



norden

NORDIC ECONOMIC POLICY REVIEW

CONSEQUENCES OF YOUTH UNEMPLOYMENT AND EFFECTIVENESS OF POLICY INTER-VENTIONS

Michael Rosholm and Michael Svarer

Scarring effects of early-career unemployment

Øivind A. Nilsen and Katrine Holm Reiso

Comment by Björn Tyrefors Hinnerich

Bad times at a tender age – How education dampens the impact of gradu-ating in a recession

Kai Liu, Kjell G. Salvanes and Erik Ø. Sørensen

Comment by Matz Dahlberg

Networks and youth labor market entry

Lena Hensvik and Oskar Nordström Skans

Comment by Daniel le Maire

Effects of payroll tax cuts for young workers

Per Skedinger

Comment by Peter Skogman Thoursie

Sanctions for young welfare recipients

Gerard J. van den Berg, Arne Uhlendorff

and Joachim Wolff

Comment by Johan Vikström

Can active labour market policies combat youth unemployment?

Jonas Maibom, Michael Rosholm and Michael Svarer

Comment by Caroline Hall



The *Nordic Economic Policy Review* is published by the Nordic Council of Ministers and addresses policy issues in a way that is useful for informed non-specialists as well as for professional economists. All articles are commissioned from leading professional economists and are subject to peer review prior to publication.

The review appears twice a year. It is published electronically on the website of the Nordic Council of Ministers: www.norden.org/en. On that website, you can also order paper copies of the Review (enter the name of the Review in the search field, and you will find all the information you need).

Managing Editor:

Professor Torben M. Andersen, Department of Economics, University of Aarhus, Denmark.

Special Editors for this volume:

Professor Michael Rosholm, Department of Economics and Business, Aarhus University and Professor Michael Svarer, Department of Economics and Business, Aarhus University.

Papers published in this volume were presented at the conference "Youth and the Labour Market" hosted by the Ministry of Finance, Sweden May 2013.

Nordic Economic Policy Review, 2014 no 1

Youth Unemployment

Udenfor se 2014:416

Nordic Economic Policy Review, 2014 no 1
Youth Unemployment

ISBN 978-92-893-2760-2
<http://dx.doi.org/10.6027/US2014-416>

US 2014:416
ISSN 1904-4526

© Nordic Council of Ministers 2014

Layout: NMR
Cover photo: Jette Koefoed/NMR

Print: Rosendahls-Schultz Grafisk
Copies: 216

Printed in Denmark



This publication has been published with financial support by the Nordic Council of Ministers. However, the contents of this publication do not necessarily reflect the views, policies or recommendations of the Nordic Council of Ministers.

www.norden.org/en/publications

Nordic co-operation

Nordic co-operation is one of the world's most extensive forms of regional collaboration, involving Denmark, Finland, Iceland, Norway, Sweden, and the Faroe Islands, Greenland, and Åland.

Nordic co-operation has firm traditions in politics, the economy, and culture. It plays an important role in European and international collaboration, and aims at creating a strong Nordic community in a strong Europe.

Nordic co-operation seeks to safeguard Nordic and regional interests and principles in the global community. Common Nordic values help the region solidify its position as one of the world's most innovative and competitive.

Nordic Council of Ministers

Ved Stranden 18
DK-1061 Copenhagen K
Phone (+45) 3396 0200

www.norden.org

Content

Consequences of youth unemployment and effectiveness of policy interventions	
<i>Michael Rosholm and Michael Svarer</i>	7
Scarring effects of early-career unemployment	
<i>Øivind A. Nilsen and Katrine Holm Reiso</i>	13
Comment by <i>Björn Tyrefors Hinnerich</i>	47
Bad times at a tender age – How education dampens the impact of graduating in a recession	
<i>Kai Liu, Kjell G. Salvanes and Erik Ø. Sørensen</i>	51
Comment by <i>Matz Dahlberg</i>	75
Networks and youth labor market entry	
<i>Lena Hensvik and Oskar Nordström Skans</i>	81
Comment by <i>Daniel le Maire</i>	119
Effects of payroll tax cuts for young workers	
<i>Per Skedinger</i>	125
Comment by <i>Peter Skogman Thoursie</i>	171
Sanctions for young welfare recipients	
<i>Gerard J. van den Berg, Arne Uhlendorff</i> <i>and Joachim Wolff</i>	177
Comment by <i>Johan Vikström</i>	209

Can active labour market policies combat youth unemployment?	
<i>Jonas Maibom, Michael Rosholm and Michael Svarer</i>	215
Comment by <i>Caroline Hall</i>	263

Consequences of youth unemployment and effectiveness of policy interventions

Michael Rosholm* and Michael Svarer**

Youth unemployment has increased disproportionately in many European countries as a consequence of the financial and economic crises. Although previous crises have also affected youth more than older workers, the concern this time is that, in the light of the depth and duration of the crisis, youth may to a larger extent than previously end up being unemployed for a long period which, in turn, may have adverse effects on their future employment prospects. At the early stages of the working career, a long period of unemployment may lead to the loss of cognitive human capital skills as well as non-cognitive skills such as work motivation, discipline, self-control etc. Therefore, the activation and reintegration of young workers is an important policy goal in many European countries. In this issue of the *Nordic Economic Policy Review*, we focus on the consequences of youth unemployment and on evaluating the effectiveness of different labour market policies targeted towards youth.

In the first paper in this volume, Nilsen and Reiso investigate whether experiencing a period of unemployment at an early age has long-term consequences on future labour market outcomes. Experiencing unemployment implies that the individual loses income and, potentially, also that human capital starts to deteriorate. A period of unemployment may therefore have long-term effects if the subsequent labour market attach-

* Department of Economics and Business, Aarhus University, rom@asb.dk.

** Department of Economics and Business, Aarhus University, msvarer@econ.au.dk.

ment is negatively affected. Identifying and assessing such long-term effects is clearly crucial if we want a complete picture of the costs of unemployment. The paper uses Norwegian register data and focuses on young people who have had some work experience before losing their job and entering unemployment. Those becoming unemployed are compared to a group of similar individuals who do not become unemployed. The analysis focuses on three labour market outcomes; being unemployed, leaving the labour force and returning to education. The study finds long-term effects on the probability of being unemployed. The probability of being unemployed in the subsequent year (after becoming unemployed) is 30 per cent, which to some extent reflects long-term unemployment. In the following years, the unemployment probability declines, but still after five years, they find a 5 per cent higher unemployment probability for those with an early period of unemployment. The study also finds that an unemployment event raises the probability of leaving the labour force and returning to education, although these effects are relatively small.

In a related study, Liu, Salvanes and Sørensen investigate the effects of graduating in recessions. The analysis is based on data on Norwegian youth who entered the labour market between 1986 and 2002. The analysis distinguishes between youth with different levels of education and focuses on two dimensions; 1) how are these labour market entry cohorts affected by business cycles, and 2) are the effects of entering during recessions persistent? The main finding is that in terms of different labour market outcomes (e.g. earnings, employment, tenure length), those with college education and more are basically insulated from the effects of recessions. For the less educated groups, there is a negative effect on labour market outcomes from entering the labour market in a recession, and youth with a vocational high-school degree are less affected than other educational groups. The detrimental effects of entering the labour market in bad times wear out over a period of 3-5 years. The scars of entering the labour market in a recession are therefore not permanent. The study highlights that it is not only important to have an educated work force, but also that young people with a vocational high-school degree are less sensitive to business cycle fluctuations than those with an academic high-school degree.

The two studies introduced above based on Norwegian data point to the importance of supporting employment prospects for young workers and

especially those with lower levels of education. Hensvik and Norström Skans investigate the importance of social contacts for labour market outcomes of youth. When analysing the role of networks in the labour market, it is found that informal recruitment channels in general, and social networks in particular, are quantitatively important for the matching of job seekers and firms. Swedish evidence suggests that about one third of the realized matches appear through *i)* formal channels, *ii)* direct applications and *iii)* social networks, respectively. The informal channels also tend to be relatively more important among the young and the less educated. The study is based on Swedish register data for the cohort of graduates from vocational high school in the summer of 2006. The sample consists of 39 000 19-year old individuals. The results suggest that access to social networks is indeed important, both in determining which particular establishments students sort into after high school and with respect to the time it takes to find a stable job. The magnitudes of these effects are non-trivial: graduates who had a summer/extra job at a particular establishment have a 35 percentage-point higher probability of finding a stable job there as compared to other students from the same class; and they have a 4 percentage point higher probability of ending up in an establishment to which someone from the summer/extra job has moved. In addition, the employment rate of graduates is estimated to increase by at least 14 percentage points if all high school job contacts were employed relative to a case where none of the contacts were employed. A consistent result is that the network effect appears to be substantially larger if the contacts are specialized in the same field as that of the graduating student. An obvious policy lesson from these results is that it is useful to integrate meetings with potential future employers in the design of social programmes targeted towards young workers about to enter the labour market.

Skedinger investigates a Swedish payroll tax reform targeted at young workers. The analysis considers the effects on worker outcomes as well as firm performance in the retail industry. By comparing young workers (affected by the payroll tax cuts) to slightly older workers as a control group, he finds that the effects on entry, exit, hours and wages have been small, both in absolute magnitudes and in relation to the sizeable cuts in taxes. The findings are in accordance with much of the previous literature on the employment effects of changes in payroll taxes. The findings are also similar to those obtained by Egebark and Kaunitz (2013) who exam-

ine the effects of the same payroll tax reforms as Skedinger, but for the entire labour market. Egebark and Kaunitz (2013) perform a cost-benefit analysis and estimate that each new job in the age group 21-24 is associated with a cost of SEK 1.0-1.5 million (USD 150 000-230 000). The conclusion is that reducing payroll taxes is a costly means of improving employment prospects for the young. The paper also examines the effect on firm performance of payroll tax cuts. The analysis is based on comparing firms with marginally larger pre-reform shares of young workers with performance after the reform. There is some evidence of increasing profit margins following the reform.

Van den Berg, Uhlendorff and Wolff investigate how sanctions affect the transition into employment for young welfare recipients. This paper gives an overview of the literature on sanctions in social welfare systems and analyses the impact of strong (complete withdrawal of benefits for three months) and mild (10 per cent reduction in benefits for three months) sanctions for young male welfare recipients in West Germany on the transition rate to unsubsidized employment. The results suggest that both mild and strong sanctions lead to a higher transition rate to work and that, although the effect is higher for strong sanctions, the marginal effect of the strong sanction is relatively small. Part of the sanction effect is due to the perceived risk of intensified monitoring after the punishment. This suggests that in the case of a first punishment during a welfare spell, it is not necessary to give the maximum possible sanction, in the sense that a less strong sanction also has a strong effect on the transition rate to work while having a smaller disutility cost for the individual.

The final paper by Maibom, Rosholm and Svarer investigates the effectiveness of active labour market policies for young unemployed Danes. The policy response to youth unemployment in Denmark has relied heavily on active measures such as frequent meetings with case workers and an intensive use of activation programmes. Empirical findings from the period prior to the financial crises suggest that both meetings and activation had a positive impact on the job finding rate of unemployed youth in Denmark. Partly based on these earlier findings, there has been an intensification of active labour market programmes in general and for youth in particular. The main empirical contribution of the paper is to evaluate a randomised field experiment that was conducted in Denmark in the winter of 2009. The main feature of the experiment was to

further intensify the classical tools of the ALMP toolbox and to shift the focus from classroom training to work practice and more firm-based job training. The main difference in terms of treatment between the treatment and the control group was in the number of meetings held with a case worker. They find that for uneducated youth, there was a negative effect on employment. This was in some sense the intention of the programme since those with no further education should be guided towards education if the option was feasible for the individual. The group of unemployed youth without a qualifying education did, in fact, accumulate slightly more education, but the magnitude was small. For the group of unemployed with some type of further education, there is some indication that the exit to employment was positively affected in the period in which the meetings took place, but the size of the effect is again small. In some sense, these findings are not surprising; the use of meetings is already quite intensive in Denmark towards youth and, at the time of the experiment, the labour market was characterised by low job finding rates and rapidly increasing unemployment. In addition, the treatment population consisted of individuals with quite long elapsed unemployment spells, and earlier evidence on the effectiveness of e.g. meetings shows much stronger effects for newly unemployed workers. The analysis found visible (positive) effects on the exit to sickness benefits.

Scarring effects of early-career unemployment*

Øivind A. Nilsen** and Katrine Holm Reiso***

Summary

The dramatically high levels of unemployment among younger workers, especially in southern Europe, emphasise an important question, how does unemployment early in a career affect future labour market opportunities? In this paper, young Norwegian residents are followed over a 15 years period. The findings show that early-career unemployment is generally associated with weaker labour market attachment. The risk of repeated unemployment decreases over time, whereas the risks of being out of the labour force and going back to school remain fairly constant. Finally, it is unlikely that the increased probability of unemployment is caused solely by selection on unobservable factors i.e. early-career unemployment leaves individuals with long-term unemployment scars.

Keywords: Unemployment persistency, scarring, matching techniques.

JEL classification numbers: J64, J65, C23.

* The authors thank Rolf Aaberge, Sascha O. Becker, Astrid Kunze, Kjell Salvanes, an anonymous referee and the editor of this journal, seminar participants at Statistics Norway, the Norwegian School of Economics, delegates at the 2011 Annual Meeting for Norwegian Economists, the 2011 Nordic Econometric Meeting, the ESEM 2011, the 2011 EALE Conference, the 7th Norwegian–German CESifo Seminar, the 2012 SOLE Conference, the 2012 ESPE Conference, the IZA 2012 workshop on “Youth Unemployment and Labour Market Integration”, and the 2013 Nordic Economic Policy Review’s conference on “Youth and the Labour Market” for helpful comments. The usual disclaimer applies.

** Department of Economics, Norwegian School of Economics, oivind.nilsen@nhh.no.

*** Department of Economics, Norwegian School of Economics, katrine.reiso@nhh.no.

It is well known that younger workers are at greater risk of becoming unemployed than their older and more established counterparts. This has become particularly evident during the most recent financial crisis and recessionary conditions affecting several countries, especially those in Southern Europe, where most countries have experienced a significant increase in youth unemployment. For some countries, such as Spain and Greece, unemployment rates among the youngest cohorts often exceed 50 per cent. With this in mind, it is of great interest to know how unemployment at an early stage in a worker's career affects future labour market opportunities. If a period of unemployment results in a permanent exit from the labour market, this may be particularly severe for the young who have their entire working career ahead of them, as opposed to older workers closer to the retirement age. This is of serious policy relevance given the concern that young people may become detached from the labour market with the increased risk of a subsequently lower aggregate labour supply. Thus, unemployment may not only induce individual costs, but may have important implications for the economy as a whole, sometimes for many years (OECD, 2011). This is the same reasoning used by policymakers when they construct specific active labour market programmes targeting young workers.

There is already ample evidence of “scarring” effects in the literature, where scarring is defined as the negative long-term effects an incidence of unemployment in *itself* has on future labour market opportunities. Thus, an individual who has been unemployed will be more likely to suffer from negative labour market experiences in the future, when compared to an otherwise identical individual previously not unemployed. For instance, using UK data, Arulampalam (2001), Gregory and Jukes (2001) and Gregg and Tominey (2005) suggest that unemployment leads to subsequent losses in the range of 4 to 14 per cent of the wages. Further, again in the UK, Arulampalam et al. (2000) and Gregg (2001) provide evidence of recurring unemployment, so-called state dependence or scarring effects, in individual unemployment histories.¹ A number of studies provide comparable Scandinavian evidence. For example, Skans (2004) finds a three percentage point increase in the probability of unemployment and a 17 per cent reduction in annual earnings five years after any initial unem-

¹ State dependence (scarring) effects have also been found in Germany. See Biewen and Steffes (2010).

ployment experience. Similarly, Eliason and Storrie (2006) also find strong evidence of earnings losses and a deteriorated employment record following job displacements using Swedish data, while Verho (2008) finds significant effects on employment, particularly significant earnings loss effects, several years after workers experience job displacement using Finnish data. Norwegian papers of particular interest include observations by Raaum and Røed (2006) of patterns of youth unemployment persistence and studies of downsizing (Huttunen et al., 2011; Bratsberg et al., 2013) indicating the increased probability of displaced workers leaving the labour force.²

Given this background, the aim of this paper is to analyse the magnitude of any possible scarring effects of unemployment on future labour market status, namely, being *unemployed* or *out of the labour force*, among workers at an early stage in their careers. At the same time, we analyse the probability of going *back to school*. We regard the return to school as a separate outcome because undertaking additional education potentially represents a commitment to return to work, and may thus be of rather less concern to policymakers than being unemployed or exiting the labour market. In our analysis, we focus on relatively young individuals who already have some work experience prior to potentially experiencing their first spell of unemployment. Restricting the sample in this manner makes the individuals in our sample more homogeneous in terms of labour market experience, and may reduce potential concerns regarding the initial state condition. In addition, work experience provides the unemployed with an incentive to register as such given they are likely to be entitled to unemployment benefits and hence are observable to researchers. Note also that as unemployment is more wide-spread among the youth, it is likely that unemployment is more randomly distributed within this group than among older workers. Thus, focusing on relatively young workers reduces any potential selection problems arising from unob-

² While there is evidence of actual scarring effects in the literature, rather less is known about the cause. Several theories attempt to explain scarring, including the depreciation of human capital (Becker, 1993), psychological discouragement or habituation effects (Clark et al., 2001), theories of job matching where the unemployed accept poorer quality employment (Pissarides, 1994), social work norms that influence individuals' preferences for work (Stutzer and Lalive, 2004) and employers using an individual's unemployment as a signal of low productivity (Lockwood, 1991).

served heterogeneity.³ Following standard practice in labour market studies, we analyse the potential scarring effects separately for males and females. The reasons for any potential gender differences include, for instance, differences in education, choice of occupation, family structures and individual preferences.

The data we use comprise young workers in Norway. Norway has a relatively low youth unemployment compared to many other European countries, with specific active labour market programmes targeted at younger workers.⁴ This suggests that young workers in Norway generally have a higher likelihood of (re)employment, and that the scarring effects in such an economy, if any, are small. Conversely, being one of few unemployed in the Norwegian economy could send a potentially stronger negative signal to employers about the motivation and skills of the applicant as just one of many unemployed in economics where unemployment is more widespread.

Our Norwegian data have several advantages in this type of analysis. First, they provide us with a very long time series. This makes it possible for us to condition on work experience before workers potentially experience unemployment for the first time and investigate the long-term individual effects for several subsequent years. Thus, unlike most studies in this field, we are able to capture the potential scarring effects resulting from an initial period of unemployment as opposed to those associated with accumulated unemployment by individuals with unknown employment histories. Second, the data sources comprise administrative registers, e.g. the public tax register, thereby reducing problems with self-reporting errors, sample attrition, etc. Third, our data are census data, and therefore highly representative, and provide a large number of observations. Finally, unlike most other studies in this field, our data include information on female workers.

Our focus is on workers who registered as unemployed for the first time during the period 1992-1998, a period of both boom and recession in Norway. We form a comparison group, constituted of young individuals

³ There is, of course, an extensive literature on school-to-work transition. However, as we focus on young workers with at least two years of work experience, we do not discuss this literature. We are aware that this restriction could make us underestimate the possible scarring effect given that unemployment could be considered a stronger signal about the qualifications and skills of individuals with less or no work experience.

