sønderjylland	5,47	3,07	5,16	5,21
	Ribe	4,81	4,6	4,89	4,93
	Vejle	7,03	7,28	7,09	10,96
	Ringkøbing	6,22	10,34	5,76	6,03
	Århus	11,79	14,94	12,33	11,51
	Viborg	4,99	6	5,01	4,11
	Nordjylland	10,22	9,32	9,29	11,23
	Kbh & Frederiksberg	7,71	10,22	9,52	8,77

Continued

Educational field	**Education**	0,49	0	3,5	0
	Office & Trade	25,51	5,75	43,17	33,7
	Building & construction	27,4	39,21	2,81	12,33
	Iron, steel & metal	25,88	27,33	1,87	11,78
	Graphic	1,57	0,89	1	0,55
	Industry engineer	1,19	0,64	2,18	1,37
	Service	0,5	0,38	4,42	1,37
	Food & domestic prod	7,55	6,26	9,33	18,9
	Agricultural & fishing	4,47	1,92	2,82	1,92
	Transport	4,07	6,51	0,49	2,74
	Health	1,36	0	28,41	7,4
	Other	0	11,11	0	7,95
Years of experience	**Mean**	1,566011	8,08046	2,817961	6,131507
	Sta.dev.	3,060407	5,309107	4,264548	4,585479
Previous a-income	**Mean**	62960,58	169144	81701,09	129772,8
	Sta.dev.	60627,47	87462,99	62408,58	66007,52
Wage prev. y.	**Mean**	51498,57	135634,6	54486,2	69377,44
	Sta.dev.	57479,94	104835,8	53658,7	81106,13

Note: A-income is total taxable income

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 9. New apprenticeship, men 24 -25 years of age from 1996-1998.

Men		1996		1998	
		24 years	25 years	24 years	25 years
Family status	**Single**	71,64	68,57	73,44	59,49
	Single parent	0	0	0	1,27
	Couple	23,88	20	25	31,65
	Couple with children	4,48	11,43	1,56	7,59
Ethnicity	**Danish**	98,51	97,14	96,88	91,14
	Immigrant	1,49	2,86	3,12	8,86
Education prev. y.	**No education**	95,52	91,43	93,75	92,41
	Short further edu	0	0	1,56	1,27
	Middle further edu	4,48	8,57	4,69	5,06
	Long further edu	0	0	0	1,27
Occupation prev. y.	**Employer & self emp.**	0	0	1,56	0
	Wage earner	76,12	80	65,62	78,48
	Unemployed	7,46	8,57	3,12	2,53
	Out of labour market	1,49	2,86	7,81	1,27
	Student (basic)	14,93	8,57	21,88	17,72
Geographical area	**Stor Kbh**	7,46	2,86	4,69	7,59
	Frederiksborg	2,99	2,86	7,81	3,8
	Roskilde	7,46	2,86	1,56	1,27
	Vestsjælland	5,97	0	6,25	3,8
	Storstrøm	5,97	2,86	6,25	6,33
	Bornholm	0	2,86	0	1,27
	Fyn	13,43	11,43	9,38	5,06
	Sønderjylland	7,46	2,86	4,69	2,53
	Ribe	5,97	8,57	4,69	1,27
	Vejle	7,46	8,57	9,38	8,86
	Ringkøbing	2,99	2,86	4,69	2,53
	Århus	10,45	22,86	12,5	20,25
	Viborg	2,99	5,71	0	2,53
	Nordjylland	4,48	11,43	15,62	13,92
	Kbh & Frederiksberg	14,93	11,43	12,5	18,99
Educational field	**Education**	0	0	0	1,27
	Office & Trade	53,73	48,57	35,94	35,44
	Building & construction	14,93	14,29	25	17,72
	Iron, steel & metal	10,45	20	14,06	18,99
	Graphic	0	0	3,12	1,27
	Industry engineer	4,48	2,86	0	2,53
	Service	1,49	0	0	0
	Food & domestic prod	4,48	2,86	7,81	8,86
	Agricultural & fishing	7,46	5,71	7,81	7,59
	Transport	1,49	5,71	4,69	2,53
	Health	1,49	0	1,56	2,53
	Other	0	0	0	1,27

Continued

Years of experience	**Mean**	2,223881	2,971429	2,46875	2,835443
	Sta.dev.	1,485494	2,00713	1,563155	1,897572
Previous a-income	**Mean**	107699,4	120800,3	104023,3	131881,7
	Sta.dev.	38314,68	36548,38	35147,11	63666,04
Wage prev. y.	**Mean**	83713,22	93343	71662,56	111091,8
	Sta.dev.	51569,28	53202,62	51040,7	77530,22

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 10. New apprenticeship, men 24 -25 years of age from 1996-1998.

Women		1996		1998	
		24 years	25 years	24 years	25 years
Family status	**Single**	53,57	45,28	45,45	42,25
	Single parent	5,95	13,21	5,05	11,27
	Couple	39,29	33,96	37,37	30,99
	Couple with children	1,19	7,55	12,12	15,49
Ethnicity	**Danish**	97,62	100	91,92	92,96
	Immigrant	2,38	0	8,08	7,04
Education prev. y.	**No education**	95,24	92,45	89,9	90,14
	Short further edu	0	0	0	0
	Middle further edu	3,57	7,55	8,08	9,86
	Long further edu	1,19	0	2,02	0
Occupation prev. y.	**Employer & self emp.**	0	0	1,01	0
	Wage earner	80,95	75,47	68,69	73,24
	Unemployed	2,38	3,77	4,04	2,82
	Out of labour market	0	0	2,02	1,41
	Student (basic)	16,67	20,75	24,24	22,54
Geographical area	**Stor Kbh**	5,95	5,66	8,08	7,04
	Frederiksborg	7,14	7,55	2,02	2,82
	Roskilde	3,57	13,21	9,09	5,63
	Vestsjælland	1,19	1,89	5,05	7,04
	Storstrøm	3,57	5,66	3,03	5,63
	Bornholm	2,38	0	2,02	0
	Fyn	2,38	7,55	8,08	9,86
	Sønderjylland	4,76	7,55	4,04	2,82
	Ribe	5,95	3,77	5,05	1,41
	Vejle	8,33	5,66	4,04	1,41
	Ringkøbing	7,14	5,66	1,01	5,63
	Århus	16,67	7,55	18,18	21,13
	Viborg	2,38	1,89	3,03	1,41
	Nordjylland	10,71	11,32	10,1	12,68
	Kbh & Frederiksberg	17,86	15,09	17,17	15,49
Educational field	**Education**	0	0	0	7,04
	Office & Trade	54,76	28,3	35,35	36,62
	Building & construction	1,19	1,89	3,03	1,41
	Iron, steel & metal	0	1,89	2,02	1,41
	Graphic	1,19	1,89	5,05	0
	Industry engineer	3,57	7,55	3,03	4,23
	Service	5,95	1,89	7,07	2,82
	Food & domestic prod	4,76	9,43	12,12	11,27
	Agricultural & fishing	1,19	5,66	3,03	2,82
	Transport	0	0	0	1,41
	Health	27,38	41,51	29,29	30,99
	Other	0	0	0	0

Continued

Years of experience	**Mean**	2,095238	2,132075	1,818182	2,239437
	Sta.dev.	1,266996	1,569392	1,146099	1,448791
Previous a-income	**Mean**	97311,6	107711,1	97952,44	108297
	Sta.dev.	25987,61	33561,27	29926,6	38945,75
Wage prev. y.	**Mean**	75031,99	73007,19	69720,43	72918,28
	Sta.dev.	36780,52	45476,57	43659,8	51522,98

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 11. Difference in differences for men

	1996	**1998**	**Difference**
24 years apprentices	3,28	3,34	-0,06
25 years apprentices	1,82	4,39	-2,57
Difference	1,46	-1,05	2,51

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 12. Difference in differences for women

	1996	**1998**	**Difference**
24 years apprentices	3,46	4,31	-0,85
25 years apprentices	2,45	3,34	-0,89
Difference	1,01	0,97	0,04

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 13. Difference in differences for men from 1996-1998

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,0005879	0,0055769		-0,0158113	0,0055863	***	-0,0149715	0,0056249	***	-0,0150963	0,00502	***
1996	ref.			ref.			ref.			ref.		
1998	-0,0145621	0,0055739	***	0,0018565	0,0055759		-0,0223738	0,0271131		-0,0332218	0,02705	
25*1998	0,0251003	0,0080137	***	0,0259448	0,0079689	***	0,023974	0,008107	***	0,0268847	0,0101	***
Dane				ref.			ref.			ref.		
Immigrant				-0,0001108	0,0089459		-0,0058848	0,0141781		-0,0080738	0,01129	
Single				ref.			ref.			ref.		
Couple				0,0091865	0,0051536	*	0,0062644	0,0071957		0,0071078	0,00688	
Couple & children				-0,004177	0,0102781		-0,0068957	0,0143223		-0,0079689	0,01129	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0061882	0,0095603		-0,0077221	0,0112495		-0,0032727	0,00804	
Out of lab.force				-0,0460124	0,006795	***	-0,0437695	0,0096671	***	-0,0304015	0,0035	***
Student				-0,0291484	0,0064151	***	-0,0363918	0,0091612	***	-0,0236731	0,0039	***
Prev. Income				-3,33E-07	4,37E-08	***	-3,23E-07	6,27E-08	***	-3,06E-06	0	***
Work experience				0,0051421	0,0014916	***	0,0030367	0,0020277		0,003641	0,00175	**
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				0,0178869	0,0118698		0,0046492	0,0162942		0,0063495	0,01946	
Roskilde				0,0161829	0,0130347		0,0332117	0,0177354	*	0,046358	0,0341	
Vestsjælland				0,0226543	0,0119518	*	0,0133357	0,0160924		0,0191672	0,02429	
Storstrøm				0,0358411	0,0119935	***	0,0231024	0,0164596		0,030593	0,02714	
Fyn				0,0147991	0,0092522		0,019921	0,0126278		0,0282458	0,02058	
Sønderjylland				0,0278612	0,0129654	**	0,035562	0,0179327	**	0,0524949	0,03671	
Ribe				0,0251263	0,0127808	**	0,0402032	0,0175072	**	0,0588555	0,03747	
Vejle				0,0343108	0,010767	***	0,0282679	0,0150694	*	0,0497836	0,03113	
Ringkøbing				0,0114406	0,0125148		0,0092371	0,0167823		0,0158452	0,02467	
Århus				0,0126842	0,0081489		0,0108735	0,0114319		0,0162544	0,01544	
Viborg				0,0112186	0,0136014		0,0212565	0,018347		0,0316425	0,03126	
Nordjylland				0,0193987	0,0091062	**	0,0063266	0,0127589		0,009294	0,01602	
Cph & Frederiksb.				-0,0012223	0,0075388		0,0004115	0,0106184		-0,0000798	0,01087	