⁴ For details about Norwegian labour market programmes for youths, see NOU (2011:14, p. 143).

who are employed, and record the labour market status of the two groups over the next 10 years.⁵ To ensure that the two groups are as comparable as possible, we employ a nearest-neighbour propensity score matching method. Our main finding is that there is a significant and persistent positive relationship between early-career unemployment and the future labour market status of being *unemployed*. There also appears to be rather constant but smaller long-run relationships between early-career unemployment and being *out of the labour force* and going *back to school*. This indicates that there may be a considerable scarring effect of unemployment early in a worker's career. We find that the estimated relationships are similar for males and females.

The remainder of the paper is structured as follows. Section 1 presents information about the institutional setting in Norway. Section 2 details the data and Section 3 describes the matching procedure. Section 4 provides the main results and those of several sensitivity analyses. Finally, we offer some concluding remarks in Section 5.

1. Institutional setting

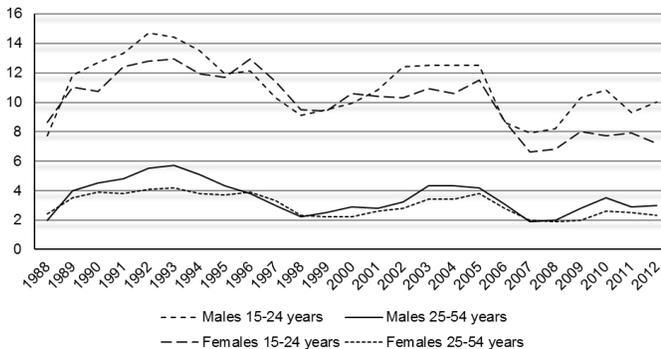
The unemployment rate in Norway has traditionally been very low. In comparison, the average unemployment rate in the 27 member countries of the European Union in 2005 was 8.9 per cent, but only 4.6 per cent in Norway (OECD.Stat). However, like most countries, unemployment in Norway among younger cohorts is much higher than for older individuals. This is clearly depicted in Figure 1, where we plot the youth and overall unemployment rates for males and females in Norway.

For instance, in 1993, during a recession in Norway, the unemployment rate among males aged 15-24 years was 14.4 per cent, but only 5.7 per cent among males aged 25-54. The corresponding figures for females were 12.9 and 4.2 per cent. In 1998, a period of boom in the Norwegian economy, the corresponding figures for males and females were 9.1 and 2.2 per cent and 9.5 and 2.3 per cent, respectively. The gender difference

⁵ We do not focus on wage scarring for those returning to employment. While there is evidence of wage scarring in the literature, this appears to be of less concern in the Norwegian context. For example, Huttunen et al. (2011) find only modest effects of displacement on earnings for those remaining in the labour force, unlike the significant effects of displacement on the probability of leaving the labour force.

in unemployment rates found among younger individuals could result from the fact that males are traditionally employed in sectors that are more exposed to fluctuations in the business cycle (for instance, manufacturing and construction), while females are more typically employed in the public sector. We should also note that females to a much larger extent than males are employed part time (46.7 per cent vs. 9.4 per cent in 1995) and that gender segregation in the Norwegian labour market is quite high (see OECD, 2002). However, females generally have more education than males, at least among the youngest cohorts. For instance, based on the figures available for individuals aged 25-29 years in 1999, 30.6 per cent of the males had a university education compared to 39.2 per cent of the females (Statistics Norway).

Figure 1. Unemployment rates for Norway, by age and gender



Source: Statistics Norway.

Individuals who are either residents or work as employees in Norway are automatically insured under the National Insurance Scheme. The conditions for receiving unemployment benefits are that the worker has previously earned income, has lost a job for reasons beyond the individual's control and is actively seeking employment and is capable of work.⁶ To receive state benefits during the review period of this study (1992-1998), a beneficiary needed to earn a minimum of approximately NOK 50 000 (in 2009 terms) the year prior to becoming unemployed, or twice this

⁶ However, individuals who resign voluntarily, or are dismissed for reasons within their control, may also receive benefits after a waiting period of at least eight weeks.

amount during the three years prior to unemployment (NAV, 2010).⁷ The benefit received is 62.4 per cent of previous earnings up to some maximum amount.⁸ The unemployment benefit period varies depending on previous earnings, where benefits could in practice be received for about three years during the period 1992-1998.⁹

The two main laws regulating hires and fires in Norway are the laws of employment (*Sysselsettingsloven*) and labour relations (*Arbeidsmiljøloven*). However, there is no legal ruling on the selection of workers to be dismissed in the case of a mass lay-off. In the main collective agreement (*Hovedavtalen*) between the labour unions and the employers' association (*Næringslivets Hovedorganisasjon*), it is stated that employers should emphasize seniority when restructuring and during mass lay-offs. However, it is possible for employers to ignore the seniority rule if there are good reasons for this.

2. Data

2.1 Construction of sample

The data are from Statistics Norway and include information on all Norwegian residents aged between 16 and 74. This information includes details of employment relationships, labour market status, earnings, education, age, experience, marital status and municipality of residence, collected from different administrative registers over the period 1986 to 2008. There is also information about the number of months an individual has been registered as unemployed during a particular year.¹⁰ Unfortunately, the registered unemployment variable is only available after 1988. Individuals entitled to unemployment benefits and those who are not may register as unemployed. However, they may only be considered for unemployment benefits if registered.

⁷ 1 NOK \approx 1/8 EUR.

⁸ The maximum benefit in 1998 was approximately NOK 340 000 (in 2009 terms).

⁹ Within a period of 52 weeks, an individual may cease to receive unemployment benefits, for instance, due to employment, and then return to receiving unemployment benefits without having to meet the minimum earnings threshold.

¹⁰ In the data, an initiated month of registered unemployment is recorded as a full month even if the unemployment spell is shorter.

The sample is constructed by pooling all individuals in the period 1992 to 1998, which constitutes what we denote as the base years. These base years are chosen to ensure that one could observe the registered unemployment histories for individuals at least four years prior to any base year and to follow individuals up to ten years after any base year. Given that we are interested in early-career scarring, we limit our sample to those who quit school within 3-4 years prior to a base year.¹¹ We condition on the number of years since school and not age *per se*, so that the more and less educated have a similar amount of labour market experience. Furthermore, we exclude individuals who delayed their schooling and are more than five years off-track as compared to their peers who engaged in education non-stop from when they commenced primary school. Individuals who completed their education two years faster than normal and those with less than nine years of education are also excluded. Further, only individuals who have been working for at least two years prior to the base year are included. This includes all individuals who in the two years prior to a base year satisfy the following criteria: working in Norway for at least twenty hours a week, registered with a plant identification number, classified as receiving a wage in the tax records, and did not complete any education.^{12,13} In addition, we exclude individuals who registered for unemployment benefits in any of the four years prior to a base year. That is, from when they quit school until the base year, none of the individuals in the sample experienced unemployment.¹⁴ By requiring no unemployment and at least two years of work experience, we have made the sample more homogeneous and we believe that this reduces any potential concerns regarding the initial state condition. Consequently, if we identify any scarring effects in the analysis, these are likely due to the initial period of unemployment and do not result from a history of multiple unemployment spells and work instability found among a subgroup of workers with poor employment records. In addition, these criteria make it likely that the individuals in the sample are entitled to unemployment

¹¹ Note that quitting school is not necessarily the same as graduating. Individuals may have completed a degree, finished only some courses, or simply dropped out.

¹² Being registered with a plant identification number indicates having an employer in the register month, being May for 1990 to 1995 and November for 1996 to 1998.

¹³ Note that the criterion of being classified as receiving a wage excludes self-employment.

¹⁴ We do not restrict the individuals in the sample to those who have worked non-stop since they quit school 3-4 years before. Thus, individuals who served in the military, travelled, etc., the year after quitting school are not excluded.

benefits in a base year so that it is economically beneficial for those who lose their job to register as unemployed.

On the basis of an individual's employment status in a base year, we divide the sample into two groups: the employed and the unemployed. The group of employed individuals is those registered with a plant identification number. In addition, individuals with a missing plant identification number, but registered with an identical plant identification number the year prior and subsequent to the year the plant identification number is missing, are also categorized as employed. A further requirement is that the individuals in the employed group should not be registered for any months of unemployment and not be a full-time student (i.e. not registered for ongoing education and working fewer than 20 hours a week) in a given base year.¹⁵ All individuals with registered unemployment in a base year, regardless of whether they are full-time students or have a plant identification number, constitute the unemployed group. Individuals in a base year who are neither part of the employed group nor part of the unemployed group are excluded.¹⁶

For each year over a period of ten years following a base year, we compare the employment statuses of the two groups, i.e. those who were unemployed in a base year versus those who were employed. We refer to these as the follow-up years. For each of the follow-up years, we divide the individual employment statuses into four categories: *employed*, *unemployed*, not participating in the labour force, i.e. *out of the labour force* and *going back to school*. To be classified as *employed* or *unemployed*, the same criteria apply as for the classification of these two groups in a base year. We classify individuals with missing information for multiple accessible employment relationship variables and who are not already classified as *employed* or *unemployed* as *out of the labour force*.¹⁷ Individuals who are full-time students, i.e. registered for ongoing education

¹⁵ Note that this definition of employment includes part-time workers.

¹⁶ Note that even though there are seven base years in total, there is only one base year observation per individual. For individuals satisfying the criteria of being in the sample in multiple base years, we use the earliest base year observation.

¹⁷ The employment relationship variables include the plant identification number, the firm identification number, the municipality of work and the start and termination dates of the employment relationships.

and working fewer than twenty hours a week, are classified as going *back to school*.¹⁸

We specify age, age squared, years of education, earnings (fixed NOK at 2000 prices), marital status and whether the individual is born outside Scandinavia as control variables. We also include information about the type of education, industry, and the size and centrality of residence. Both educational type and industry type are divided into nine categories.¹⁹ The types of residence areas are divided into seven categories based on the size and centrality characteristics defined by Statistics Norway (Hartvedt et al., 1999), ranging from the urban capital region to relatively rural micro regions. In addition, we calculate separate unemployment rates for males and females across 46 regional labour markets.^{20,21}

2.2 Descriptive analysis

Table 1 reports the characteristics of the two groups (unemployed and employed) in a given base year by gender. All characteristics are for the year prior to the base year. We can see that even though the unemployed and employed groups are similar, they are not identical. For instance, individuals in the unemployed group are on average younger, less likely to be married (especially males), and have lower levels of education and lower wages compared to those in the employed group. Among other factors, they are also less likely to work in the public sector and more likely to work in the construction industry, and less likely to live in the capital region. Moreover, individuals in the unemployed group typically live in local labour market areas with higher unemployment rates.

Figures A1 (males) and A2 (females) in Appendix A depict the shares of individuals classified as being *unemployed, out of the labour force* and *going back to school* in the follow-up years, where we split the sample according to the individual's employment status in the base year, i.e. un-

¹⁸ We excluded 7.8 per cent of the individuals in the sample because of inconsistencies in their employment relationship variables over time.

¹⁹ See Statistics Norway (1989) for the education type classification and Statistics Norway (1983) for the industry classification.

²⁰ The 46 regional labour markets are categorized by Statistics Norway and classified according to commuting statistics (Bhuller, 2009).

²¹ We employ data from the Norwegian Social Science Data Services (NSD) to construct these unemployment rates. NSD is not responsible for the analysis of the data nor the interpretations drawn in this study.

employed or employed. As shown, the unemployed group has a higher probability of being in any of the above-mentioned employment statuses in all follow-up years when compared to the employed group. However, these differences may result from differences in the observed characteristics and not the initial unemployment experience. Accordingly, to construct a valid control group for the unemployed group, we employ matching.

Table 1. Descriptive statistics before matching . Mean values and shares. All characteristics measured the year prior to the base year

	Males				Females			
	Un empl.	Empl.	bias (%) ^{a)}	p-value	Un-empl.	Empl.	bias (%) ^{a)}	p-value
Age	22.22	24.46	75.7	0.00	22.59	24.48	66.4	0.00
Yrs. of educ.	11.58	13.14	76.9	0.00	12.08	13.48	68.5	0.00
Earnings in 1 000 ^{b)}	173	237	74.0	0.00	146	196	73.9	0.00
Married	.06	.17	33.5	0.00	.13	.20	20.4	0.00
Non-Scand.	.02	.02	3.1	0.07	.02	.02	0.1	0.98
Education type^{c)}								
General	.14	.08	18.7	0.00	.21	.12	25.6	0.00
Teaching	.01	.03	13.8	0.00	.06	.12	21.7	0.00
Humanities/art	.03	.03	0.2	0.92	.07	.06	6.1	0.03
Business adm.	.10	.19	25.8	0.00	.31	.29	4.3	0.02
Sciences/techn.	.62	.54	17.4	0.00	.10	.10	0.8	0.66
Transport	.02	.02	0.3	0.88	.03	.02	4.5	0.01
Health services	.00	.03	20.4	0.00	.05	.19	44.3	0.00
Agriculture	.03	.03	0.5	0.77	.02	.02	4.3	0.01
Service/defence	.05	.06	3.9	0.04	.16	.09	19.9	0.00
Industry^{c)}								
Agriculture	.04	.03	8.2	0.00	.02	.01	7.5	0.00
Petroleum	.01	.01	5.7	0.00	.00	.01	2.7	0.18
Manufacturing	.28	.23	10.3	0.00	.10	.07	9.7	0.00
Electricity	.05	.03	10.4	0.00	.01	.00	7.5	0.00
Construction	.25	.13	29.9	0.00	.02	.01	7.8	0.00
Wholesale	.18	.17	1.9	0.28	.36	.22	31.4	0.00
Transport	.04	.05	5.3	0.01	.02	.03	5.9	0.00
Finance	.04	.10	25.9	0.00	.07	.08	5.9	0.00
Public	.14	.26	31.2	0.00	.41	.57	33.2	0.00

Table 1. Continued....

	Males				Females			
	Un empl.	Empl.	bias (%) ^{a)}	p- value	Un- empl.	Empl.	bias (%) ^{a)}	p- value
Residence Char.^{c)}								
Capital region	.18	.26	20.5	0.00	.25	.33	17.6	0.00
Metropolis region	.16	.18	3.7	0.05	.18	.17	3.6	0.05
University region	.02	.02	2.2	0.20	.02	.02	1.0	0.61
Centre region	.29	.26	6.6	0.00	.24	.23	2.3	0.21
Med.-size region	.10	.09	4.7	0.01	.09	.08	4.4	0.02
Small-size region	.08	.07	4.6	0.01	.06	.05	2.8	0.12
Micro-size region	.17	.13	11.5	0.00	.16	.12	11.2	0.00
Base years^{c)}								
1992	.33	.22	24.7	0.00	.27	.26	2.1	0.24
1993	.18	.12	17.3	0.00	.17	.14	8.4	0.00
1994	.12	.12	1.6	0.37	.15	.14	3.9	0.03
1995	.10	.11	3.0	0.10	.14	.12	5.3	0.00
1996	.10	.11	4.5	0.01	.12	.11	1.0	0.58
1997	.07	.14	23.2	0.00	.09	.11	8.6	0.00
1998	.09	.17	24.7	0.00	.07	.11	17.3	0.00
Unempl. rates	6.49	5.94	30.8	0.00	4.74	4.56	16.5	0.00
No. of individu- als	3 294	45 139			3 128	45 041		

Source: Own calculations.

Note: a) Absolute standardized bias. For each covariate X , the absolute standardized bias is defined as

$$\left| 100 \cdot \bar{X}_U - \bar{X}_E / \sqrt{0.5 \cdot (V_U(X) + V_E(X))} \right| \text{ where } \bar{X}_U(V_U) \text{ is the mean (variance) in the unemployed group and}$$

 $\bar{X}_E(V_E) \text{ is the mean (variance) in the employed group. b) Fixed NOK in 2000 prices. c) Shares in each category within each group (unemployed and employed). Sums vertically to one.}$

3. Empirical method

3.1 Matching estimator

It would be desirable to compare the two potential outcomes Y_i^1 (labour market status if experienced initial unemployment) and Y_i^0 (labour market status if did *not* experience initial unemployment) in the follow-up years for individuals in the unemployed group. However, we can only observe a single outcome for each individual in the unemployed group, Y_i^1 , and not the potential outcome for these same individuals had they not been unemployed, Y_i^0 .

Instead, we could compare the mean differences in outcomes for all individuals in the unemployed group, the group “treated” with an initial

unemployment period in a base year, and obtain the average treatment effects. We formally define this average treatment effect on the treated (ATT) as:

$$\tau^{ATT} = E(Y^1 | D=1) - E(Y^0 | D=1), \quad (1)$$

where D indicates treatment, i.e. initial unemployment in a base year, and takes a value of $D=1$ if the group experiences initial unemployment, and $D=0$ otherwise. $E(Y^1 | D=1)$ is the mean outcome for individuals in the treated group who experience initial unemployment given that they *are* actually experiencing (read: are treated with) initial unemployment. This means that outcome $E(Y^1 | D=1)$ is observable. On the other hand, the second term in equation (1), $E(Y^0 | D=1)$, is never observed. This hypothetical term denotes the mean outcome for those in the treated group, $D=1$, who do *not* experience initial unemployment. Using the mean outcome of the employed group $E(Y^0 | D=0)$ may not be an appropriate alternative for $E(Y^0 | D=1)$. This non-interchangeability of $E(Y^0 | D=0)$ and $E(Y^0 | D=1)$ is due to the fact that characteristics that determine whether an individual experiences unemployment in a base year are also likely to determine the individual's future labour market status.

One way of dealing with this effect, often referred to as the selection effect, when estimating the ATT is by using a matching method. In essence, this method ensures that a control group, consisting of individuals from the employed group, $D=0$, is equal to the treated group, $D=1$, in terms of observed characteristics (see Caliendo and Kopeinig, 2008 for an overview). For instance, every unemployed 26-year-old man with 13 years of education, five years of work experience, working in the wholesale industry, living in the university region, etc. (...) in a base year, is matched with an employed man with the exact same characteristics. With such matching, we could anticipate that the mean outcome of the employed group $E(Y^0 | D=0)$ could be used as proxy for the hypothetical term $E(Y^0 | D=1)$. However, with many often continuous variables, there will be many groups. To diminish this dimensionality problem, we match the individuals using propensity scores.²²

²² We have considered methods that explicitly control for unobserved characteristics. However, we do not find binary fixed effects panel data methods to be satisfactory in this context

The propensity score, defined as $p_i(x_i) = \Pr(D_i = 1 | x_i)$, assigns each individual i a probability of experiencing unemployment in a base year, given its characteristics x_i . The propensity scores are estimated separately for males and females using logistic regressions. All controls from the summary statistics reported in Table 1, in addition to the square root of age, are included in the estimations. To reduce potential problems caused by the endogeneity of the explanatory variables, all measures are for the year prior to the base year.²³ The estimated propensity score of each individual in the unemployed group is then matched with the nearest estimated propensity score of an individual in the employed group. This form of matching is referred to as the one-to-one nearest-neighbour propensity score matching method. After the matching, we have one employed individual for each unemployed individual in a base year.