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Immigrant*1998							0,0115363	0,0183218		0,0134271	0,01794	
Couple*1998							0,0062056	0,0103222		0,0008671	0,00801	
Couple&child*1998							0,0038266	0,0206072		0,0079338	0,02449	
Unemployed*1998							0,0019803	0,0218295		0,0000393	0,01604	
Out of lab.for.*1998							-0,0043624	0,013627		0,0270547	0,02968	
Student*1998							0,0143017	0,0128546		0,0284736	0,01792	
Prev. Income*1998							-2,92E-08	8,78E-08		1,02E-06	0	
Work exp.*1998							0,0048666	0,0030091		0,0015305	0,00238	
Frederiksborg*1998							0,0285624	0,0238098		0,0204681	0,03407	
Roskilde*1998							-0,0370296	0,0261763		-0,0184944	0,00593	***
Vestsjælland*1998							0,0213447	0,0240852		0,0096121	0,02636	
Storstrøm*1998							0,0271544	0,0240436		0,0068894	0,02284	
Fyn*1998							-0,0125398	0,0185794		-0,0107976	0,00964	
Sønderjylland*1998							-0,016635	0,0259807		-0,0126853	0,01024	
Ribe*1998							-0,0326307	0,0256463		-0,0165827	0,00699	**
Vejle*1998							0,0124747	0,0215521		-0,0050859	0,01377	
Ringkøbing*1998							0,0043777	0,0252415		-0,0027477	0,01929	
Århus*1998							0,0044117	0,0163253		-0,0012818	0,01378	
Viborg*1998							-0,0227753	0,0273579		-0,015055	0,00962	
Nordjylland*1998							0,0266463	0,0182327		0,0157197	0,02402	
Cph & Fred.*1998							-0,0028496	0,0151055		-0,0005159	0,01398	
Constant	0,0327628	0,0038803	***	0,0608346	0,0091847	***	0,0670165	0,012235	***			
Obs	7687			7687			7687			7687		
Adj R2	0,0022			0,0146			0,0149			0,0777		

Source: Statistics Denmark register panel data from 1995 to 2004 &Dream register on AAS 1997-2005

Table 14. Difference in differences for women from 1996-1998.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,0084981	0,0052821		-0,0098945	0,005357	*	-0,0095025	0,0053883	*	-0,0030283	0,00173	*
1996												
1998	-0,0100908	0,0053669	*	0,0087732	0,0052644	*	-0,0082374	0,0269884		-0,0090105	0,01016	
25*1998	0,0003339	0,0076554		0,0011039	0,0075933		0,0004126	0,0076817		0,0005737	0,00241	
Dane				ref.			ref.			ref.		
Immigrant				0,0064988	0,0082241		-0,0050312	0,0119181		-0,0035855	0,0038	
Single				ref.			ref.			ref.		
Couple				0,0068794	0,0043708		0,001874	0,0060845		0,0001624	0,00185	
Couple & children				0,0093798	0,0066378		-0,0001936	0,0093187		-0,0011192	0,00327	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0265183	0,0092483	***	-0,0321726	0,0108503	***	-0,006371	0,00152	***
Out of lab.force				-0,0598068	0,006046	***	-0,0549118	0,0084287	***	-0,0819374	0,00457	***
Student				-0,0368977	0,005843	***	-0,0387061	0,0083676	***	-0,0081825	0,00176	***
Prev. Income				-3,89E-07	5,65E-08	***	-3,84E-07	7,94E-08	***	-1,03E-06	0	***
Work experience				0,0041906	0,0016653	**	0,0040834	0,0022802	*	0,0014993	0,00072	**
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				0,0085899	0,0117287		0,0314613	0,0160121	**	0,0158107	0,01213	
Roskilde				0,0565103	0,0128472	***	0,0429169	0,0168873	**	0,0247019	0,01619	
Vestsjælland				0,0082491	0,0118986		-0,0019805	0,0170704		-0,0015329	0,00563	
Storstrøm				0,0259607	0,0119768	**	0,0282769	0,0162862	*	0,0155521	0,01262	
Fyn				-0,0061267	0,0089675		-0,0058802	0,0124367		-0,0018507	0,00391	
Sønderjylland				0,0161985	0,0123024		0,0291645	0,0167683	*	0,0140544	0,01201	
Ribe				0,0128281	0,0123314		0,0259373	0,0169329		0,0143173	0,0124	
Vejle				0,0009565	0,010545		0,0167754	0,0142796		0,0079066	0,00818	
Ringkøbing				0,0044254	0,0115453		0,0216558	0,0156824		0,0095522	0,00939	
Århus				0,0060518	0,0082986		0,002523	0,0112724		0,0013875	0,00439	
Viborg				-0,0042741	0,0128683		-0,0001925	0,0178102		0,0002452	0,0064	
Nordjylland				0,0139661	0,0093507		0,0213083	0,0129516	*	0,010659	0,00833	
Cph & Frederiksb.				-0,009727	0,0077508		-0,0004264	0,0106968		0,0003988	0,00389	

Continued

continued

Immigrant*1998							0,0218507	0,0164753		0,0101422	0,00674	
Couple*1998							0,0109523	0,0087489		0,0036134	0,00328	
Couple&child*1998							0,0196577	0,0132855		0,009901	0,0088	
Unemployed*1998							0,0329645	0,0213698		0,0195701	0,01834	
Out of lab.for.*1998							-0,0104216	0,0120999		0,9212091	0,00681	***
Student*1998							0,003583	0,0117023		0,0030111	0,00416	
Prev. Income*1998							-5,00E-09	1,13E-07		2,09E-07	0	
Work exp.*1998							-8,65E-06	0,0033469		-0,0003651	0,00098	
Frederiksborg*1998							-0,0495631	0,0235271		-0,0070406	0,00138	***
Roskilde*1998							0,0344527	0,0260415		-0,0003108	0,00631	
Vestsjælland*1998							0,0174384	0,0238707		0,0077061	0,01453	
Storstrøm*1998							-0,004726	0,0240392		-0,0033895	0,00413	
Fyn*1998							-0,0013849	0,0179738		8,04E-05	0,00601	
Sønderjylland*1998							-0,0286383	0,0246729		-0,0054559	0,00255	**
Ribe*1998							-0,0278775	0,0247135		-0,0058266	0,00227	***
Vejle*1998							-0,0357881	0,0211889	*	-0,0065029	0,00177	***
Ringkøbing*1998							-0,0376717	0,0231728		-0,0061946	0,00196	***
Århus*1998							0,0078085	0,0166542		0,00102	0,00569	
Viborg*1998							-0,0099733	0,0257692		-0,0027854	0,00567	
Nordjylland*1998							-0,015298	0,0187423		-0,00462	0,0028	*
Cph & Fred.*1998							-0,0184819	0,0155479		-0,0045699	0,0031	
Constant	0,0346392	0,0036832	***	0,0820905	0,0100286	***	0,0798105	0,0134352	***			
Obs	9006			9006			9006			9006		
Adj R2	0,001			0,0189			0,02			0,0832		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 15. Difference in differences for men from 1996-2002.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,002031	0,0055148		-0,0161921	0,0052179	***	-0,0149715	0,0052436	***	-0,0123349	0,00413	***
1996	ref.			ref.			ref.			ref.		
2002	-0,0145621	0,0052036	***	0,0065126	0,0055501		-0,0296625	0,0260353		-0,0255189	0,0203	
25*2002	0,0048236	0,0080089		0,0065267	0,0079646		0,0043918	0,00806		0,0053854	0,00708	
Dane				ref.			ref.			ref.		
Immigrant				0,0011719	0,0086954		-0,0058848	0,0132169		-0,0065734	0,00912	
Single				ref.			ref.			ref.		
Couple				-0,0011388	0,0051614		0,0062644	0,0067078		0,005862	0,00571	
Couple with children				-0,003963	0,0103692		-0,0068957	0,0133513		-0,0064802	0,0091	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0000392	0,0097373		-0,0077221	0,0104868		-0,0026747	0,00657	
Out of labor force				-0,0439967	0,0068433	***	-0,0437695	0,0090117	***	-0,0240327	0,00295	***
Student				-0,0273208	0,0061431	***	-0,0363918	0,0085401	***	-0,0196862	0,00349	***
Prev. Income				-2,68E-07	3,95E-08	***	-3,23E-07	5,85E-08	***	-2,10E-06	0	***
Work experience				0,0045661	0,001453	***	0,0030367	0,0018903		0,002985	0,00144	**
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				0,0097038	0,0116963		0,0046492	0,0151896		0,0052311	0,0161	
Roskilde				0,0390919	0,0130734	***	0,0332117	0,0165331	**	0,0391272	0,02943	
Vestsjælland				0,0020407	0,0118248		0,0133357	0,0150014		0,0160491	0,02069	
Storstrøm				0,0270644	0,0123233	**	0,0231024	0,0153437		0,0256627	0,02319	
Fyn				0,0183198	0,0090488	**	0,019921	0,0117717	*	0,0235167	0,01738	
Sønderjylland				0,0590319	0,0130402	***	0,035562	0,016717	**	0,0444529	0,03184	
Ribe				0,027417	0,0128248	**	0,0402032	0,0163203	**	0,0499673	0,03263	
Vejle				0,0311602	0,0108818	***	0,0282679	0,0140478	**	0,0421665	0,02701	
Ringkøbing				0,0110875	0,0118337		0,0092371	0,0156446		0,0131144	0,02059	
Århus				0,0106615	0,0079225		0,0108735	0,0106569		0,013387	0,01278	
Viborg				0,016957	0,0133208		0,0212565	0,0171032		0,026476	0,0266	
Nordjylland				0,0003834	0,0090169		0,0063266	0,0118939		0,0076721	0,01332	
Cph & Frederiksberg				0,0016732	0,0073692		0,0004115	0,0098985		-0,0000654	0,00891	

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Immigrant*2002							0,0124855	0,0175876		0,0125503	0,01536	
Couple*2002							-0,0163734	0,0105129		-0,0096441	0,0047	**
Couple&child*2002							0,0080174	0,0212489		0,012085	0,02697	
Unemployed*2002							0,0488422	0,0306335		0,0266466	0,03412	
Out of labor force*2002							-0,0017229	0,0138661		-0,0063753	0,01399	
Student*2002							0,0186064	0,0123102		0,0192228	0,01514	
Prev. Income*2002							8,69E-08	7,99E-08		9,04E-07	0	
Work experience*2002							0,0050223	0,0030397	*	0,0033823	0,00225	
Frederiksborg*2002							0,0120071	0,0238189		0,0101457	0,02672	
Roskilde*2002							0,0187132	0,0271003		0,006458	0,02245	
Vestsjælland*2002							-0,0305911	0,0244616				
Storstrøm*2002							0,012856	0,0259346		0,0099153	0,02517	
Fyn*2002							-0,0037635	0,0184258		-0,0040702	0,01151	
Sønderjylland*2002							0,0631111	0,0267594	**	0,0222954	0,03233	
Ribe*2002							-0,0321893	0,0264677		-0,0137273	0,00606	
Vejle*2002							0,0077589	0,0222622		-0,003264	0,0132	
Ringkøbing*2002							0,0045689	0,0239094		-0,0005222	0,01824	
Århus*2002							-0,0011835	0,0159449		-0,0024637	0,01157	
Viborg*2002							-0,0081845	0,0273081		-0,0056149	0,01498	
Nordjylland*2002							-0,0145372	0,0182362		-0,0105398	0,00831	
Cph & Frederiksberg*2002							0,002438	0,0148332		0,0043356	0,01459	
Constant	0,0327628	0,0036225	***	0,0558864	0,0087519	***	0,0670165	0,0114055	***			
Obs	6877			6877			6877			6784		
Adj R2	0,0013			0,0156			0,0168			0,0944		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 16. Difference in differences for women from 1996-2002.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,0022089	0,0054141		-0,0101358	0,0051238	**	-0,0095025	0,0051488	*	-0,0024195	0,00141	*
1996	ref.			ref.			ref.			ref.		
2002	-0,0100908	0,0051193	**	0,0039545	0,0054583		-0,0490407	0,0274273	*	-0,0137315	0,00918	
25*2002	0,0005817	0,0078334		-0,0003588	0,0077794		-0,0021465	0,007878		-0,0007178	0,00192	
Dane				ref.			ref.			ref.		
Immigrant				0,0078135	0,0082773		-0,0050312	0,0113883		-0,0028438	0,003	
Single				ref.			ref.			ref.		
Couple				0,0012832	0,0043984		0,001874	0,005814		0,0001298	0,00148	
Couple with children				0,004869	0,0069826		-0,0001936	0,0089045		-0,0008909	0,00259	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0324463	0,0098727	***	-0,0321726	0,0103679	***	-0,0049644	0,00129	***
Out of labor force				-0,0526453	0,0063324	***	-0,0549118	0,008054	***	-0,0560031	0,00372	***
Student				-0,0285598	0,0057353	***	-0,0387061	0,0079956	***	-0,0067063	0,00167	***
Prev. Income				-3,19E-07	5,46E-08	***	-3,84E-07	7,59E-08	***	-1,01E-06	0	***
Work experience				0,0048782	0,0017028	***	0,0040834	0,0021788	*	0,0011978	0,00059	**
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				0,034881	0,0119406	***	0,0314613	0,0153002	**	0,0129328	0,01017	
Roskilde				0,0500606	0,0127602	***	0,0429169	0,0161366	***	0,0203216	0,01372	
Vestsjælland				0,0045376	0,0123079		-0,0019805	0,0163115		-0,0012199	0,00447	
Storstrøm				0,0234166	0,0122767	*	0,0282769	0,0155622	*	0,0127246	0,01058	
Fyn				0,0007115	0,0091096		-0,0058802	0,0118838		-0,0014715	0,0031	
Sønderjylland				0,0154316	0,0128158		0,0291645	0,0160228	*	0,0114939	0,01005	
Ribe				0,0334755	0,0125233	***	0,0259373	0,0161801		0,0116903	0,01035	
Vejle				0,0221439	0,0107657	**	0,0167754	0,0136448		0,0064073	0,00674	
Ringkøbing				0,0139852	0,0115525		0,0216558	0,0149852		0,0077437	0,00774	
Århus				0,0022176	0,008224		0,002523	0,0107713		0,0011089	0,00352	
Viborg				0,0103851	0,0128898		-0,0001925	0,0170184		0,000196	0,00511	
Nordjylland				0,0113703	0,0093976		0,0213083	0,0123758	*	0,0086529	0,00691	
Cph & Frederiksberg				-0,0005953	0,007716		-0,0004264	0,0102213		0,0003187	0,00311	