3.2 *Assessing the matching quality*

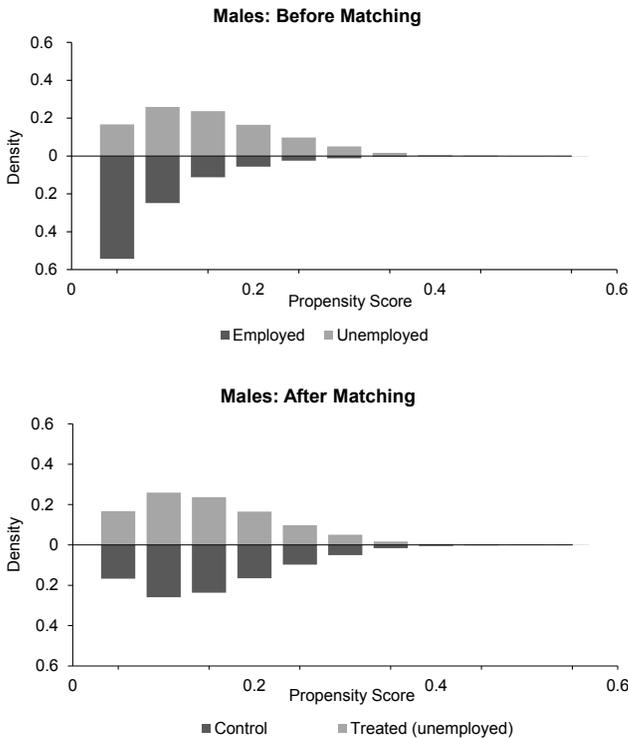
Figures 2 and 3 depict the distributions of the estimated propensity scores before and after matching for males and females, respectively. While the distributions for the unemployed and the employed groups differ, the distributions of the employed groups cover the ranges of the unemployed groups. The extreme values (minimum and maximum) of the propensity score for the unemployed group are within the extreme values of the em-

given that they only utilize information on individuals who experience changes in their employment status over time, thus making the definition of the control group unclear. In addition, if the scarring effect is permanent, it is removed when using fixed effects methods, while not having any spell data available prevents us from applying duration models. In addition, we considered a variety of plausible instruments for unemployment without success. For instance, using downsizing or plant closures to instrument unemployment will not satisfy the exogeneity condition, given that these will have an effect on the subsequent employment status through work-to-work transitions, and not solely through unemployment experience. Given migration decisions and differences in job match qualities, local or business cycle unemployment rates are also invalid as exogenous instruments.

²³ The variables measured prior to treatment are usually considered as exogenous, i.e. they are not influenced by the treatment itself. This is not always the case. For instance, absence because of sickness in the pretreatment period may in itself be the result of working in a firm experiencing downsizings or an increased risk of bankruptcy, which in turn may lead to an initial period of unemployment in a base year. In this sense, absence because of sickness is not exogenous to the experience of initial unemployment and thus, we do not include this in the matching. Nevertheless, we should note that there is information in the data on long-term spells of sickness (lasting 15 days or more) for 1992 onwards. We found that individuals in the treated group have a somewhat higher incidence of sickness in the pretreatment period (i.e. prior to the base year) compared to those in the matched control group.

ployed group (not shown). These patterns are important as they ensure the existence of a counterpart from the employed group for every individual in the treated group, i.e. the unemployed group. This is referred to as the common support condition. The results in Figures 2 and 3 indicate that this condition is satisfied in that after matching, the distributions of the treated and the control groups are visually identical for both genders.²⁴

Figure 2. Propensity scores before and after matching – males

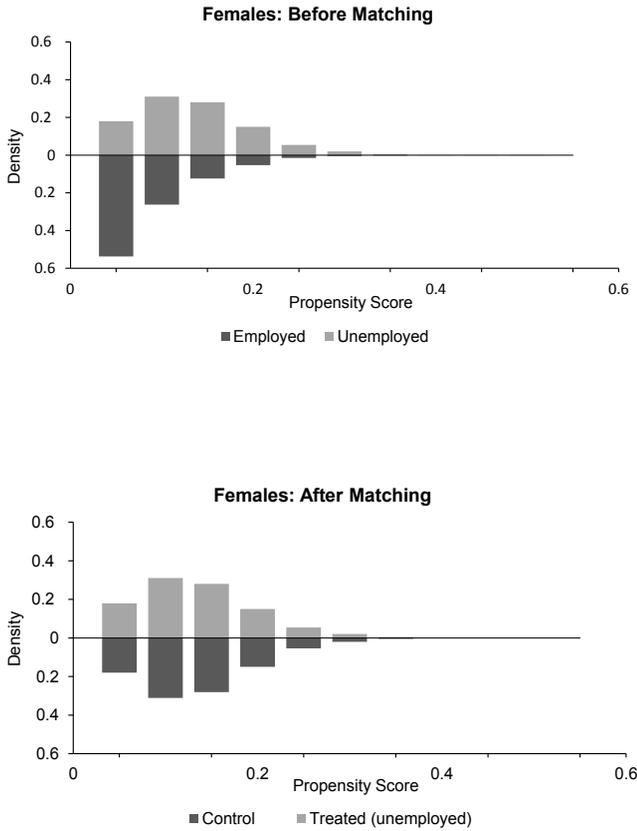


Source: Own calculations.

Note: The employed group is not equal to the control group. The control group consists of a limited sample of the employed group after matching.

²⁴ Another condition, the conditional independence assumption (CIA), also needs to hold when we condition on $p(x)$ instead of X (Rosenbaum and Rubin, 1983). The CIA states that given the observed characteristics, X , the potential outcomes are independent of treatment. Put differently, when the observed characteristics are taken into account, the probability of experiencing unemployment in a base year should be uncorrelated to whether an individual, in fact, experiences unemployment or not in the given base year.

Figure 3. Propensity scores before and after matching – females



Source: Own calculations.

Note: The employed group is not equal to the control group. The control group consists of a limited sample of the employed group after matching.

Table 2. Descriptive statistics after matching. Mean values and shares. All characteristics measured the year prior to the base year

	Males				Females			
	Treated (Un-empl.)	Control	bias (%) ^{a)}	p-value	Treated (Un-empl.)	Control	bias (%) ^{a)}	p-value
Age	22.22	22.14	2.6	0.23	22.59	22.59	0.2	0.95
Yrs. of educ.	11.58	11.55	1.7	0.40	12.08	12.09	0.5	0.84
Earnings in 1 000 ^{b)}	173	172	1.1	0.64	146	147	0.9	0.71
Married	.06	.06	1.3	0.50	.13	.13	0.3	0.91
Non-Scand.	.02	.02	2.8	0.26	.02	.02	0.7	0.78
Education type^{c)}								
General	.14	.15	3.3	0.24	.21	.21	0.4	0.90
Teaching	.01	.01	0.8	0.64	.06	.06	1.1	0.59
Humanities/art	.03	.03	0.4	0.88	.07	.08	3.7	0.17
Business adm.	.10	.10	1.1	0.59	.31	.31	1.6	0.53
Sciences/techn.	.62	.62	1.2	0.61	.10	.09	0.2	0.93
Transport	.02	.02	0.0	1.00	.03	.03	1.4	0.61
Health services	.00	.00	0.0	1.00	.05	.04	2.1	0.21
Agriculture	.03	.03	0.4	0.89	.02	.02	1.0	0.72
Service/defence	.05	.05	2.8	0.22	.16	.15	2.5	0.36
Industry^{c)}								
Agriculture	.04	.04	0.3	0.90	.02	.02	0.3	0.92
Petroleum	.01	.01	0.4	0.86	.00	.00	0.0	1.00
Manufacturing	.28	.28	1.3	0.60	.10	.10	0.7	0.80
Electricity	.05	.05	1.8	0.52	.01	.01	2.5	0.39
Construction	.25	.24	1.9	0.49	.02	.02	0.9	0.76
Wholesale	.18	.18	0.9	0.73	.36	.37	0.7	0.79
Transport	.04	.03	1.5	0.50	.02	.02	1.0	0.67
Finance	.04	.04	0.5	0.79	.07	.07	0.0	1.00
Public	.14	.13	0.6	0.77	.41	.41	0.1	0.98
Residence Char.^{c)}								
Capital region	.18	.18	0.3	0.90	.25	.25	0.0	1.00
Metropolis region	.16	.17	1.1	0.67	.18	.18	1.8	0.47
University region	.02	.02	1.5	0.56	.02	.02	0.9	0.72
Centre region	.29	.28	1.4	0.59	.24	.25	1.1	0.68
Med.-size region	.10	.10	2.1	0.41	.09	.09	2.5	0.36
Small-size region	.08	.08	0.3	0.89	.06	.06	0.8	0.74
Micro-size region	.17	.18	2.7	0.30	.16	.16	1.0	0.71
Base years^{c)}								
1992	.33	.33	0.3	0.92	.27	.27	1.3	0.61
1993	.18	.17	3.3	0.21	.17	.18	0.7	0.79
1994	.12	.13	0.9	0.71	.15	.15	0.7	0.78
1995	.10	.11	0.9	0.72	.14	.14	0.4	0.88
1996	.10	.10	0.3	0.90	.12	.12	0.9	0.72
1997	.07	.07	0.4	0.85	.09	.09	1.4	0.56
1998	.09	.09	1.4	0.52	.07	.06	0.9	0.68
Unempl. rates	6.49	6.44	2.4	0.32	4.74	4.73	1.5	0.56
No. of individuals	3 294	3 294			3 128	3 128		

Source: Own calculations.

Note: See notes to Table 1.

The results in Table 2 show that the means of the observed characteristics for the treatment and the control groups are very similar after matching. The *p*-values of the *t*-tests show that none of the means are significantly different between the two groups. Furthermore, there is no absolute standardized bias (Rosenbaum and Rubin, 1985) greater than 4 per cent for any of the observed characteristics for either males or fe-

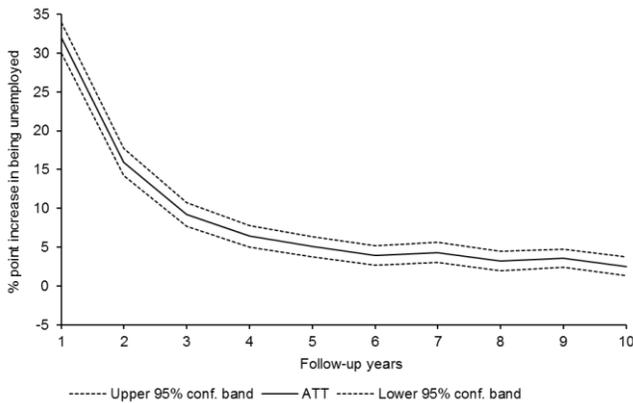
males. Hence, the matching procedure has generated a control group for the treated group that is, on average, identical in terms of the observed characteristics.

4. Results

4.1 Main results

Figures 4, 5 and 6, respectively, depict (for males) the average treatment effects on the treated, ATTs, i.e. the differences in the probability of being *unemployed*, *out of the labour force* and *going back to school* in the follow-up years. The ATTs are the mean differences in outcomes between the group consisting of those who experience initial unemployment in a base year (the treated group) relative to the control group.

Figure 4. Average treatment effect on the treated (ATT) on the probability of being unemployed in the follow-up years – males



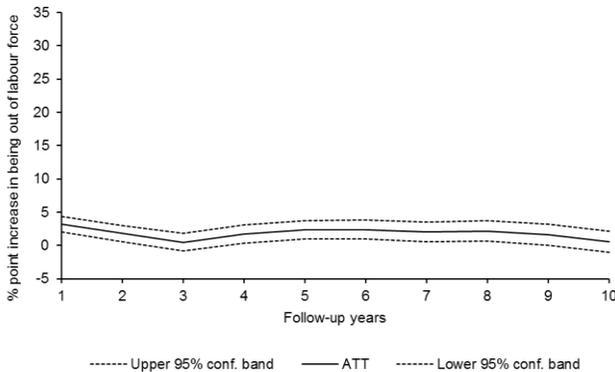
Source: Own calculations.

Note: The confidence band is calculated assuming the ATT to follow a normal distribution (reported standard errors from STATA routine *psmatch2*).

Starting with the average treatment effects on the treated of being *unemployed* in Figure 4, we can see that this is somewhat higher than 30 percentage points in the first follow-up year. Thus, individuals who experienced unemployment in a base year are on average 30 percentage points more likely to be unemployed this year relative to similar individuals who

did not experience unemployment in a base year. Note, however, that in the first follow-up year, it is likely that individuals in the treated group are in the same continuous unemployment spell that started in a base year.²⁵ The estimated effect drops to about 5 percentage points in the fifth year. Looking at the probabilities behind this figure in follow-up year five (not shown), we find that those in the control group have a probability of 5.3 per cent of being *unemployed*, while the corresponding number for the individuals in the treated group is much higher at 10.4 per cent. Turning to the evolution over time, we see that the average treatment effects appear to stabilize at 4 percentage points from follow-up year six onwards.

Figure 5. Average treatment effect on the treated (ATT) on the probability of being out of the labour force in the follow-up years – males



Source: Own calculations.

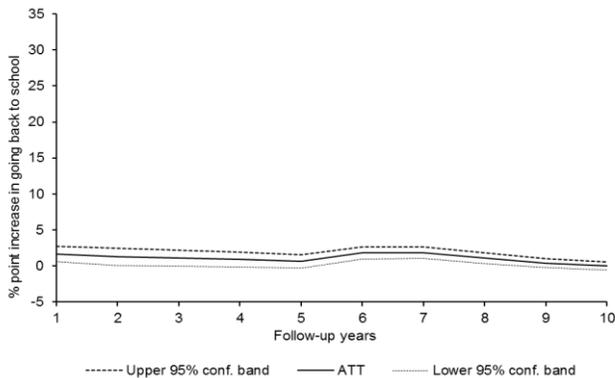
Note: See note to Figure 4.

Moving now to the treatment effects on *out of the labour force*, this appears quite stable over time, fluctuating around 2 percentage points. This appears consistent with the findings in Huttunen et al. (2011) (see

²⁵ The data do not allow us to investigate how many individuals are in the one continuous unemployment spell. We only observe the number of months an individual is registered as unemployed each year, so the individual may have been repeatedly unemployed both within the same year, and from one year to the next. Therefore, we are prevented from performing more detailed analyses of the duration of unemployment spells. However, most of the unemployed experience relatively short unemployment spells. For instance, recent figures show that 57.3 per cent of the registered unemployed aged 25-29 years had a spell duration of less than three months and only 5.6 per cent had a spell duration of more than one year (Norwegian Labour and Welfare Service). Hence, we are inclined to believe that most of the long-term effects are driven by repeated unemployment.

their Figure 3), where they analyse the effects of job displacement in Norway. Admittedly, the *out of the labour force* effects found in this study are smaller. One reason could be that in addition to *unemployment* and *out of the labour force*, we are analysing *going back to school* separately. The *back to school* effect is also stable over time and relatively small.²⁶

Figure 6. Average treatment effect on the treated (ATT) on the probability of back to school in the follow-up years – males



Source: Own calculation.

Note: See note to Figure 4.

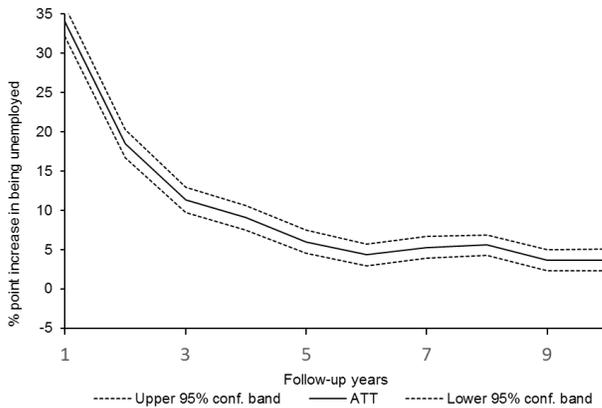
Our unemployment scarring effects align with related studies analysing relatively young individuals. Interestingly, Arulampalam (2002) finds that the scarring effects are smaller for younger individuals (those less than 25 years old). She states: “This is consistent with the view that although the incidence of unemployment is generally higher among the younger men relative to older men, the younger men are less scarred by their experience in terms of relative probabilities.” In an earlier version of the present paper (Nilsen and Reiso, 2011), the average age of the individuals was two years older, in which case the scarring effects were found to be somewhat larger. We also note that we consider our current sample to be positively selected, given that the included individuals have at least

²⁶ In an earlier version of this work (Nilsen and Reiso, 2011), we grouped *out of the labour force* and *back to school* together. We found, not very surprisingly, that the pattern over time was the same, but that the probability of being out of the labour force (which included back to school) was larger. This could indicate that the merging of the two subgroups causes some problems when analysing the effects of unemployment for relatively young individuals.

two years of work experience and no unemployment experience since they quit school. As mentioned, these requirements are induced to reduce potential concerns regarding the initial state condition and to make the sample more homogeneous in terms of labour market experience. In addition, we only include individuals who quit school within a time frame of two years prior to, and five years after, what is expected had they undertaken their education non-stop from when they commenced primary school. Thus, when we find unemployment scarring for this somewhat selected sample, we could interpret the effects as a lower bound. It is also important to keep in mind that recurring unemployment is and should be of concern, whether it is due to state dependency or unobserved heterogeneity, even though the policy implications of the two differ.

Figures 7, 8 and 9 depict the comparable findings for females, corresponding to the differences in the incidences of *unemployment*, *out of the labour force* and *back to school*, respectively. Somewhat surprisingly, we find the pattern for females to be very similar to that for males. As discussed, in Norway, females appear to undertake more education, typically work in different industries and tend to be more family oriented earlier in the life cycle when compared to males.

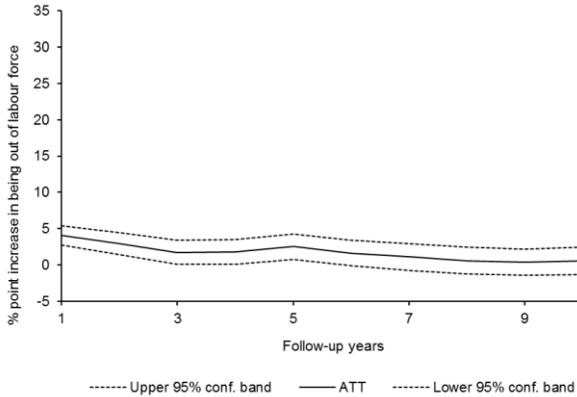
Figure 7. Average treatment effect on the treated (ATT) on the probability of being unemployed in the follow-up years – females



Source: Own calculations.

Note: See note to Figure 4.

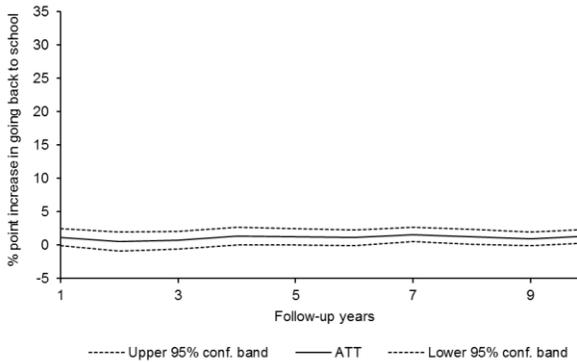
Figure 8. Average treatment effect on the treated (ATT) on the probability of being out of the labour force in the follow-up years – females



Source: Own calculations.

Note: See note to Figure 4.

Figure 9. Average treatment effect on the treated (ATT) on the probability of back to school in the follow-up years – females



Source: Own calculations.

Note: See note to Figure 4.

As a robustness check, we estimate a model with months of unemployment per year as an outcome (where the number of months is zero for those with no registered unemployment in the given follow-up year).²⁷

²⁷ These results, together with those described in the subsequent paragraph, are not shown, but are available from the authors upon request.

We find that those in the treatment group who experience unemployment in a base year are on average unemployed for an additional 1.2 months in the first follow-up year as compared to those in the control group. However, this difference contracts rather rapidly and remains between 0.1 and 0.2 months (i.e. less than a week) from follow-up year five and onwards. However, it is unclear what this model really captures, for instance, whether these results are driven by the fact that more individuals in the treated group are unemployed in the follow-up years as compared to the control group, or perhaps whether those in the treated group who experience unemployment are unemployed for a larger fraction of the follow-up years. To investigate the latter, we examine the number of months of unemployment among those actually unemployed in the follow-up years, i.e. the number of months *conditional* on experiencing unemployment. In doing so, we find that among those who experience unemployment in the first follow-up year, the individuals in the treated group are on average unemployed for an additional month compared to those in the control group. This difference contracts to about zero in the subsequent follow-up years. Thus, in the long run, it appears that even though individuals in the treated group are more likely to become unemployed, they do not necessarily have longer unemployment spells than those in the control group.

With the current recessionary conditions in southern Europe in mind, an interesting and relevant question is whether the potential scarring effects vary with the business cycle at the time of initial unemployment. If one believes that the scarring effect stems from signalling, i.e. that employers use individual unemployment histories as a signal of low productivity and favour those with less unemployment, one could hold the prior belief that individuals experiencing initial unemployment in recessionary years could be less scarred. The reason for this is that being unemployed in such a situation is the norm rather than the exception and does not send a strong signal to the employers. We split the two samples, males and females respectively, such that two subsamples include those who experience unemployment in the base years of a recession (1992 and 1993), and two subsamples include those who experience unemployment in the remaining base years (1994-1998). The relationship between initial unemployment and future *unemployment* is found to be smaller in the long run (follow-up years 4-10 for males and 6-10 for females) for the subsamples experiencing unemployment in the base years of a recession, compared to

the subsamples experiencing unemployment in the remaining base years. This pattern is consistent with the signalling theory. However, the findings are also consistent with a selection story where the unobserved characteristics of individuals experiencing initial unemployment may vary with the general level of unemployment. That is, when more individuals are affected by unemployment during recessions, the unemployed may be more productive, on average, compared to those who are unemployed during periods of expansion. If our controls (including years of education and previous earnings) are unable to fully capture productivity, this could also explain the observed pattern. Thus, to conclude from a single sample split which theory or theories explain the scarring effects and/or which unobserved characteristics account for the revealed pattern is rather speculative. Note also that for both males and females, the patterns of the ATTs for these subsamples do not differ to any considerable extent from the results for the full sample already reported.

4.2 Sensitivity analysis

Even though we control for a variety of observed characteristics, there could be unobserved factors, such as productivity, preferences for work and ability, which affect both the probability of becoming unemployed in a base year and the outcome variables in the follow-up years. To address this so-called unobserved selection issue, we apply a procedure proposed by Rosenbaum (2002). This procedure tests how much these unobserved factors must influence the selection process into being treated, i.e. experiencing unemployment in a base year, before the estimated effects are no longer significant.²⁸

Appendix B includes details of the Rosenbaum bounding approach. Based on the results in Table B1, we state the following. The estimated effect of being *unemployed* for males is not especially sensitive to unobserved selection bias (all but a small minority of the *p*-values in the follow-up years are zero when changing the individual relative differences of receiving treatment by a factor of 1.5, i.e. 50 per cent). However, the estimated effects for *out of the labour force* and *back to school* are more sensitive. Turning to females, the overall finding is consistent with the

²⁸ In addition to Rosenbaum (2002), Aakvik (2001) and Caliendo and Kopeinig (2008) also provide useful overviews of this approach.

reported results for males. However, note that this does not infer that selection biases are present. What we may conclude is that given that most of the estimated effects of initial unemployment on being *unemployed* are robust to a relatively high level of unobserved selection bias, these effects are unlikely to solely be caused by selection on unobservable factors.

5. Concluding remarks

This paper contributes to the existing literature by investigating a possible scarring effect of initial unemployment on future labour market status for early-career workers with some years of work experience. We conduct separate analyses for males and females. Taking advantage of rich register data from Norway, we use a matching estimator to construct a control group that is as similar as possible with regard to observables as the individuals experiencing an incidence of unemployment. This is done in an attempt to disentangle the effects of observables and the potential scarring effects.

The main finding is that there is a persistent negative relationship between early-career unemployment and future labour market status for both genders. For males, the average treatment effects on *unemployment* start at about 30 percentage points in the first follow-up year and decline to 5 percentage points by the fifth year. In contrast, the treatment effects on being *out of the labour force* and *back to school* are about 2 percentage points and rather stable over time. Comparing males and females, we find the patterns to be very similar. When we analyse the sensitivity of the results using the Rosenbaum (2002) bounding approach, we find the majority of the estimated effects of initial unemployment on *unemployment* to be robust to a relatively high level of unobserved selection bias. Thus, it appears as if unemployment leaves early-career workers with long-term employment scars. The existence of these scars is consistent with the findings of other Scandinavian studies of labour displacement, even though most of these are based on older and more established workers. Furthermore, individuals who experience unemployment at an early stage in their career face a longer time horizon until retirement, thereby making the long-term scarring effects particularly severe.

The results of our analysis are for individuals with at least two years of labour market experience prior to the incidence of unemployment. The unemployed with no prior work experience may be even more scarred. Thus, when there is a strong indication that early labour market history is decisive for subsequent labour market success, these findings may be used as support for significant public expenditures targeting young workers. Such policies may also be justified knowing that there is an intergenerational correlation in unemployment (Ekhaugen, 2009). However, to obtain more specific policy recommendations, research is needed regarding the exact cause(s) of unemployment scarring. Given the incidence of extremely high unemployment among youth, especially in southern Europe, this is an important and urgent topic for future research.

References

- Aakvik, A. (2001), Bounding a matching estimator: The case of a Norwegian training program, *Oxford Bulletin of Economics and Statistics* 63, 115-143.
- Arulampalam, W. (2001), Is unemployment really scarring? Effects of unemployment experiences on wages, *Economic Journal* 111, 585-606.
- Arulampalam, W. (2002), State dependence in unemployment incidence: Evidence for British men revisited, IZA Discussion Paper 630, Bonn.
- Arulampalam, W., Booth, A.L. and Taylor, M.P. (2000), Unemployment persistence, *Oxford Economic Papers* 52, 24-50.
- Becker, G.S. (1993), *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education*, third edition, NBER with University of Chicago Press, Chicago.
- Bhuller, M.S. (2009), *Inndeling av Norge i arbeidsmarkedsregioner*, Notater 2009:24, Statistics Norway, Oslo.
- Biewen, M. and Steffes, S. (2010), Unemployment persistence: Is there evidence for stigma effects?, *Economic Letters* 106, 188-190.
- Bratsberg, B., Fevang, E. and Røed, K. (2013), Job loss and disability insurance, *Labour Economics* 24, 137-150.
- Caliendo, M. and Kopeinig, S. (2008), Some practical guidance for the implementation of propensity score matching, *Journal of Economic Surveys* 22, 31-72.
- Clark, A.E., Georgellis, Y. and Sanfey, P. (2001), Scarring: The psychological impact of past unemployment, *Economica*, New Series 68, 221-241.
- Ekhaugen, T. (2009), Extracting the causal component from the intergenerational correlation in unemployment, *Journal of Population Economics* 22, 97-113.
- Eliason, M. and Storrie, D. (2006), Lasting or latent scars? Swedish evidence on the long-term effects of job displacement, *Journal of Labour Economics* 24, 831-856.
- Gregg, P. (2001), The impact of youth unemployment on adult unemployment in the NCDS, *Economic Journal* 111, 626-653.

- Gregg, P. and Tominey, E. (2005), The wage scar from male youth unemployment, *Labour Economics* 12, 487-492.
- Gregory, M. and Jukes, R. (2001), Unemployment and subsequent earnings: Estimating scarring among British men 1984-94, *Economic Journal* 111, F607-F625.
- Hartvedt, H., Hustoft, A.G., Nymoen, E., Stålnacke, M. and Utne, H. (1999), Standard for økonomiske regioner, Reports 99/6, Statistics Norway, Oslo.
- Huttunen, K., Møen, J. and Salvanes, K.G. (2011), How destructive is creative destruction? Effects of job loss on mobility, withdrawal and income, *Journal of the European Economic Association* 9, 840-870.
- Lockwood, B. (1991), Information externalities in the labour market and the duration of unemployment, *Review of Economic Studies* 58, 733-753.
- Mantel, N. and Haenszel, W. (1959), Statistical aspects of the analysis of data from retrospective studies of disease, *Journal of the National Cancer Institute* 22, 719-747.
- NAV (2010), Grunnbeløpet (G), Norwegian Labour and Welfare Administration.
- Nilsen, Ø.A. and Reiso, K.H. (2011), Scarring effects of unemployment, IZA Discussion Paper 6198, Bonn.
- NOU 2011:14, Bedre integrering; Mål, strategier, tiltak (Improved integration: Goals strategies and policies), Norges offentlige utredninger, Oslo.
- OECD Economic Outlook (2011), Persistence of High Unemployment: What Risks? What Policies?, Chapter 5, 253-285.
- OECD Employment Outlook (2002), Females at Work: Who are They and How are They Faring?, Chapter 2, 61-125.
- Pissarides, A. (1994), Search unemployment with on-the-job search, *Review of Economic Studies* 61, 457-475.
- Raaum, O. and Røed, K. (2006), Do business cycle conditions at the time of labour market entry affect future employment possibilities?, *Review of Economics and Statistics* 88, 193-210.
- Rosenbaum, P.R. (2002), *Observational Studies*, Springer, New York.
- Rosenbaum, P.R. and Rubin, D.B. (1983), The central role of propensity score in observational studies of causal effects, *Biometrika* 70, 41-55.
- Rosenbaum, P.R. and Rubin, D.B. (1985), Constructing a control group using multivariate matched sampling methods that incorporate the propensity score, *American Statistician* 39, 33-38.
- Skans, O.N. (2004), Scarring effects of first labour market experience: A sibling based analysis, IFAU Working Paper 2004:14, Uppsala.
- Statistics Norway (1983), Standard industrial classification, Statistics Norway, Oslo-Kongsvinger.
- Statistics Norway (1989), Norwegian standard classification of education, Statistics Norway, Oslo-Kongsvinger.
- Stutzer, A. and Lalive, R. (2004), The role of social norms in job searching and subjective well-being, *Journal of the European Economic Association* 2, 696-719.
- Verho, J. (2008), Scars of recession: The long-term costs of the Finnish economic crisis, IFAU Working Paper 9, Uppsala.

Appendix A

Figure A.1 Shares of males in the two groups (employed and unemployed) being unemployed, out of the labour force or back to school in the follow-up years

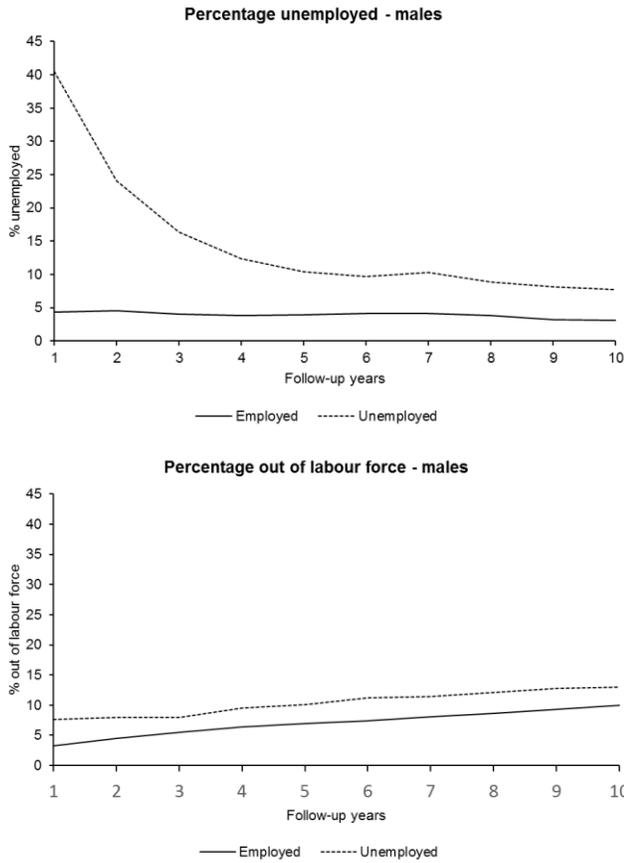
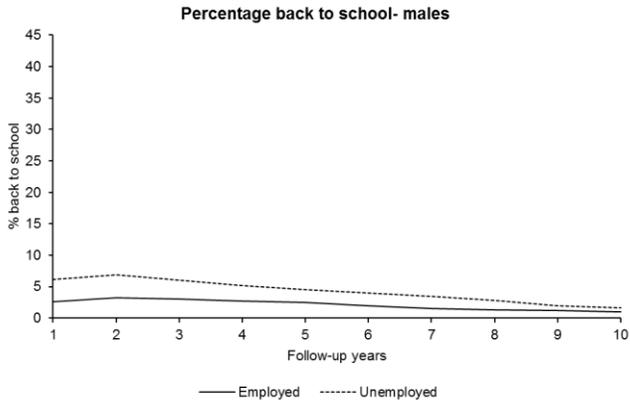


Figure A.1 Continued....



Source: Own calculations.

Figure A.2 Shares of females in the two groups (employed and unemployed) being unemployed, out of the labour force or back to school in the follow-up years

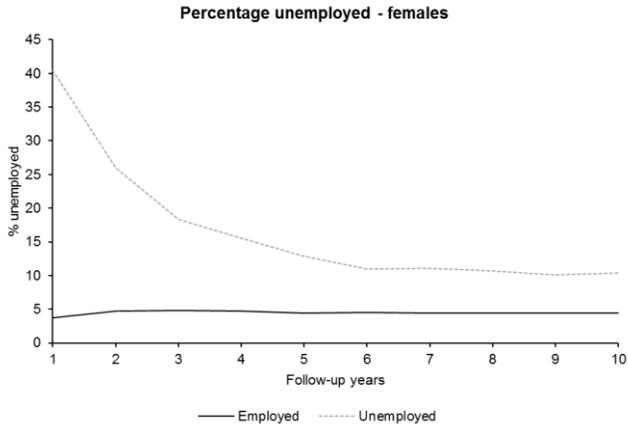
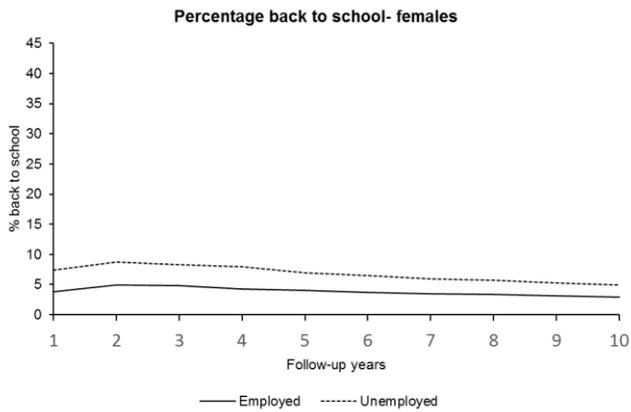
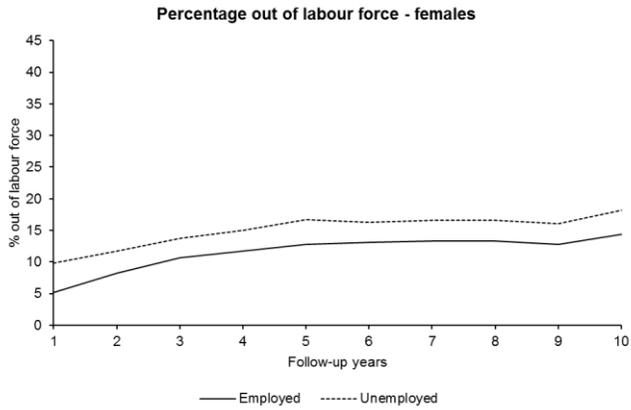


Figure A.2 Continued....



Source: Own calculations.

Appendix B

The start of the Rosenbaum bounding approach is the probability for individual i of being treated:

$$\pi_i = \Pr(D_i = 1 \mid x_i) = F(\beta x_i + \gamma u_i), \quad (\text{B.1})$$

where u_i is an unobserved variable and γ is the effect of u_i on the probability of being in the treated group. If we have a matched pair of individuals, i and j , with the same observed characteristics x , the odds ratio $(\pi_i/(1-\pi_i))/(\pi_j/(1-\pi_j))$ (i.e. the relative odds of receiving treatment for these two individuals), given that we let $F(\cdot)$ be the logistic distribution, may be written as:

$$\frac{\frac{\pi_i}{1-\pi_i}}{\frac{\pi_j}{1-\pi_j}} = \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} = \frac{e(\beta x_i + \gamma u_i)}{e(\beta x_j + \gamma u_j)} = e[\gamma(u_i - u_j)], \quad (\text{B.2})$$

where e is the exponential function. The x vector cancels out given that the two matched individuals have the same observed characteristics. Assuming u to be binary, $-1 \leq (u_i - u_j) \leq 1$, equation (B.2) may be rewritten as:

$$\frac{1}{e^\gamma} \leq \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} \leq e^\gamma. \quad (\text{B.3})$$

If $e^\gamma = 1$, the two matched individuals have the same probability of being in the treated group. If a value of e^γ slightly larger than 1 changes the inference about the effects of treatment, the estimated effects are interpreted as being sensitive to unobserved selection bias. In contrast, if a large value of e^γ does not change the inference, the estimated effects are insensitive to unobserved selection bias. In line with Aakvik (2001), $e^\gamma = 2$ is considered to be a very large number, changing the individual's

relative differences of receiving treatment by a factor of 2, i.e. 100 per cent.

There are two bounds related to the test procedure: a test statistic when the effects of treatment are overestimated, denoted Q_{MH}^+ , and another test statistic when the effects of treatment are under-estimated, denoted Q_{MH}^- . Table B1 reports the p -values for both the upper and the lower Mantel-Haenszel (MH) bounds (see Mantel and Haenszel, 1959) for various values of e^y ; i.e. $e^y = 1$ (the reference point of no unobserved selection bias), $e^y = 1.5$ and $e^y = 2$. It does this for the estimated effect of initial unemployment on being *unemployed, out of the labour force* and *back to school* in each follow-up year.