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Immigrant*2002							0,0258679	0,0166213		0,0087541	0,00558	
Couple*2002							-0,0006971	0,0089136		0,0003532	0,00229	
Couple&child*2002							0,0151666	0,0144299		0,0087602	0,00856	
Unemployed*2002							-0,0171087	0,0422147				
Out of labor force*2002							0,0055721	0,0131079		0,9214622	0,00916	***
Student*2002							0,0208199	0,0114977	*	0,0058121	0,00471	
Prev. Income*2002							1,25E-07	1,10E-07		3,04E-07	0	
Work experience*2002							0,0019318	0,0035304		0,0004231	0,0009	
Frederiksborg*2002							0,0080686	0,024512		0,0007661	0,00641	
Roskilde*2002							0,0204864	0,0264461		0,0001797	0,00594	
Vestsjælland*2002							0,0154037	0,0248895		0,006337	0,01364	
Storstrøm*2002							-0,0127478	0,0253781		-0,0023032	0,00427	
Fyn*2002							0,0173459	0,0185306		0,0064949	0,00979	
Sønderjylland*2002							-0,0377762	0,0268022		-0,0053173	0,00155	***
Ribe*2002							0,0196423	0,0255658		0,0025138	0,00813	
Vejle*2002							0,0160005	0,0222775		0,0026651	0,00759	
Ringkøbing*2002							-0,017156	0,0235787		-0,0035935	0,00297	
Århus*2002							0,0008478	0,0167042		-0,0000461	0,00464	
Viborg*2002							0,0252543	0,0260784		0,0083942	0,01458	
Nordjylland*2002							-0,0210029	0,019044		-0,0038962	0,00236	*
Cph & Frederiksberg*2002							0,0015721	0,0156507		0,0004146	0,00463	
Constant	0,0346392	0,0035133	***	0,0677051	0,0099073	***	0,0798105	0,0128379	***			
Obs	7994			7994			7994			7976		
Adj R2	0,0005			0,016			0,0161			0,0916		

Source: Statistics Denmark register panel data from 1995 to 2004 &Dream register on AAS 1997-2005

Table 17. Difference in differences for men from 1996-2003.