Table B.1 Robustness check, MH test

Follow-up years	1		2		3		4		5		6		7		8		9		10	
	mh+	mh-																		
Males																				
<i>Unemployed</i>																				
$e^{\gamma} = 1.0$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
$e^{\gamma} = 1.5$	0	0	0	0	0	0	0	0	0	0	0.05	0	0.02	0	0.20	0	0.03	0	0.44	0
$e^{\gamma} = 2.0$	0	0	0	0	0	0	0.11	0	0.40	0	-	0	-	0	-	0	-	0	-	0
<i>Out of the labour force</i>																				
$e^{\gamma} = 1.0$	0	0	0	0	0.24	0.24	0.01	0.01	0	0	0	0	0	0	0	0	0.02	0.02	0.25	0.25
$e^{\gamma} = 1.5$	0.06	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0
$e^{\gamma} = 2.0$	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0
<i>Back to school</i>																				
$e^{\gamma} = 1.0$	0	0	0.02	0.02	0.03	0.03	0.05	0.05	0.11	0.11	0	0	0	0	0	0	0.11	0.11	0.54	0.54
$e^{\gamma} = 1.5$	-	0	-	0	-	0	-	0	-	0	0.08	0	0.07	0	0.32	0	-	0	-	0
$e^{\gamma} = 2.0$	-	0	-	0	-	0	-	0	-	0	-	0	0.36	0	-	0	-	0	-	0
Females																				
<i>Unemployed</i>																				
$e^{\gamma} = 1.0$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
$e^{\gamma} = 1.5$	0	0	0	0	0	0	0	0	0	0	0.07	0	0.02	0	0	0	0.20	0	0.25	0
$e^{\gamma} = 2.0$	0	0	0	0	0	0	0	0	0.51	0	-	0	0.49	0	0.16	0	-	0	-	0
<i>Out of the labour force</i>																				
$e^{\gamma} = 1.0$	0	0	0	0	0.02	0.02	0.02	0.02	0	0	0.04	0.04	0.12	0.12	0.27	0.27	0.35	0.35	0.30	0.30
$e^{\gamma} = 1.5$	0.04	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0
$e^{\gamma} = 2.0$	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0
<i>Back to school</i>																				
$e^{\gamma} = 1.0$	0.04	0.04	0.23	0.23	0.15	0.15	0.02	0.02	0.02	0.02	0.03	0.03	0	0	0.02	0.02	0.04	0.04	0.01	0.01
$e^{\gamma} = 1.5$	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0
$e^{\gamma} = 2.0$	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-	0

Source: Own calculations.

Note: *mh+* denotes the *p*-values of the upper bound and *mh-* denotes the *p*-values of the lower bound. A "-" denotes a negative treatment effect resulting from assuming a large positive unobserved heterogeneity bias.

Comment on Nilsen and Holm Reiso: Scarring effects of early career unemployment

Björn Tyrefors Hinnerich*

The paper by Nilsen and Holm Reiso raises the question: Does youth unemployment have a negative long-term effect on future labor market possibilities *in itself*? That is: Does youth unemployment cause negative labor market outcomes later in life for the affected population? No doubt, this is a highly important question to ask, but from a causal point of view, it is also a question that it is difficult to answer. I believe that Nilsen and Holm Reiso do a good job, given the circumstances. Moreover, the paper is very easy to digest and clearly written and methodological problems are discussed.

In this comment, I will start with a methodological discussion. Second, I will propose some improvements that would strengthen their empirical design. Finally, I will argue that even though the method used has some problems, from a policy perspective, it is reasonable to accept the hypothesis put forward and move on to randomized controlled experiments in order to understand the driving mechanism causing scarring effects.

In order to create a control group, Nilsen and Holm Reiso use the method of propensity score matching, a method that summarizes all observable variables into a single score and thus provides a solution to a dimensionality problem. This matching procedure attempts to mimic randomization by creating a sample of units that received the treatment

* Department of Economics, Stockholm University, bjorn.hinnerich@ne.su.se.

(early unemployment) that is comparable on all observables to a sample of units that did not receive the treatment (not early unemployed). After creating the two groups, differences in future labor market outcomes are estimated. For example, Nilsen and Holm Reiso find that the probability of being unemployed one year after the treatment is about 30 percentage points higher for the treatment group. After five years the effect is smaller, around 5 percentages points, but still significant, indicating scarring effects.

Many other studies (cited in their paper) show evidence of scarring effects. Nilsen and Holm Reiso restrict the analysis to a country (Norway) with a very strong labor market with low unemployment both for young and older workers. Furthermore, they only analyze young people with some previous labor market experience. If scarring effects appear under these economic circumstances, the argument goes, then these could be considered as a lower bound. I believe that the argument has merit, given the plentiful evidence of scarring effects in the literature.

Although the results hold up for standard tests, these tests are never better than the variables they defined as observable. That is, we might still suspect that treatment and control groups are unbalanced with respect to other variables not taken into account. Let me start by mentioning a few additional observables that could be of importance and that could potentially be obtained.

Recent research has shown that cognitive and non-cognitive skills measured early in life seem to affect future success in life (e.g. Herrnstein and Murray, 1994 and Lindqvist and Vestman, 2011). However, these observables are missing when constructing the control/treatment groups. In Sweden, military draft data has been used to construct cognitive and non-cognitive skill measures affecting the future return on investments (Lindqvist and Vestman, 2011). A drawback with using military draft data is that we constrain the analysis to men. On the other hand, the results in the paper show that men and women are not affected differently. In the absence of military draft data, grade data from junior high or high school in for example math, or even better test score data, could be used in order to measure cognitive skills. I believe that Nilsen and Holm Reiso's measures used – education type – are insufficient measures of cognitive skills. Moreover, socioeconomic background variables, such as par-

ents' income and education, should be feasible to collect and are necessary for a credible design.

Relating to Skans (2004), we would expect the estimate of the scarring effect to decrease, since the unknowns (the omitted variables) are likely to bias the estimates upwards. Moreover, further unobservable variables such as innate ability and family characteristics would still leave us somewhat skeptical. The major result in Skans (2004), where unobserved family characteristics are taken into account, is that effects beyond a five-year horizon are no longer statistically significant. Thus, I am more confident of the result, although potentially somewhat exaggerated, in the short run. However, I feel more uncertain of the really long-term effects presented. Now, let me now turn to a policy discussion.

Given the vast evidence of scarring effects in the literature, the results do not seem unrealistic, although possibly somewhat inflated. Nilsen and Holm Reiso mention in that "there is evidence of actual scarring effects in the literature, rather less is known about the cause. Several theories attempt to explain scarring, including the depreciation of human capital ..., psychological discouragement or habituation effects ..., theories of job matching where the unemployed accept poorer quality employment..., social work norms that influence individuals' preferences for work ... and employers using an individual's unemployment as a signal of low productivity" (footnote 2).

Depending on which of those mechanisms that is most important, we need to design different programs/treatments in order to efficiently reduce scarring effects. If human capital depreciation is the main driver, then active labor training would potentially be the best solution. However, if signaling is what matters, training does not help and it may even increase the scars. Being part of the program might even be a stronger "bad signal". If the effects are driven by psychological discouragements, then cognitive behavioral therapy should maybe be an integral part of active labor market policies.

I believe that the main argument for the paper, as argued by Nilsen and Holm Reiso, is that even in unlikely settings, we can verify scarring effects. Therefore, I think that the authors should argue strongly for randomized controlled trials in order to distinguish the mechanisms causing scarring effects. A more general discussion on this matter and ideas on how to implement randomized experiments are given too little weight in

the paper. The OECD countries already spend large sums on active labor market policies so the cost of setting up randomized experiments with different treatments should be well within reasonable budget limits.¹

References

- Forslund, A., Fredriksson, P. and Vikström, J. (2011), What active labor market policy works in a recession?, *Nordic Economic Policy Review* 1/2011, 171-201.
- Hernstein, R.J. and Murray, C. (1994), *The Bell Curve: Intelligence and Class Structure in American Life*, Free Press, New York.
- Lindqvist, E. and Vestman, R. (2011), The labor market returns to cognitive and noncognitive ability: Evidence from the Swedish Enlistment, *American Economic Journal: Applied Economics* 3, 101-128.
- Skans, O.N. (2004), Scarring effects of first labour market experience: A sibling based analysis, IFAU Working Paper 2004:14, Uppsala.

¹ See Forslund et al. (2011) for exact figures.

Bad times at a tender age – How education dampens the impact of graduating in a recession*

Kai Liu^{**}, Kjell G. Salvanes^{***} and Erik Ø. Sørensen^{****}

Summary

We study the effect of entering the labor market in good times as opposed to bad times: to what degree may education dampen short- and long-term negative labor market effects of finishing school in a recession? We focus on vocational training, which has been underdeveloped in many countries with high youth unemployment, but also assess the outcomes for those without a completed high school degree, those with only an academic high school degree and those with a college degree. We measure how these four educational groups fare in terms of labor market outcomes. Across most outcomes such as earnings, probability of a full-time job and tenure length, those with vocational training are the closest to the college educated in terms of early career experiences.

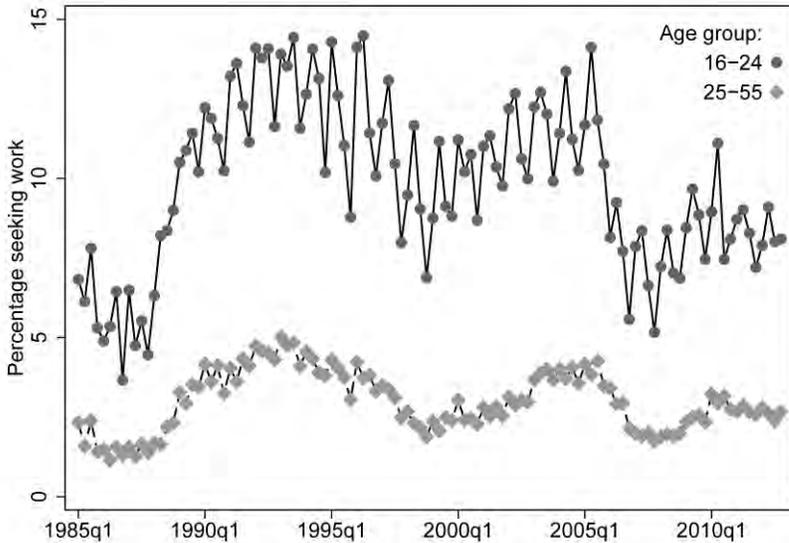
Keywords: youth unemployment, vocational education, business cycles.

JEL classification numbers: E24, E32, I21.

* We are grateful for comments by Matz Dahlberg on a previous version. In addition to administrative data, some of the data used are taken from Statistics Norway's Labor Force Survey 1985-2012 (all quarters). These data have been made available by the Norwegian Social Science Data Services (NSD). Neither Statistics Norway nor NSD are responsible for the analysis or the interpretations made in this paper. ** Department of Economics, NHH Norwegian School of Economics, Kai.Liu@nhh.no. *** Department of Economics, NHH Norwegian School of Economics, Kjell.Salvanes@nhh.no. **** Department of Economics, NHH Norwegian School of Economics, Erik.Sorensen@nhh.no.

In the OECD, 26 million 15 to 24 year olds are not in work, education or training and are seeking a job (OECD, 2013). This is an increase of 30 percent from 1997, and many countries in Southern Europe have youth unemployment rates of 50 percent or more (*The Economist*, 2013). A concern among policy makers is that the joblessness at the start of the working careers will have long lasting effects on job opportunities and lifetime earnings. Indeed, increasing evidence suggests that the condition of the labor market at the time of entering the labor market may have negative short- and long-term effects on labor market outcomes. For high-skilled workers with a college degree, long-term negative effects have been documented for many countries such as the US, Canada, Germany and also the Nordic countries (Oyer, 2006; Oreopoulos et al., 2012; Kahn, 2010; Liu et al., 2012). For low skilled workers, the focus has been more on the direct short- and long-term effects on long-term employment of being unemployed early on in the career (Burgess et al., 2003; Raaum and Røed, 2006; Gardecki and Neumark, 1998).

In this paper, we ask the question to what degree education is insulating against negative short- and long-term effects of entering the labor market in a recession as opposed to in a boom. We are interested in the degree to which education length per se is important: high school drop outs vs high school graduates vs. those with college education. But we are also interested in to what degree the type of education is important. Recently, there has been a great deal of attention on vocational education since in many countries with high youth unemployment rates, this track in high school has been underdeveloped (*The Economist*, 2013) and we especially focus on to what degree those starting out with vocational skills fare better than those with an academic high school degree and how they compare to the college educated. We analyze the short- and long-term effect of initial labor market conditions for high school drop outs, high school graduates with a vocational degree, high school graduates with an academic degree and the college educated. We use Norwegian employer-employee data and assess the direct impact and persistence on earnings and unemployment, as well as on certain aspects of the quality of the jobs, such as the proportion of full-time jobs and job tenure of entering the labor market in recessions. In addition, we also provide some descriptive evidence of job mobility to get a better understanding of why differences across skill groups exist.

Figure 1. Proportion of the workforce seeking work

Source: Statistics Norway (2003).

Note: Share seeking work (y-axis) vs time (x-axis). Own calculations based on Labor Force Surveys.

We are mainly using the largest downturn in the Norwegian economy after World War II, the early 1990's recession, when youth unemployment tripled over a period of a couple of years and stayed high for almost 15 years before it dropped to a lower level (but not back to pre-1990 levels). The development of unemployment rates for youth and core labor force can be seen from Figure 1. The level of youth unemployment throughout this period is lower than what is currently experienced in Southern Europe, but it is the steep increase in the youth unemployment rate that we are exploiting in this paper. Note also that the official numbers are slightly misleading, since they do not count those in active labor market programs.¹ In 1993, the year when the number of people on programs peaked, on average 8 percent of the youth aged 16-24 and 2 per-

¹ The labor force survey did not have questions covering various employment and training programs before a redesign of the questionnaire in 1996. Bø (1999) explains that before 1996, such program participants would typically be counted as employed (if in employment programs) or out of the labor force (if they were in training or qualification programs).

cent of those aged 25-55 were enrolled in programs in addition to those counted as seeking work in Figure 1.²

Different mechanisms may explain why the immediate negative effect of entering the labor market when it is thin may persist, and why the impact and persistence may differ across different parts of the distribution of education. A bad labor market at the time of entry will increase the probability of being unemployed and will affect labor market experience, and hence human capital accumulation (Gibbons and Waldman, 2006). A special version of this reasoning has been used in the empirical literature focusing on the fact that low-skilled workers have a much higher probability of experiencing unemployment. In the “scarring” literature, the effect of being unemployed has a negative effect in itself through mechanisms such as loss of human capital while unemployed, or just the negative experience of being unemployed (Burgess et al., 2003; Nilsen and Reiso, 2013). Search theory provides another perspective that predicts differences in catching up after a negative shock in the availability of well paid jobs, job quality, or good matches (Topel and Ward, 1992; Manning, 2000). Since it is well known that search activity and job-to-job mobility is an important part of early lifecycle careers, one would expect that the degree of search activity may explain the persistence of a negative business cycle shock when starting out. It is also well documented that more skilled workers move more across labor markets, for instance since their skills are more fit for a national labor market, and this search activity may explain potential differences in persistence.

For our interest in how education might dampen the impact of graduating in a recession, we divide the population into four skill categories: drop-outs from high school, high school graduates from the academic and vocational track, as well as workers with at least some college education (but not advanced post graduate degrees). Instead of assessing the effect of being unemployed, we estimate how these four groups fare in terms of earnings, the probability of being unemployed and the probability of a full-time job. We then assess two other aspects of job outcomes which have been focused on in a complementary literature – job tenure and job mobility (see Neumark et al., 1999; Jaeger and Stevens, 1999; Burgess

² Own calculations based on numbers from Norwegian Labor and Welfare Organization (NAV), <http://www.nav.no/Om+NAV/Tall+og+analyse/Arbeidsmarked/Annen+arbeidsmarkedsstatistikk/Historisk+statistikk>.

and Rees, 1998; Bratberg et al., 2010). Although the previous literature on this has focused on whether there has been a trend in these outcomes over time, we also focus on the cyclical pattern and persistence.

Our empirical approach does not account for compositional changes in final education caused by the business cycle. There are reasons to suspect that such compositional changes would not have any large impacts on the composition of our groups given the business cycle variations in our period. Betts and McFarland (1995) found that an increase in unemployment led to increased enrollment at community colleges. However, their estimates taken together with Norwegian business cycle fluctuations would not radically impact the composition of our groups; they estimate a 4 percent increase in enrollment (not percentage points) per percentage point of adult unemployment. Duncan (1965) suggested that dropping out of high school would be more tempting in good times when alternatives to school are good. Rees and Mocan (1997) found this effect to be quantitatively small (about half the effect size that Betts and McFarland (1995) found on college enrollment). As to the choice between academic and vocational high school, not a great deal is known about how economic circumstances affect this choice. But at the college level, it is known that the choice of major is not very sensitive to economic circumstances (Arcidiacono, 2004; Beffy et al., 2012). We conjecture that the effect on the analogous choice between academic and vocational high school is also small.

The rest of the paper proceeds as follows: In the next section, we present data and variable definitions. The results follow, first in Section 2 where we look at experience profiles of our outcomes, and then in Section 3 where we quantify the relation between initial labor market conditions and the experience profiles. In Section 4 we make some concluding remarks.

1. Data and sample selection

The data on workers used in our study are derived from administrative registers and prepared for research by Statistics Norway. The data cover all Norwegian residents aged 16-74 old in the years 1986-2010. We have information about employment relationships, labor income, educational

attainment, field of education and date of completion, labor market status, and a set of demographic variables such as gender, age, experience and marital status. A unique person identifier allows us to follow workers over time. Likewise, each worker is matched to a firm allowing us to identify each worker's employer.

The sample used in our main analysis is constructed by first identifying the cohorts graduating between 1986 and 2002. We use data from 1986 to 2008 such that we can follow them for some years after graduation and confirm that we see the end of their educational career in the data. For the remainder of the paper, education will be taken as the maximum level of education within these years. We focus on the first ten years following graduation from college.

Table 1. Relative cohort sizes (fractions) by education group

	1986-1990		1991-1995		1996-2002	
	Male	Female	Male	Female	Male	Female
High School dropout	0.257	0.270	0.164	0.164	0.179	0.125
Academic high school	0.090	0.211	0.076	0.168	0.090	0.126
Vocational high school	0.281	0.107	0.290	0.120	0.286	0.169
College	0.373	0.411	0.470	0.548	0.444	0.581

Source: Own calculations.

Note: Relative sizes of the cohort/education groups identified by the sample selection criteria outlined in Section 1.

The main focus of the study is to compare four groups of workers, 1) those entering the labor market from mandatory school only (we call these “dropouts”), 2) those entering the labor market with a high school degree from the academic track, 3) those entering the labor market with a completed vocational high school and 4) those with at least some college education (not necessarily a completed academic degree, but excluding those that take advanced post graduate educations). We know the date of graduation and for the first three groups, we restrict the sample to those aged 16-24 when they enter the labor market. This excludes mature students who return to school either to finish degrees or start a new career. For the college group, we require that the age at the year of graduation is between 20 and 29. Table 1 reports the relative sizes of the education groups over the period. We note that the share of the high school dropout group is decreasing over time and, as expected, the share of the cohorts taking some college education is increasing. The most striking gender

difference is how women are distributed more intensely on academic rather than vocational high school. This is not because fewer women go on to higher education: In Norway, as in many other countries, there are more women earning at least some college education, by a margin that has been increasing for some time.

The outcome variables we look at are (the log of) real earnings deflated by the Consumer Price Index.³ The probability of unemployment is defined as having had a registered unemployment spell during the year or belonging to one of the types of labor market programs (employment programs, qualification programs and back to work programs). This measure is not the same as that in Figure 1, since it cumulates events over the year, whereas those in Figure 1 record the stock at a certain point in time. A full-time job is defined as a job requiring more than 30 hours of work per week. Job tenure is defined as the maximum length (in years) of employment spells at the same plant with a starting date at or before the year of observation. We focus on job tenures up to 2000, since there would be a right-censoring of job spells toward the last few years of the sample. Job mobility is defined as a transition between jobs or between employment and unemployment.

We exploit one major and one smaller downturn plus two upturns taking place in Norway in the observational window. Returning to national unemployment, as reported in Figure 1, the severe bust that took place in Norway at the beginning of the observational window, starting in 1988/89 is particularly interesting for this analysis. The downturn lasted until 1993 when unemployment started to decrease. This is the deepest and longest lasting downturn in Norway since WWII. National unemployment rose from 1.5 percent to 5.5 percent which is a historically large unemployment rate in Norway.⁴ Following this recession, growth and employed picked up, and a boom took place around 1998 where the unemployment rate was down to 2.4 percent of the labor force. The recovery flattened out in 1998, but lasted until 2001. In 2001-2003, there was a mild recession before a new and strong expansion started.

³ Our measure of earnings is the sum of pre-tax market income (from wages and self-employment) and work-related cash transfers, such as unemployment benefits, sickness benefits and parental leave benefits.

⁴ The national unemployment rates from the Norwegian Labor and Welfare Organization (NAV).

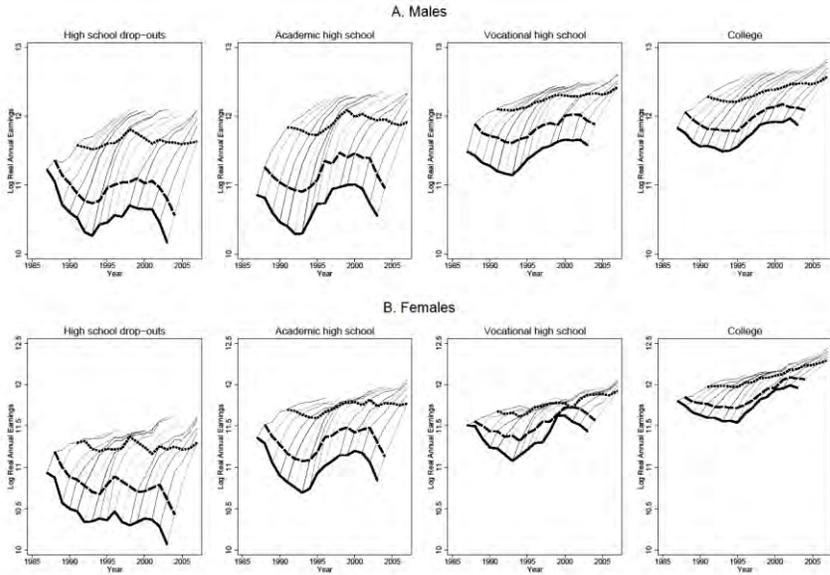
2. Results: The effects of graduating in a recession across gender and skill groups

We look at the experience profiles of our outcome measures over the early career for those graduating between 1986 and 2002, where by “experience” we mean potential years of work experience after leaving the final education. For each cohort of graduates, we draw how outcomes change over time. We add draws through the first, the second, and the fifth year after graduation. This is done separately for skill groups and genders.

2.1 Effects on earnings of entering the labor market in recession

In Figure 2, we plot the experience profiles of log annual real earnings by cohorts for the four skill groups from dropouts to those with some college education, for men and women, respectively. Focusing on men first, we notice that there are large variations in earnings across cohorts for all skill groups and that these are strongly correlated with the business cycle. We see that in first year earnings, there are clear differences in the impact of the business cycle across skill groups, where the drop-outs and those with an academic high school have a much stronger variation over the business cycle than men with vocational training or some college education. In fact, those with academic high school look much more similar to the dropouts than to those with vocational training. In order to better visualize the persistence of starting out in good and bad times, we indicate the persistence of the pattern by connecting the earnings profiles for cohorts one, two and five years after graduation. After two years of experience, we see that the patterns persist for all groups. However, after five years, there is very little left of the negative income shock for starting to work in a recession for all education groups, even though the drop-outs and the academic high school cohorts seem to get a larger boost from the late 1990’s boom. The initial differences gradually fade out and earnings across cohorts converge in a little over five years for all groups.

Figure 2. Annual (log) earnings by experience



Source: Own calculations.

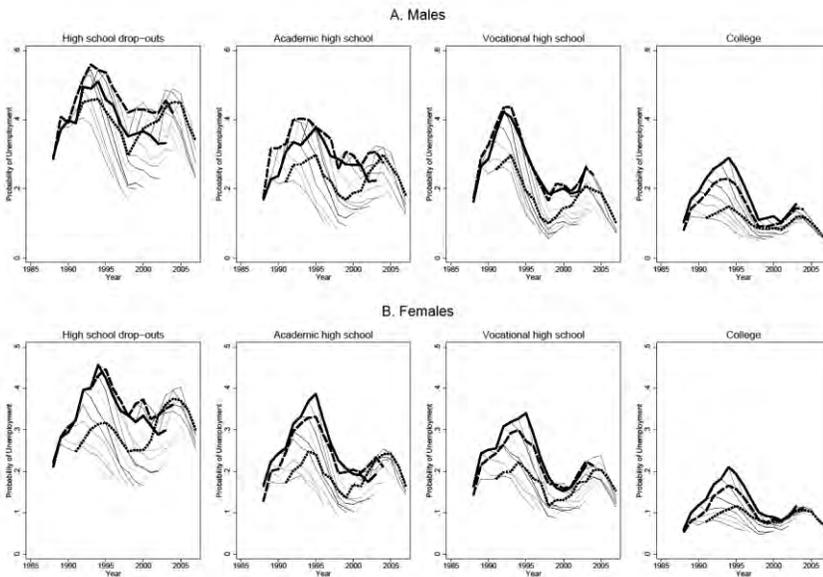
Note: For each group and cohort, mean (log) earnings (y-axis) are drawn against time (x-axis), starting the year after graduation. Our measure of earnings is the sum of pre-tax market income (from wages and self-employment) and work-related cash transfers, such as unemployment benefits, sickness benefits and parental leave benefits, deflated by the Consumer Price Index. The thick solid lines indicate the means one year after graduation, the thick long-dashed lines two years after graduation, and the dotted lines five years after graduation.

For women, we see a very similar pattern on the earnings from graduating in a recession; however, with some interesting differences as compared to men. The levels are lower, not so much because the starting levels are lower, but because the experience profiles are not as steep as for men. Another interesting difference is that women who dropped out from high school appear to have a quite strong negative trend in start earnings in the whole period at which we are looking. And whereas the male dropouts seem to have benefited from the late 1990s boom, this does not seem to be the case for women. After five years of experience, the earnings levels are smoother, similar to those for males, but at a lower level – and with five years of experience, dropouts and those with academic high school of both genders seem to have a very small increase in real earnings, unlike those with vocational high school and those with some college, who have increasing real earnings over the period.

2.2 Effects on unemployment of entering the labor market in recession

The large fluctuations in earnings we saw in Figure 2 do not only (or even primarily) reflect differences in wages, but also differences in hours worked. Unemployment is an important reason for such differences in hours worked. Figure 3 provides the unemployment pattern and persistence across skill groups and gender. We see large differences both in the immediate effect on unemployment across skills, and in the impact for those who have reached five years of experience. This fact is not particularly surprising since wage compression and the importance of unions in wage negotiations in Norway are very high, leaving less room for effects on wages and more adjustment on quantities such as unemployment (Kahn, 1998).

Figure 3. Unemployment by experience



Source: Own calculations.

Note: For each group and cohort, the fractions with at least one recorded spell in unemployment or labor market programs (y-axis) are drawn against time (x-axis), starting the year after graduation. The thick solid lines indicate the means one year after graduation, the thick long-dashed lines two years after graduation, and the dotted lines five years after graduation. Our unemployment data series is not available before 1988, which is why one cohort is first observed in the second year after graduation, such that the thick solid and long-dashed lines start in the same year.

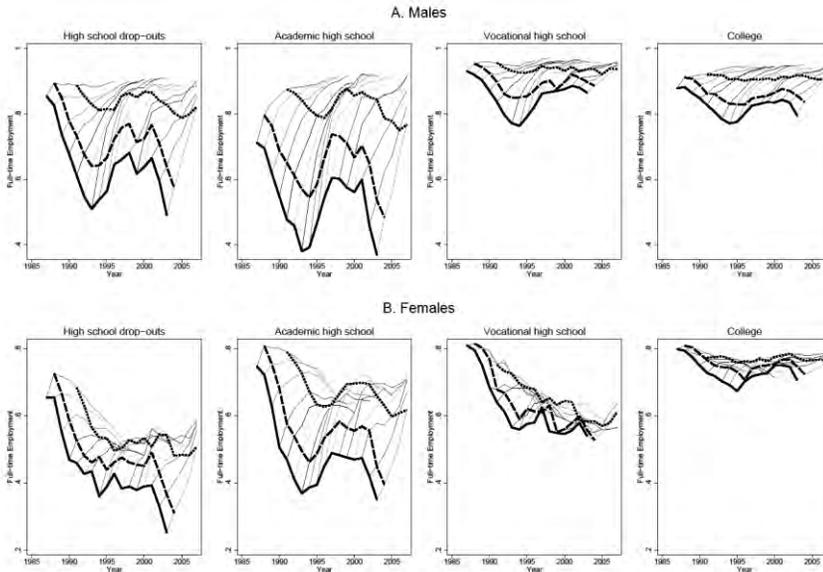
The skill group with some college education is almost insulated from being unemployed, except in the very early career, while the three other

skill groups on average experience quite big differences in unemployment rates depending on the labor market conditions when entering. More remarkable is that the effects do not seem to dampen to any considerable extent with experience and especially for high school dropouts, the effects even seem to become stronger over time; notice the large impact of the mid 2000's boom on high school drop-outs of both genders. The results resemble the results for low-skill groups in the "scarring" literature (for Norwegian evidence along these lines, see Raaum and Røed (2006) and Nilsen and Reiso (2013)). For women, we in general see lower effects on unemployment of starting out in a bad year which is most likely due to the fact that a larger number of women have jobs in the public sector such as in education and health. Other than this, we see a very similar pattern as for men.

2.3 Effects on the fraction with a full-time job of entering the labor market in recession

Figure 4 shows the fraction with a full-time job. If we consider this as in some sense an aspect of the quality of the job, we see that all skill groups of men have a very high probability of getting a full-time job almost directly out of school. However, for all skill groups, this varies over the business cycle, and dramatically much more so for dropouts and those with academic high school. Thin labor markets provide less of an opportunity for a full-time job, and again it is very clear that the level of education insulates graduates from a bad shock.

For women we have, as expected, a lower probability of a full-time job than for men since we know that women work more part time. Notice also what seems to be a secular downward trend for all women except the group with some college education. Also for this outcome does it seem that women are a little less affected than men and the persistence seems to disappear after five years, with the exception for those with a vocational training. However, there appears to be a noticeable secular trend in having a full-time job for the three lowest skill groups, while there is no comparable trend among those with some college education.

Figure 4. Proportion with full-time job by experience

Source: Own calculations.

Note: For each group and cohort, the proportions with full-time jobs, meaning at least 30 hrs/week (y-axis), are drawn against time (x-axis), starting the year after graduation. The thick solid lines indicate the means one year after graduation, the thick long-dashed lines two years after graduation, and the dotted lines five years after graduation.

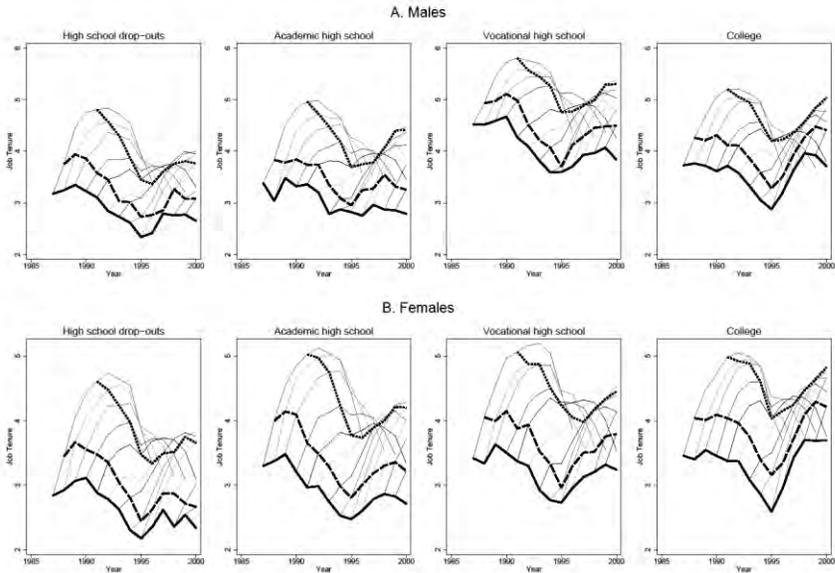
2.4 Effects on mean job tenure of entering the labor market in a recession

Whether graduates are able to match to a good job is another aspect of labor market performance. We expect tenures at good jobs to be longer, but the business cycle can have two opposite effects on mean tenure: bad times might make it necessary to accept worse jobs with less of an opportunity for advancement, which would give an incentive to leave such jobs early such that tenures would be short. On the other hand, we would expect search on the job to be more effective in good times, which could mean that the tenures would go up in bad times.

In Figure 5, we see that the mean job tenure is around four years for the first jobs in Norway and this is in line with what was found in Bratberg et al. (2010). We also see that for men, there is an inverse U-shape when split by skill levels, those with vocational high school actually realize the longest tenures early in the career. This is not true for women

where mean job tenure is much the same for everyone except dropouts, who have a smaller mean tenure. We all see that for both men and women, the pattern established for tenure in the first job is also very evident in the pattern at five years of experience.

Figure 5. Mean of maximum tenures by experience



Source: Own calculations.

Note: For each group and cohort, the mean maximum tenures of the job that has been started at a point in time (y-axis) are drawn against time (x-axis), starting the year after graduation. The thick solid lines indicate the means one year after graduation, the thick long-dashed lines two years after graduation and the thick dotted lines five years after graduation.

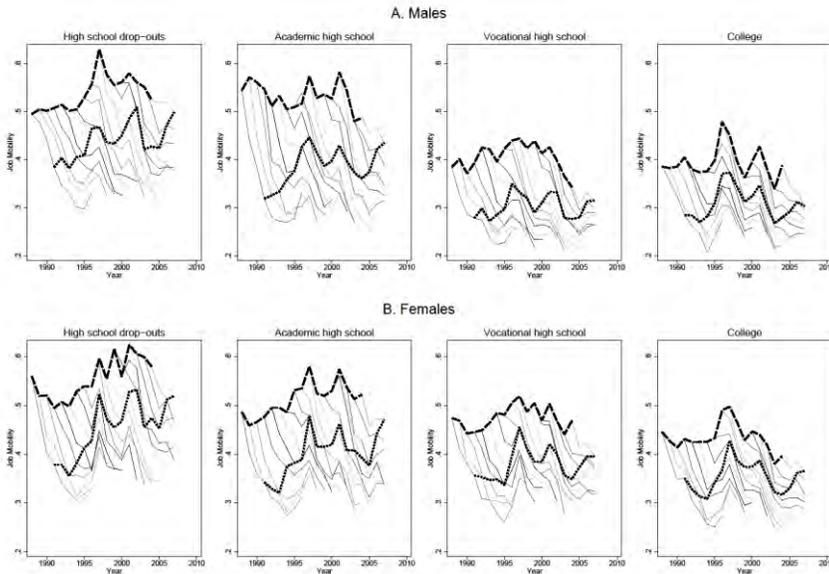
2.5 Effects on job mobility of entering the labor market in a recession

One possible explanation for why mean tenures are so stable could be that there was very little job mobility, such that tenure at the first job in effect corresponded to tenure over the whole period considered. This is not the case, as can clearly be seen from Figure 6. Job mobility is high from the outset and, of course, declining over the experience profile, as graduates settle into jobs that fit them.

It does not seem that job mobility is as sensitive to business cycle variations as some of our other measures, although it seems to increase for

everyone in the good times of the late 1990's, coming out of the early 1990's recession. For both men and women, this effect seems most pronounced for high-school dropouts. But it is also clear that while experience lowers the level of job mobility, it does not seem to dampen the impact of what business cycle variation there is – the short-dashed lines are at least as volatile as the long-dashed ones.

Figure 6. Job mobility by experience



Source: Own calculations.

Note: For each group and cohort, the mean fractions of the population changing labor market status, between jobs or between employment and unemployment (y-axis), are drawn against time (x-axis), starting two years after graduation. The thick solid lines indicate the means one year after graduation. The thick long-dashed lines two years after graduation, and the dotted lines five years after graduation.

3. Measuring the short- and long-term effects of initial labor market conditions

In the introduction, we looked at the aggregate business cycle patterns in Norway over the period (Figure 1), and in Section 2 we examined the experience profiles of five different outcomes. In this section, we would like to connect these two parts of the picture, relating the experience pro-

file of outcomes to the state of the labor market at the time of graduation. We want to do so using the first-order patterns in the data we have already presented, since what we want to shed some light on is the effect of national business cycles as youth outcomes. Previous work that only uses within-region variation in business cycles (such as Oreopoulos et al., 2012 and Liu et al., 2012) restrict the attention to variation in data that is arguably closer to true exogeneous variation. This comes at the cost of less directly addressing the issue of national business cycles such as those we see in Figure 1. For this study, we have chosen in favor of descriptive relevance.

3.1 *The framework*

In line with much previous work, we approximate initial labor market conditions using the unemployment rate at the time of graduation. We follow Oreopoulos et al. (2012) and Liu et al. (2012) and estimate the outcome for graduating cohort c at time t as

$$y_{ct} = \beta_1 + \beta_2^e U_c + \gamma_e + \delta_1 c + \delta_2 c^2 + \varepsilon_{ct}, \tag{1}$$

where γ_e is a fixed effect for the year of potential labor market experience ($e = t - c$) and δ_1 and δ_2 make up a quadratic time trend. U_c , is the national unemployment rate measured at the time of graduation for each cohort. The coefficients on the initial unemployment rate, β_2^e , are allowed to vary with the levels of potential experiences. If U_c were to represent exogenous variation in labor demand, the β_2^e estimated using ordinary least squares would capture the causal effect of initial labor market conditions. As pointed out by Oreopoulos et al. (2012), β_2^e estimate the average change by experience level, given the regular evolution of the unemployment rate faced in the future.