	Dif in dif (ols)			Dif in dif with cov (ols)			Dif in dif with cov (probit)		
	Coef	Stand. Err		Coef	Stand. Err		Coef	Stand. Err	
24 years	ref.			ref.			ref.		
25 years	-0,0145621	0,0055467	***	-0,0153408	0,0055332	***	-0,0160366	0,00536	***
1996	ref.			ref.			ref.		
1997	0,0037298	0,0055103		0,0039384	0,0054892		0,0038297	0,00501	
1998	0,0005879	0,0055496		0,0022568	0,005533		0,0025596	0,00499	
1999	0,0032497	0,0055519		0,0053715	0,005538		0,0046132	0,00517	
2000	0,0010761	0,0057183		0,0037778	0,0057098		0,0045853	0,00536	
2001	0,0074216	0,0059157		0,0100707	0,0059049	*	0,0094071	0,00599	
2002	0,002031	0,0058784		0,0060039	0,0058714		0,0055556	0,00567	
2003	0,0052731	0,0059997		-0,0254107	0,0064917	***	-0,0190016	0,00271	***
25*1997	0,0005414	0,0079078		0,0015035	0,0078733		0,0032196	0,00798	
25*1998	0,0251003	0,0079745	***	0,0254089	0,0079365	***	0,0334358	0,01314	**
25*1999	0,0096055	0,0079935		0,0111425	0,0079586		0,0162124	0,01037	
25*2000	0,0011661	0,008116		0,0023706	0,0080817		0,0023376	0,0081	
25*2001	0,0088164	0,0083843		0,010637	0,0083477		0,0149137	0,01042	
25*2002	0,0048236	0,008537		0,0060155	0,0084985		0,0088604	0,00982	
25*2003	0,0001537	0,0085464		-0,000387	0,0085084		0,0019137	0,00816	
Dane				ref.			ref.		
Immigrant				-0,0000299	0,0042546		-0,000205	0,0039	
Single				ref.			ref.		
Couple				-0,0032788	0,0027416		-0,002818	0,00223	
Couple with children				-0,011663	0,0056258	**	-0,0102077	0,00386	***
Wage earner				ref.			ref.		
Unemployed				-0,0018899	0,0058638		-0,000422	0,00473	
Out of labor force				-0,0416576	0,0036573	***	-0,0290865	0,00141	***
Student				-0,0201521	0,0032362	***	-0,0148072	0,00194	***
Prev. Income				-2,46E-07	2,08E-08	***	-2,05E-06	0	***
Work experience				0,00457	0,0007748	***	0,004512	0,00066	***
Great Copenhagen				ref.			ref.		
Frederiksborg				0,0156164	0,0063463	**	0,01571	0,00763	**
Roskilde				0,0224486	0,0070881	***	0,0227628	0,00933	**
Vestsjælland				0,0181875	0,0065366	***	0,0199447	0,00852	**
Storstrøm				0,0189506	0,0065952	***	0,0186613	0,00831	**
Fyn				0,0077603	0,004866		0,0074801	0,00512	
Sønderjylland				0,0230695	0,0070268	***	0,0239736	0,00942	**
Ribe				0,0099943	0,0069097		0,0095001	0,00755	
Vejle				0,0154719	0,0057872	***	0,0150868	0,00696	**
Ringkøbing				0,0098816	0,0065072		0,0098517	0,00719	
Århus				0,0025645	0,0042428		0,0030146	0,00407	
Viborg				0,0160391	0,0072571	**	0,017115	0,00895	*
Nordjylland				0,0050377	0,0048255		0,0050217	0,00485	
Cph & Frederiksberg				-0,0052789	0,0039124		-0,0046445	0,00336	
Constant	0,0327628	0,0038613	***	0,0578278	0,0057464	***			
Obs	27571			27571			27571		
Adj R2	0,0012			0,011			0,0503		

Source: Statistics Denmark register panel data from 1995 to 2004 &Dream register on AAS 1997-2005

Table 18. Difference in differences for women from 1996-2003.

	Dif in dif (ols)			Dif in dif with cov (ols)			Dif in dif with cov (probit)		
	Coef	Stand. Err		Coef	Stand. Err		Coef	Stand. Err	
24 years	ref.			ref.			ref.		
25 years	-0,0100908	0,0052866		-0,0108064	0,005266	**	-0,009377	0,00454	**
1996	ref.			ref.			ref.		
1997	0,0054125	0,0051875		0,0057362	0,0051612		0,0051781	0,00458	
1998	0,0084981	0,0052031		0,008343	0,0051815		0,0072338	0,00474	
1999	0,0032816	0,0051734		0,0040009	0,0051537		0,0039979	0,00451	
2000	-0,0009758	0,005382		0,0002162	0,0053648		0,00165	0,00451	
2001	0,0008966	0,0055576		0,0010743	0,0055409		0,0023174	0,00469	
2002	0,0022089	0,005591		0,0036737	0,0055793		0,0049066	0,005	
2003	0,005931	0,0055375		-0,0217798	0,0061235	***	-0,0184783	0,00251	***
25*1997	-0,0063108	0,0074793		-0,006082	0,0074375		-0,0046519	0,00546	
25*1998	0,0003339	0,0075409		0,0010903	0,007497		0,0023758	0,00659	
25*1999	0,0010937	0,0075289		0,000623	0,0074863		0,0012727	0,00654	
25*2000	0,0078362	0,0076482		0,0080319	0,0076048		0,0081242	0,00787	
25*2001	-0,0005246	0,0079119		0,0010121	0,0078659		0,0016097	0,00699	
25*2002	0,0005817	0,0080893		-0,0000237	0,0080421		-0,0001977	0,00672	
25*2003	-0,00089	0,0080452		-0,0002078	0,0080002		0,0008363	0,00676	
Dane				ref.			ref.		
Immigrant				-0,0026201	0,0040205		-0,0036179	0,00346	
Single				ref.			ref.		
Couple				-0,0022928	0,0022454		-0,0018118	0,00175	
Couple with children				0,0126674	0,0035806	***	0,0132074	0,00398	***
Wage earner				ref.			ref.		
Unemployed				-0,0237761	0,0056314	***	-0,0130689	0,00275	***
Out of labor force				-0,0496615	0,0032446	***	-0,0359184	0,0013	***
Student				-0,0227747	0,002834	***	-0,0167712	0,00172	***
Prev. Income				-2,17E-07	2,45E-08	***	-2,05E-06	0	***
Work experience				0,0036322	0,0008491	***	0,0038704	0,00069	***
Great Copenhagen				ref.			ref.		
Frederiksborg				0,0058866	0,0061775		0,0060741	0,00582	
Roskilde				0,0294097	0,0070073	***	0,0253741	0,00884	***
Vestsjælland				0,0110574	0,0062781	*	0,0113704	0,00664	*
Storstrøm				0,0202966	0,0063301	***	0,0195269	0,00759	***
Fyn				-0,0020987	0,0047398		-0,001183	0,00385	
Sønderjylland				0,0126841	0,0065998	*	0,0105973	0,00676	
Ribe				0,0127009	0,0064593	**	0,0103929	0,00656	
Vejle				0,0115736	0,0055407	**	0,0106356	0,0057	*
Ringkøbing				0,0063895	0,0060104		0,0044841	0,00542	
Århus				0,0008445	0,0042955		0,000887	0,00363	
Viborg				0,0011929	0,0067711		0,0013239	0,00575	
Nordjylland				0,0014982	0,0048562		0,0015131	0,00416	
Cph & Frederiksberg				-0,0118183	0,003999	***	-0,0092961	0,00293	***
Constant	0,0346392	0,0036281	***	0,0653414	0,0058782	***			
Obs	32787			32787			32787		
Adj R2	0,0006			0,0127			0,0588		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 19. Difference in differences for between 23-26 years of age from 1996-1998.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,0037347	0,0039752		-0,0199999	0,004167	***	-0,0177095	0,0042733	***	-0,0186235	0,004	***
1996	ref.			ref.			ref.			ref.		
1998	-0,0195749	0,004063	***	0,0049879	0,0039672		-0,0128528	0,0192951		-0,0317006	0,01952	
25*1998	0,0177983	0,0058093	***	0,0186932	0,0057777	***	0,0142074	0,006088	***	0,0192316	0,00711	***
Dane				ref.			ref.			ref.		
Immigrant				-0,0067836	0,0064744		-0,011735	0,0096274		-0,0145997	0,0064	**
Single				ref.			ref.			ref.		
Couple				0,0069235	0,0038296	*	-0,0022253	0,005394		-0,0014708	0,00479	
Couple & children				-0,003187	0,0070372		-0,006384	0,009632		-0,0073137	0,0084	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0128663	0,0068634	*	-0,0112771	0,0081712		-0,0058675	0,00596	
Out of lab.force				-0,0448089	0,0049104	***	-0,0443552	0,0070171	***	-0,031462	0,00272	***
Student				-0,0255396	0,0046978	***	-0,0292842	0,0067188	***	-0,0199368	0,00329	***
Prev. Income				-3,33E-07	3,16E-08	***	-3,20E-07	4,48E-08	***	-3,06E-06	0	***
Work experience				0,0050309	0,0010411	***	0,0033777	0,0014162	**	0,0043003	0,0013	***
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				0,0213701	0,0083523	**	0,0189184	0,0115027	*	0,0218983	0,01525	
Roskilde				0,0136411	0,0096368		0,0168925	0,0132454		0,0171842	0,01688	
Vestsjælland				0,0214953	0,0085793	**	0,0237037	0,0117118	**	0,0262609	0,01704	
Storstrøm				0,0276385	0,0085549	***	0,0156136	0,0118774		0,0150032	0,01455	
Fyn				0,0059094	0,0066823		0,005132	0,0091829		0,0054191	0,00949	
Sønderjylland				0,0220633	0,0090977	**	0,0274488	0,012505	**	0,0335005	0,01952	*
Ribe				0,0067787	0,0091034		0,0091697	0,0126285		0,0097586	0,01458	
Vejle				0,0179972	0,0078518	**	0,0175932	0,0109177		0,0243576	0,0156	
Ringkøbing				0,0070171	0,0087827		0,0050524	0,0119091		0,0057884	0,01301	
Århus				0,002905	0,0058975		-0,0002755	0,0082179		0,0005977	0,00773	
Viborg				-0,0044067	0,0096574		0,0039576	0,0132132		0,0047215	0,01403	
Nordjylland				0,0029108	0,0066302		-0,0035245	0,0091934		-0,0022262	0,00825	
Cph & Frederiksb.				-0,0099446	0,0054911	*	-0,0051451	0,0076917		-0,0053468	0,00664	

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Immigrant*1998							0,009629	0,0130206		0,0172027	0,01389	
Couple*1998							0,0182287	0,0076644	**	0,014316	0,0087	*
Couple&child*1998							0,0060299	0,014121		0,0105454	0,01804	
Unemployed*1998							-0,0067933	0,0152805		-0,0056067	0,01061	
Out of lab.for.*1998							-0,0014848	0,0098358		0,0250945	0,01921	
Student*1998							0,0070493	0,0094025		0,0138991	0,01004	
Prev. Income*1998							-3,17E-08	6,33E-08		1,00E-06	0	*
Work exp.*1998							0,0038068	0,0020947	*	0,0009196	0,00176	
Frederiksborg*1998							0,0055285	0,0167362		-0,0018296	0,01205	
Roskilde*1998							-0,006561	0,0193085		-0,0061778	0,01226	
Vestsjælland*1998							-0,0044134	0,0172135		-0,0056126	0,01098	
Storstrøm*1998							0,0252078	0,0171247		0,0109536	0,01766	
Fyn*1998							0,0019205	0,0133894		-0,0007197	0,01095	
Sønderjylland*1998							-0,0115496	0,018234		-0,0108062	0,00892	
Ribe*1998							-0,005043	0,0182269		-0,0051033	0,01263	
Vejle*1998							0,0010382	0,0157169		-0,0065449	0,00986	
Ringkøbing*1998							0,0042416	0,0176427		0,0000603	0,0148	
Århus*1998							0,0069193	0,0118097		0,0049424	0,01147	
Viborg*1998							-0,0175251	0,0193639		-0,0135141	0,00964	
Nordjylland*1998							0,0131427	0,0132731		0,0102706	0,01489	
Cph & Fred.*1998							-0,0086894	0,0109949		-0,0058853	0,0084	
Constant	0,0365472	0,0027784	***	0,0729766	0,006547	***	0,076949	0,0087303	***			
Obs	15290			15290			15290			15290		
Adj R2	0,0025			0,0149			0,015			0,0662		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 20. Difference in differences for women between 23-26 years of age from 1996-1998.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
24 years	ref.			ref.			ref.			ref.		
25 years	0,0088415	0,0039153	**	-0,0187154	0,0041538	***	-0,0177061	0,0042373	***	-0,0164157	0,0037	***
1996	ref.			ref.			ref.			ref.		
1998	-0,0185785	0,0040932	***	0,0088444	0,0038955	**	0,0203999	0,0201664		-0,0052455	0,01883	
25*1998	0,0035899	0,0057786		0,0049051	0,0057342		0,0027985	0,005968		0,0058907	0,00549	
Dane				ref.			ref.			ref.		
Immigrant				-0,0078134	0,0062066		-0,0082557	0,0091601		-0,0140766	0,00719	**
Single				ref.			ref.			ref.		
Couple				-0,0011627	0,0033314		-0,0034415	0,0046967		-0,0031712	0,00376	
Couple & children				0,0129888	0,0049393	***	0,0041375	0,006978		0,004893	0,00735	
Wage earner				ref.			ref.			ref.		
Unemployed				-0,0282747	0,0070826	***	-0,0280525	0,008294	***	-0,0166949	0,0041	***
Out of lab.force				-0,0620341	0,0044685	***	-0,0587497	0,0062822	***	-0,0442002	0,00307	***
Student				-0,042882	0,0044419	***	-0,0460016	0,0064032	***	-0,0267618	0,00318	***
Prev. Income				-4,15E-07	4,17E-08	***	-4,56E-07	6,09E-08	***	-4,02E-06	0	***
Work experience				0,0044022	0,0012146	***	0,0046643	0,0016674	***	0,0050402	0,00142	***
Great Copenhagen				ref.			ref.			ref.		
Frederiksborg				-0,0087936	0,008687		0,0097535	0,0121597		0,0096463	0,01288	
Roskilde				0,02855	0,009619	***	0,0299537	0,0131612	**	0,03178	0,01848	*
Vestsjælland				0,0030888	0,0088185		0,0086092	0,0123834		0,0103356	0,01391	
Storstrøm				0,0244899	0,0089553	***	0,024199	0,0126488	*	0,025729	0,0168	
Fyn				-0,0123678	0,0068255	*	-0,0051431	0,0095867		-0,0037071	0,00813	
Sønderjylland				0,0067855	0,0092352		0,0270859	0,0129618	**	0,0283461	0,01754	
Ribe				0,000434	0,0091465		0,0160265	0,0127871		0,018724	0,01546	
Vejle				-0,0006662	0,0078832		0,0102441	0,0110004		0,0113674	0,01208	
Ringkøbing				0,0028132	0,0085947		0,0225889	0,0119479	*	0,0222691	0,01492	
Århus				-0,0096997	0,0062482		-0,0029837	0,0087202		-0,0032988	0,0074	
Viborg				0,0068492	0,0095002		0,0227058	0,0131184	*	0,0238041	0,01687	
Nordjylland				-0,0046308	0,007066		0,0030196	0,0098968		0,0038824	0,00956	
Cph & Frederiksb.				-0,0217872	0,005853	***	-0,0047202	0,0082386		-0,0038462	0,00703	