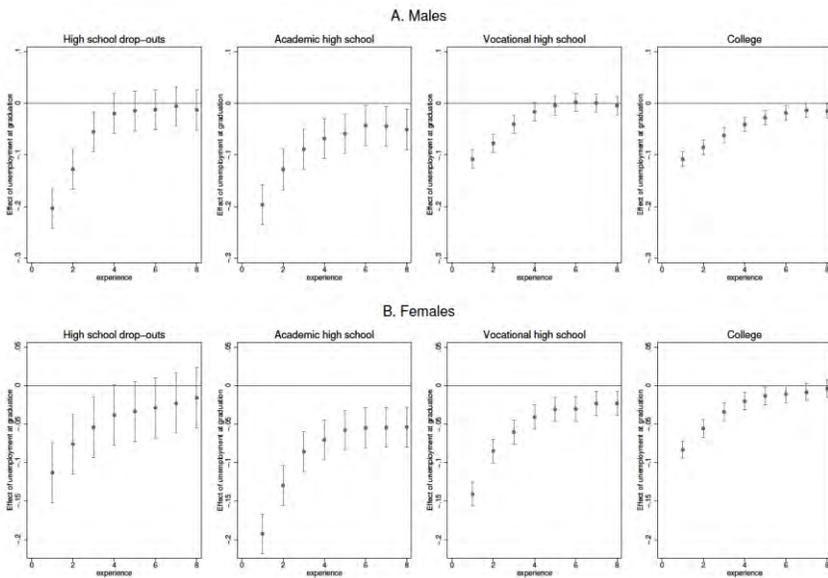
In order to estimate the model, we first cluster our panel data into cells defined by cohort and calendar year. By not including indicators for each region, we are only using between cohort-time variation in unemployment and outcomes, which wins us some precision and directly relates the variation in the experience-profiles in Section 2 to the unemployment history we saw in Figure 1. On the other hand, it might introduce some

biases – see Liu et al. (2012) for an approach that restricts identification to within time and region variation.

3.2 Results on persistence

Unemployment at the time of graduation has a negative effect on earnings, not only in the first year (that we have looked at), but also for a number of periods, as seen in Figure 7, but with the exception of those with academic high school, most of the effect seems to have tapered off four years after graduation.

Figure 7. Persistence of initial unemployment in annual (log) earnings



Source: Own calculations.

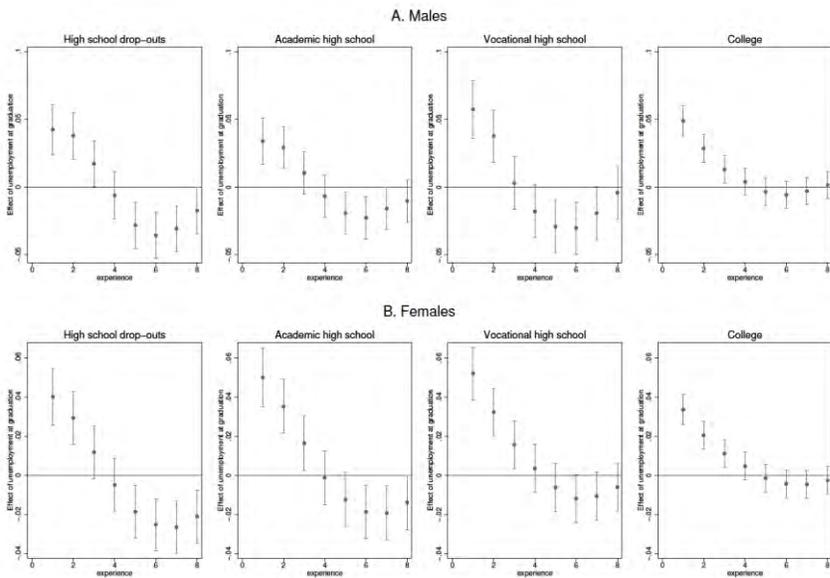
Note: We plot point estimates and a 95 percent confidence interval for the β_2^e coefficient in equation 1, where initial unemployment is measured in percentage points (y-axis), against years of potential experience (x-axis).

For unemployment, we also see dramatic effects (Figure 8). One percentage point in the economy-wide unemployment rate at graduation is

associated with a 4 to 5 percentage point increase in the unemployment of graduates, although this effect also tapers off in five years.⁵

The chance of getting a full-time job is seriously damaged by graduating in a recession, as can be seen in Figure 9. For high school drop-outs and those with academic high school only, the effect seems to persist a long time. This is very different for those with vocational high school and college, for whom the effects are initially smaller, and for whom they are very small four years later.

Figure 8. Persistence of initial unemployment on unemployment



Source: Own calculations.

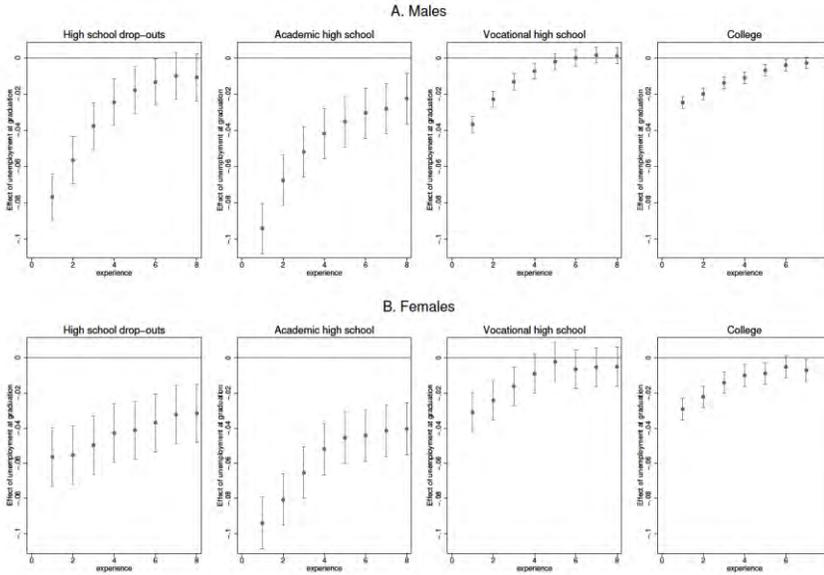
Note: We plot point estimates and a 95 percent confidence interval for the β_2^e coefficient in equation 1, where initial unemployment is measured in percentage points (y-axis), against years of potential experience (x-axis).

For job mobility, we see that the initial effects are negative, but they are also pretty small (Figure 11). The effects of one percentage point of unemployment only have marginal effects as compared to the large levels that can be seen in Figure 6. The effect turns positive after a few years,

⁵ For the non-college groups it might even seem that after five years, the effect of initial employment seems to be a good thing. We believe that this is an artifact of the relatively short time period in the data.

probably reflecting that workers tend to leave a job earlier when they have initially been matched to a worse job. This is confirmed by looking at the picture for mean maximum tenures (Figure 10). The effect of initial unemployment on maximum tenure is negative for a long time, indicating that some workers are able to find their way to other jobs.

Figure 9. Persistence of initial unemployment on proportion with full-time job

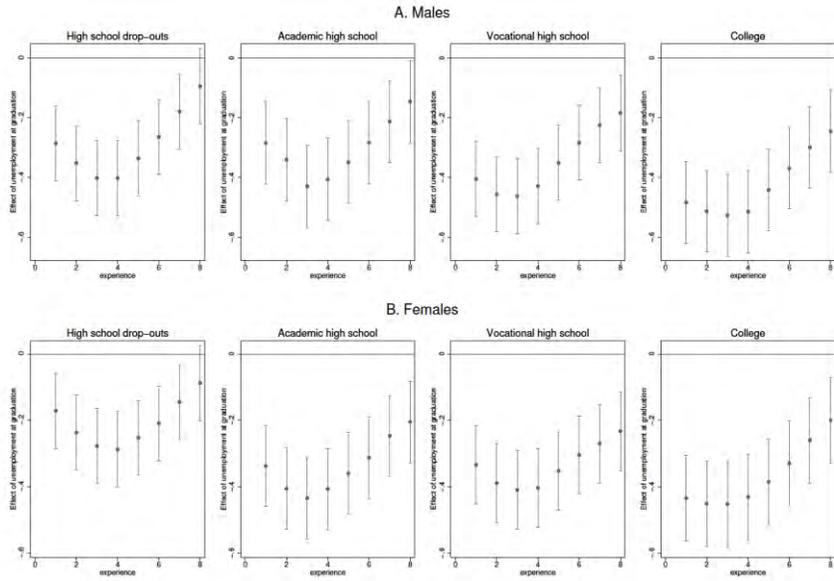


Source: Own calculations.

Note: We plot point estimates and a 95 percent confidence interval for the β_2^c coefficient in equation 1, where initial unemployment is measured in percentage points (y-axis), against years of potential experience (x-axis).

Summarizing Figures 7-11, we do not want to make too much out of different degrees of persistence for the educational groups we look at; in many of the cases, the estimated precision does not support this. But we do want to emphasize that many of these adverse effects of initial labor market experience seem quite persistent.

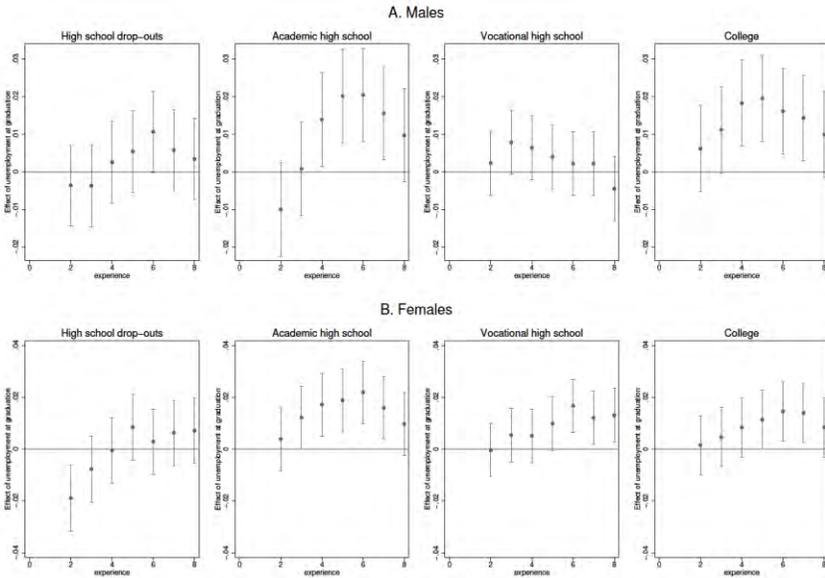
Figure 10. Persistence of initial unemployment on maximum tenure



Source: Own calculations.

Note: We plot point estimates and a 95 percent confidence interval for the β_2^E coefficient in equation 1, where initial unemployment is measured in percentage points (y-axis), against years of potential experience (x-axis).

Figure 11. Persistence of initial unemployment on job mobility



Source: Own calculations.

Note: We plot point estimates and a 95 percent confidence interval for the β_2^c coefficient in equation 1, where initial unemployment is measured in percentage points (y-axis), against years of potential experience (x-axis).

4. Concluding remarks

In this paper, we study the effect on different skill groups entering the labor market in good times as opposed to in bad times. We are interested in to what degree education may insulate short- and long-term negative labor market effects of starting out in a recession. Our results address the intuition that education is not uni-dimensional, as expressed by *The Economist* in a recent article assessing youth unemployment rates: “What matters is not the number of years of education people get, but the content”, and they continue “But it is unwise to conclude that governments should simply continue with the established policy of boosting the number of people who graduate from university” (*The Economist*, 2013). In this paper, we have assessed the outcomes for those starting out in a job without a completed high school degree (dropouts), with (only) an aca-

demically high school degree and those with a college degree. We assess the effects separately for men and women, and measure how these four groups fare in terms of earnings, the probability of being unemployed, the probability of a full-time job, job tenure and the job mobility by graduation cohort. We also examine the persistence of the shocks for the different groups and show how education may insulate against business cycle shocks.

We find that both the number of years of education and the type of education divide the cohorts into groups that fare very differently in times of recession. High school drop outs perform worse along all dimensions and the college educated outperform all other groups. It is probably reasonable to assume that a college education is not an alternative for high school drop outs for different reasons, so a more reasonable comparison is high school drop outs to those with vocational training and to those with an academic high school degree only when entering the labor market. The question is whether helping these workers to do a vocational training program, for instance by providing more and better vocational training programs, can be beneficial. Those with a vocational high school degree outperform both those with an academic high school degree and dropouts. In fact, those with vocational training seem much more similar to the college educated in terms of outcomes with one important exception: they appear to be as little isolated from becoming unemployed (persistently so) as the other high school graduates, but they do better than the high school dropouts. In sum, both the length and the type of education are important predictors of a good and smooth transition from school to work.

References

- Arcidiacono, P. (2004), Ability sorting and the returns to college major, *Journal of Econometrics* 121, 343-375.
- Beffy, M., Fougère, D. and Arnaud, M. (2012), Choosing the field of study in post-secondary education: Do expected earnings matter?, *Review of Economics and Statistics* 94, 334-347.
- Betts, J.R. and McFarland, L.L. (1995), Safe port in a storm: The impact of labor market conditions on community college enrollments, *Journal of Human Resources* 30, 741-765.

- Bratberg, E., Salvanes, K.G. and Vaage, K. (2010), Has job stability decreased? Population data from a small open economy, *Scandinavian Journal of Economics* 112, 163-183.
- Burgess, S., Propper, C., Rees, H. and Shearer, A. (2003), The class of 1981: The effects of early career unemployment on subsequent unemployment experiences, *Labour Economics* 10, 291-309.
- Burgess, S. and Rees, H. (1998), A disaggregate analysis of the evolution of job tenure in Britain, 1975-1993, *British Journal of Industrial Relations* 36, 629-655.
- Bø, T.P. (1999), Klassifisering av registrerte arbeidsledige og personer på tiltak i arbeidskraftundersøkelsen (AKU), Statistics Norway, Notat 99/31.
- Duncan, B. (1965), Dropouts and the unemployed, *Journal of Political Economy* 73, 121-134.
- Gardecki, R. and Neumark, D. (1998), Order from chaos? The effects of early labor market experiences on adult labor market outcomes, *Industrial and Labor Relations Review* 51, 299-322.
- Gibbons, R. and Waldman, M. (2006), Enriching a theory of wage and promotion dynamics inside firms, *Journal of Labor Economics* 24, 59-108.
- Jaeger, D.A. and Stevens, A.H. (1999), Is job stability in the United States falling? Reconciling trends in the current population survey and panel study of income dynamics, *Journal of Labor Economics* 17, S1-28.
- Kahn, L.M. (1998), Against the wind: Bargaining recentralisation and wage inequality in Norway 1987-91, *Economic Journal* 108, 603-645.
- Kahn, L.B. (2010), The long-term labor market consequences of graduating from college in a bad economy, *Labour Economics* 17, 303-316.
- Liu, K., Salvanes, K.G. and Sørensen, E.Ø. (2012), Good skills in bad times: Cyclical skill mismatch and the long-term effects of graduating in a recession, Discussion Paper 16/2012, Department of Economics, NHH Norwegian School of Economics.
- Manning, A. (2000), Movin' on up: Interpreting the earnings-experience profile, *Bulletin of Economic Research* 52, 261-295.
- Neumark, D., Polsky, D. and Hansen, D. (1999), Has job stability declined yet? New evidence for the 1990s, *Journal of Labor Economics* 17, S29-64.
- Nilsen, Ø.A. and Holm Reiso, K. (2013), Scarring effects of early-career unemployment, *Nordic Economic Policy Review*, this issue.
- OECD (2013), *Education at a Glance 2013*, OECD Publishing, Paris.
- Oreopoulos, P., von Wachter, T. and Heisz, A. (2012), The short- and long-term career effects of graduating in a recession, *American Economic Journal: Applied Economics* 4, 1-29.
- Oyer, P. (2006), Initial labor market conditions and long-term outcomes for economists, *Journal of Economic Perspectives* 20, 143-160.
- Raaum, O. and Røed, K. (2006), Do business cycle conditions at the time of labor market entry affect future employment prospects?, *Review of Economics and Statistics* 88, 193-210.
- Rees, D.I. and Mocan, H.N. (1997), Labor market conditions and the high school dropout rate: Evidence from New York State, *Economics of Education Review* 16, 103-109.
- Statistics Norway (2003), *Labour Force Survey 2001*, Official Statistics of Norway C748, Statistics Norway, Oslo.

The Economist (2013), Youth unemployment: Generation jobless, The Economist 407, April 27 2013, 49-52.

Topel, R.H. and Ward, M.P. (1992), Job mobility and the careers of young men, Quarterly Journal of Economics 107, 439-479.

Comment on Liu, Salvanes and Sørensen: Bad times at a tender age – How education dampens the impact of graduating in a recession

Matz Dahlberg^{*}

The overall aim of the paper is to examine how graduation from school at different stages of the business cycle is related to future labor market outcomes. In particular, the authors are interested in examining if some educational degrees insure better against bad labor market outcomes when graduating in a recession than do other degrees. Is, for example, a young high-school drop-out worse off in the short- as well as in the long-run by graduating in a recession as compared to another young person graduating at the same time but with a completed high-school degree or some college education? This and related questions examined in the paper are all interesting and important and the answer to them might provide important input to the policy-making process.

Using rich Norwegian micro data, the authors present several interesting relationships between the time of graduation (over the business cycle), years of schooling (those without a completed high school degree, labeled “high school dropouts” in the paper, those with a vocational high school degree, those with an academic high school degree and those with some college education) and future labor market outcomes (defined by labor earnings, unemployment, full time job, job tenure and job mobility). The main conclusion reached by the authors is that education isolates from bad shocks. This is especially true for those with some college edu-

^{*} IBF and Department of Economics, Uppsala University, matz.dahlberg@ibf.uu.se.

cation, less true for those with a vocational high school degree and least true for high school dropouts and those with an academic high school degree.

My comments on the paper will be divided into two parts. The first part will deal with methodological questions and the second part will ask some policy-related questions.

1. Methodological questions

From a methodological point of view, I will raise three issues: The selection of individuals into and out of education at different points in time and how that affects a causal interpretation of the results, the measure of the unemployment shock and a couple of things on statistical significance.

First, from a policy-making perspective, it is important to know whether the estimates in the paper shall mainly be seen as representing causal relationships or if they shall mainly be seen as representing correlations. Given the policy focus of the journal, it can be worth discussing potential threats to a causal interpretation (and hence, to the ability to provide policy advice from the results).

When examining what the short- and longer-run effects are of leaving school with different types of educational lengths and different types of educational degrees at different stages of the business cycle, an important assumption for a causal interpretation is that the business cycle in itself does not affect the individuals' choices of educational length and educational degrees. There are, however, several papers indicating that the individuals' educational choices are a function of the business cycle. For example, using US data, Betts and McFarland (1995) find a strong positive relationship between the local unemployment rate and community college enrollments; when the local unemployment rate increases, so does community college enrollment. Along the same line, also using US data, Rees and Mocan (1997) find a negative relationship between the unemployment rate and the number of high school dropouts, that is, the worse the labor market conditions are, the fewer choose to drop-out from school. Examining what are the effects of the local youth labor market on enrollment rates in England, Clark (2011) finds large impacts; the larger is the youth unemployment rate, the larger is the enrollment in post-

compulsory education. This indicates that the allocation of individuals into different educational programs at different points in time is not random.