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continued

Immigrant*1998							0,0009841	0,0124595		0,0106114	0,01394	
Couple*1998							0,0045854	0,0066648		0,0045118	0,00581	
Couple&child*1998							0,0177152	0,0098827	*	0,0135314	0,01135	
Unemployed*1998							0,0069528	0,0163209		0,0120204	0,01732	
Out of lab.for.*1998							-0,0066286	0,0089432		0,0249224	0,01738	
Student*1998							0,0056774	0,0088956		0,0101496	0,00843	
Prev. Income*1998							7,34E-08	8,38E-08		1,04E-06	0	*
Work exp.*1998							-0,0004894	0,0024389		-0,001673	0,00195	
Frederiksborg*1998							-0,0385191	0,0173783	**	-0,0187171	0,00588	***
Roskilde*1998							-0,0024624	0,0192877		-0,0084956	0,01005	
Vestsjælland*1998							-0,0118391	0,0176434		-0,0084481	0,0104	
Storstrøm*1998							-0,0013691	0,0179215		-0,0070407	0,01029	
Fyn*1998							-0,0156716	0,0136587		-0,0079984	0,00889	
Sønderjylland*1998							-0,0418797	0,0184753	**	-0,0190758	0,00555	***
Ribe*1998							-0,0328076	0,0183	*	-0,0178805	0,00616	***
Vejle*1998							-0,0233341	0,0157773		-0,0137937	0,00723	*
Ringkøbing*1998							-0,0414115	0,0172031	**	-0,0188201	0,00537	***
Århus*1998							-0,0144308	0,0125021		-0,0062309	0,00855	
Viborg*1998							-0,0338171	0,0190246	*	-0,01743	0,00644	***
Nordjylland*1998							-0,0165783	0,0141383		-0,0101556	0,008	
Cph & Fred.*1998							-0,03433	0,0117143	***	-0,0190117	0,00554	***
Constant	0,0420237	0,0027535	***	0,1063789	0,0074648	***	0,1011044	0,0102316	***			
Obs	17932			17932			17932			17932		
Adj R2	0,0024			0,0203			0,0206			0,0763		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 21. Difference in differences for unskilled men from 1996-1998.

	D in d (ols)			D in d cov (ols)			D in d cov & time cov (ols)			D in d cov & t. cov (probit)		
	Coef	St.err.		Coef	St.err.		Coef	St.err.		Coef.	St.err.	
25*1998	0,0368532	0,011492	***	0,0394337	0,0113076	***	0,0386336	0,0116229	***	0,0352807	0,01359	***
Adj R2	0,0037			0,0378			0,0373			0,1292		

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 22. Difference in differences for unskilled women from 1996-1998.

	D in d (ols)		D in d cov (ols)		D in d cov & time cov (ols)		D in d cov & t. cov (probit)	
	Coef	St.err.	Coef	St.err.	Coef	St.err.	Coef.	St.err.
25*1998	0,0018973	0,0117872	0,0015598	0,0114965	0,0016218	0,0117359	-0,0000238	0,00127
Adj R2	0,0013		0,0531		0,0538		0,1802	

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

Table 23. Averages applied for calculating elasticities.

	Yearly ave. wage DkK	Ave. edu. Income (3,5 y)	Entrance rate	95% conf.inte
New voc.train. 25y 1996	121269	424442		
New adult voc. Train. 25 y 1998	159886	559601		
Wage difference Dkr	38617	135159		
Wage difference in pct	31,84	31,84		
Voc. Train. 25y 1996			1,82	
Estimated increase 25y 1998			2,69	0,71-4,67
Entrance difference in pct			147,8	39,01-256,59
Exchange rate 1000US$ (2007)	5664			

Source: Statistics Denmark register panel data from 1995 to 2004 & Dream register on AAS 1997-2005

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