The non-random selection of individuals into different educational degrees makes the interpretation of the cohort-profile figures in the paper somewhat hard to interpret, both within and between degrees. Are, e.g., high school dropouts in a bust similar to high school dropouts in a boom? Assume that it is those with the highest ability or who are the highest strivers among the potential high-school dropouts that choose not to drop out in a bust, but who would have dropped out in a boom, then this selection will affect the future labor market outcomes of those who drop out from high-school in a recession to look relatively worse. The same type of selection process will also affect the comparison between individuals with different educational degrees.

More generally, but in a similar vein, it would be interesting to get a sense of how much of the observed differences in the cohort-profile figures that is due to educational length per se, and how much that can be explained by selection of individuals with different characteristics, like ability and whether they are strivers, into different educational degrees. For example, to what extent would those with some college education do equally well on the labor market even without their college education?

The relationships presented in the paper are interesting both if they are mainly to be interpreted as correlations and if they are mainly to be interpreted as causal effects, but from a policy perspective it would have been interesting, and important, if the paper could have provided some more guidance on this issue.

Second, I would like to raise a question related to the measure of unemployment shock used in the regressions. In the paper, the national unemployment rate is used, assuming that everybody meets the same shock in a given year. However, there are large regional variations in the unemployment rate, implying that individuals graduating with the same degree in different regions in a given year might meet very different shocks. It was not clear to me why the authors did not (also) use the regional unemployment rate.

My last methodological point relates to the statistical significance of the regression estimates presented in the figures. First, in the figures, 95 percent confidence intervals are presented within each educational degree

(indicating whether the estimates are significantly different from zero for each time point and educational degree). However, when it comes to the comparison over educational degrees, it is less clear what the interpretation should be. Are the estimates for the different educational degrees at each experience time point significantly different from each other? Second, the unexpected results that in the long run it seems good to graduate in a recession for non-college groups (significant positive effects after five years) is explained away by the authors as being an artifact of the relatively short time period in the data. But if this explanation holds for these unexpected results, why does it not also apply to all other (more expected) results?

2. Policy-related questions

Given that we have obtained a causal relationship between the state of the economy at graduation time and future labor market outcomes for individuals with different educational degrees, policy-makers would be interested in at least two questions. The first question is related to the magnitudes of the effects. Are they large enough to care about from a policy perspective? For example, how large a part of the youth unemployment rate at a specific point in time can be explained by the business cycle at the individuals' graduation time? How large are the direct (contemporaneous) effects? How large are the indirect (lagged) effects? And how important are these effects as compared to other explanations for the youth unemployment rate?

If the effects are large enough to be concerned about from a policy-perspective, the next question is which policy conclusions that can be drawn. To me, this seems like a very hard question to answer. A natural answer would perhaps be to instigate different types of labor market programs for individuals with different educational degrees in recessions, but which type of programs should that be? And for whom? And given that we can come up with ideas for suitable programs (or some other policies), there is still the question about the timing of the policy (which holds for all types of policies that are to be a function of the business cycle). Should it be instigated before, during or after a recession? And are we able to properly identify (the beginning of) a recession?

Finally, policy-makers in different countries would be interested in getting an idea about how general the results are. Can they, e.g., be generalized to the other Nordic countries? To other time periods? Can the results be reconciled with similar research from the other Nordic countries (e.g. the long-run scarring effects for those with Swedish vocational high school degrees as estimated in Nordström Skans, 2011)?

References

- Betts, J.R. and McFarland, L.L. (1995), Safe port in a storm: The impact of labor market conditions on community college enrollments, *Journal of Human Resources* 30, 741-765.
- Clark, D. (2011), Do recessions keep students in school? The impact of youth unemployment on enrolment in post-compulsory education in England, *Economica* 78, 523-545.
- Nordström Skans, O. (2011), Scarring effects of the first labor market experience, IZA Discussion Paper 5565, Bonn.
- Rees, D.I. and Mocan, H.N. (1997), Labor market conditions and the high school dropout rate: Evidence from New York State, *Economics of Education Review* 16, 103-109.

Networks and youth labor market entry*

Lena Hensvik** and Oskar Nordström Skans***

Summary

This paper provides an overview of existing knowledge regarding the role played by social networks in the process where young workers are matched to employing firms. We discuss standard theories of why social networks may be an important element in the job-matching process and survey the empirical literature on labor market networks with an emphasis on studies pertaining to the role of social contacts during the school-to-work transition phase. In addition, we present some novel evidence on how contacts established while working during the final year in high school affect youth labor market entry. Finally, we discuss how insights from this literature can be used to improve the quality of social programs targeted towards young workers in the Nordic countries.

Keywords: referrals, school-to-work transition, youth unemployment.

JEL classification numbers: M51, J64, J24, Z13.

* Any direct or indirect policy advice expressed in this article reflects the views and opinions of the authors and not those of IFAU. We thank Max Bohlin Roberts for excellent research assistance and Daniel le Maire and an anonymous referee for useful comments.

** Institute for Evaluation of Labour Market and Education Policy (IFAU), Uppsala University and CESifo, lena.hensvik@ifau.uu.se.

*** IFAU, Uppsala University and IZA, oskar.nordstrom_skans@ifau.uu.se.

The successfulness of young workers' transition from school to working life is influenced by a number of factors, including the human capital provided by the schools, the beliefs and motivation of the young and the conditions on the relevant labor market. However, a defining feature in this process is that the intrinsic uncertainty of the recruiting process which motivates key economic concepts such as statistical discrimination, search frictions and the signaling value of education is likely to be particularly pronounced when employers evaluate previously untested workers. A salient feature of most labor markets is that employers try to mitigate this uncertainty by relying on the direct personal interactions between workers and agents of the firms that are provided by social networks. In this article, we discuss the role played by networks for the labor market entry of young workers.

Recently, the economic literature has seen a huge flow of theoretical and empirical papers discerning the role played by labor market networks (see Kramarz and Skans, 2013 for references). An obvious reason for this growing interest is the prevailing fact that labor market networks appear to be at least as important for the matching of workers and jobs as are all formal recruitment channels combined. In this article, we summarize some of this evidence, although we are by no means claiming to provide a full account of all the relevant literature. We focus on studies that we believe to be relevant for understanding the role played by networks when young workers transit from school to work. We start off by documenting the prevalence of networks as an important mechanism for the matching process, with a focus on the situation in Sweden. Next, we briefly describe the different rationales for the use of labor market networks that have been emphasized in the economic literature and discuss what we can learn from recent empirical studies on the relevance of these competing theories. We end our overview of the existing literature by surveying the evidence regarding how social contacts affect the job finding rate and the ensuing match quality with an emphasis on studies that refer to the situation for young workers.

Then, we turn to an empirical investigation of how the contacts established during summer or extra jobs held by students that are enrolled at Swedish vocational schools affect their labor market entry. By focusing on the role of job contacts obtained through in-school work experience, we complement the picture emerging from a closely related previous

study on Swedish data which focuses on the role of family links (Kramarz and Skans, 2013).

The results confirm that social networks are indeed important, both in determining which particular establishments students sort into after high school and the time it takes to find a stable job. The estimated magnitudes are non-trivial: graduates who had a summer/extra job at a particular establishment have a 35 percentage-point higher probability of finding a stable job there as compared to other students from the same class; and they are estimated to have around a four percentage point higher probability of ending up in an establishment to which someone from the summer/extra job has moved. The interpretation in terms of networks is supported by the fact that workers who move just before the summer jobs are found to be unrelated to the ensuing post-graduation employment patterns of the graduates. In addition, we find that the employment rate of graduates is estimated to increase by 14 percentage points if all high school job contacts were employed relative to a case where none of the contacts were employed. Consistently, the estimates are found to be substantially larger if the contacts are specialized in the same occupation as the graduating student.

In the final section of the paper, we summarize the evidence presented in the article and try to spell out how the results can guide policy makers in the design of social programs for young workers.

1. Networks are a key element in the job matching process

A vast number of international studies suggest that job finding networks are a key element in the process where workers and firms are matched. The literature is surveyed by e.g. Ioannides and Loury (2004) and Bewley (1999), reporting that most studies that try to document the fraction of job-matches that are formed with the help of social networks and informal contacts find results in the range of 30 to 60 percent. Essentially, the original studies of interest come in one of two forms; the first is based on surveys to workers who are asked how they found their last job, and the second is based on surveys to firms that are asked how they filled their

last vacancy.¹ However, it is noteworthy that, although uniformly showing that networks are important, the results tend to differ between studies in exactly *how* important. One potential reason for this is that survey questionnaires may phrase their questions differently (e.g. “how did you find your job?”, or “how did you first learn about your job?”). Another obvious reason is that the surveyed populations may differ. In general, youths and low educated appear to be overrepresented among those using social contacts, see e.g. Pelizzari (2004).

The picture presented by Ioannides and Loury, and by Bewley, is also well matched by Swedish survey evidence. Data from the Swedish Labor Force Survey presented in Behtoui (2008) show that around 40 percent of the jobs were found through informal contacts and that this share is higher among the young, the less educated, men and when unemployment is high. In addition, informal contacts appear to be used less frequently among immigrants. Similarly, the Public Employment Service (PES) regularly surveys previously unemployed workers who have found jobs, asking them how they found information about these jobs. The results in Nilsson (2011) for the period 2006-2010 show that around 70 percent received information through some form of informal channel (slightly more for the younger), whereof about half was through a friend or acquaintance. An additional source of information is provided by the Swedish National Board for Youth Affairs, which regularly surveys Swedish youths regarding how they got their current job. Results from the years 2004 to 2011 suggest that a fairly stable 70 percent of employed 19 to 25-year olds found employment through informal contacts, whereof somewhat more than half through people they knew (the other half from direct contact with an employer).

A second strand of studies has instead surveyed the demand side of the labor market; asking employers how they find suitable employees. Here, we have once more found a number of Swedish studies, all of which present results from specific surveys to employers. Examples include Klingvall (1998), Behrenz (2001), Nutek (2000), Ekström (2001), Svenskt Näringsliv (2010) and Riksrevisionen (2010). The results suggest that around 60 to 70 percent of the employers rely on informal recruit-

¹ A third set of studies focuses on documenting which channels the unemployed are using in their attempts to find employment, but since these are less informative regarding the question under study here, we have not included them in this review.

ment channels, where suggestions from incumbent employees provide one important component.²

On top of the survey evidence, the international literature contains a number of ambitious attempts to identify the causal impact of social contacts on the probability that workers are matched to a specific establishment or firm. In contrast to the survey based studies, these studies use a control group to identify the probability that a particular worker should have ended up at the same establishment, had the contact not been present. For example, Bayer et al. (2008) show that individuals are more likely to work with very close neighbors (contacts) than with somewhat more distant neighbors (control group) in the US. Similarly, Kramarz and Skans (2013) show that Swedish graduates are much more likely to find their first stable job where their parents (contacts) work than where their classmates' parents (control group) work. These differences also remain when they compare graduates with parents who are employed within the same firm, but at different establishments; a result that implies that parents have to be present in an establishment for the effect to be potent. A key insight from these studies is that it is very difficult to predict to which specific establishment a worker will be matched without access to network data. In other words, if someone ends up in an establishment where he or she has a contact, it is usually because of the network and not because of some other correlated factor.

Overall, our reading of the existing evidence is that it consistently shows that networks are very important factors in the job matching process. Our review of Swedish evidence suggests a rough decomposition into one third of the jobs found through formal channels (responding to formally posted vacancies), one third through direct contact with employers, and one third through social networks. This picture is fairly well-mirrored by firm-side responses on how vacancies are filled. Overall, both Swedish and international evidence also seems to suggest that networks are more important for the young and less-educated.

² The main outlier is Behrenz (2001) who found a lower fraction for informal contacts. A possible reason is that the sample was drawn from firms with posted vacancies at the PES.

2. Why are networks so important?

Given that a host of different types of studies consistently shows that social networks provide an important factor in the worker-firm matching process, in particular for the young, a natural next question is “why?”. Below, we provide an overview of the key theoretical explanations and discuss some existing empirical evidence which may shed some light on why social networks act as a crucial intermediary in modern labor markets.

There is an enormous amount of studies pertaining to the use of job finding networks within both sociology and economics, and it is not within the scope of this article to provide a full account of this literature. However, following Jackson (2010), we believe that it is fair to distinguish between two broad motivations for the use of social networks in the job matching process.

The first of these explanations focuses on the role played by networks in diffusing information about available jobs among potential job seekers. We can think of this explanation as being *supply-side oriented*, since it focuses on the behavior of job seekers, whereas firms feature as passive agents. The seminal paper in this tradition is Granovetter (1973) which emphasizes the *Strength of weak ties*, a hypothesis suggesting that contacts that are further away from an individual convey more novel information (about job vacancies) and that weak social ties are therefore crucial for the job finding process. Recently, works by several economists have explored various aspects of how supply-oriented networks could be incorporated into standard models of matching on the labor market (see e.g. Calvo-Armengol and Jackson, 2004). According to this strand of the literature, access to networks matters for labor market outcomes since it provides information about additional job openings. In terms of different types of networks, it is clear that it is most useful for agents to have access to networks that are as dispersed as possible since that provides the broadest possible coverage of information about possible vacancies. Since the focus in this tradition is mainly on the diffusion of information about vacancies as such, there is very little emphasis on aspects related to information about the quality of jobs and (in particular) information about the characteristics of individual workers. Therefore, in general, it does not

appear to be particularly important what type of social tie one has to the employed agents.³

The other main explanation focuses on firm-side rationales and could thus be viewed as *demand-side oriented*. A key insight motivating this focus is that firms appear to be fully aware of, and actively promoting, the use of social contacts in the matching process. But why do they find it beneficial to do so? Recent research argues that a probable reason is that social networks allow firms to find the types of workers they want to hire. In other words, the social tie may serve as an intermediary reducing ex ante uncertainty that workers and firms may experience when trying to evaluate the opposing agent. One version of these models emphasizes that social networks serve as a direct means of reducing the uncertainty regarding how well-suited the capabilities of a certain worker are for a particular firm by providing credible information in both directions (see e.g. Simon and Warner, 1992).

In contrast to the supply-side models, a key element arising from many models in the demand focused literature is that the quality of the demand-side agents matters. A prominent example is Montgomery (1991) who argues that firms like to hire friends of *productive* workers under the assumption that friends resemble each other so that recruited friends are also likely to be more productive than other workers. Similarly, a recent randomized field experiment by Beaman and Magruder (2012) shows that productive workers are more able to refer other productive workers and that they will do so if firms let the referring (incumbent) worker gain from the productivity of their referred entrants. Such incentives also appear to shift the types of networks used from family ties to professional ties. Importantly, the (perceived) productivity of those belonging to a young worker's network becomes a key determinant for the usefulness of the network, if it is true that referrals are primarily asked from workers who are believed to be productive by their employers.

Casella and Hanaki (2006) provide a very interesting extension to Montgomery's (1991) referral model which sheds some light on the resilience of the use of job-finding networks by young workers. In short, they

³ An interesting early exception is Boorman (1975) which provides an interesting model where employed agents first provide information about job openings to their closest friends (if these are unemployed) and only provide information to acquaintances if all close friends are employed, suggesting that strong social ties may be particularly important when unemployment is high.

postulate that young workers without experience can invest in costly formal merits or in contacts with older workers who may generate referral offers. They show that if firms can obtain referrals at no (or low) cost, they will always choose to hire through personal referrals rather than via formal merits, even when the formal signals are more informative about the workers' productivity. The reason is that while formal merits transmit information to the entire market, personal referrals transmit *private* information to the establishments that are connected to the worker, allowing them to extract some of the surplus from the referral hires. The private information is in this way a source of monopsonistic power, which redistributes more of the surplus from the match to the firms.

There are good reasons to believe that the demand-side models are particularly relevant for young workers who are entering the labor market. The reason is that employers may find it particularly difficult to identify the individual abilities of workers who are at the beginning of their careers. Evidence corresponding to this line of argument shows that firms overprice formal merits (such as schooling) and that they underprice abilities that are difficult to screen early in workers' careers (Altonji and Pierret, 2001 and Hensvik and Skans, 2013 for a replication using Swedish data).

Although, as always within social sciences, it seems obvious that different proposed explanations all contain elements of truth, a host of recent evidence suggests to us that information about worker heterogeneity is a key element motivating the frequent use of job-finding networks. An interesting recent contribution is Brown et al. (2012) who study explicit data on referrals from a large single firm in the US. They find that employees who receive referrals from older workers, more tenured workers or workers in higher ranks have the highest initial starting salaries. Earlier work by Fernandez et al. (2000) and Castilla (2005) on data from a US customer support center suggests that referrals and referrers have correlated characteristics and that referrals are more productive than other entrants early in their careers. Dustmann et al. (2011) who use ethnic similarity as a proxy for social ties also show evidence of wage growth and mobility patterns that are consistent with a model where networks reduce the ex ante uncertainty regarding the quality of matching between workers and jobs. Finally, Hensvik and Skans (2013), who rely on Swedish data, show that workers who enter via co-worker networks have

better test scores (that are difficult for employers to observe) but lower schooling (which is easy to observe), suggesting that networks provide employers with information about abilities that are difficult to observe otherwise. In addition, they find that abilities are correlated between linked incumbents and entrants along the lines suggested by both Montgomery (1991) and Beaman and Magruder (2012).

It also seems to matter empirically who the demand side agent is. Kramarz and Skans (2013) and Hensvik and Skans (2013) both find that networks to a larger extent lead to employment when the demand-side agent (the employed) is of a better quality. More precisely, Kramarz and Skans find that parents with higher wages and longer elapsed tenure matter more – also when comparing different parents within the same firm. As shown by Hensvik and Skans (2013), firms are more likely to recruit former co-workers of incumbent workers when the incumbent workers have higher test scores.

A key result found in Kramarz and Skans (2013) is also that the impact of networks requires that the demand side contacts are present at the actual establishment. Once demand side contacts have left the establishment, or if these contacts are employed by another establishment within the same firm, the effect is essentially zero.

Overall, we take this evidence as supporting the notion that considerations regarding aspects related to worker heterogeneity provide an important element which motivates the widespread use of networks in the matching process. Ultimately, this is a very important insight, in particular when thinking about networks in a policy context, since it implies that not all social contacts matter alike. In order to make a difference, networks should carry useful information to employers about worker characteristics.

3. Networks and labor market consequences for the young

3.1 Time-to-job

A key aspect in terms of the consequences of job finding networks is to what extent access to useful networks reduces the time it takes for young workers to find employment. Conceptually, networks could matter for

where young workers work without affecting the time to employment. The issue can be studied in two ways, either by analyzing the relationship between measures of network quality and the duration until employment, or by studying differences in the elapsed time to job between youths that found their job through a social contact and those that found employment through other means. Essentially, the first strategy is more appealing, but also more demanding, and we have not found any studies in that vein that pertain to youths in particular (see our analysis below, though).

More generally, Bayer et al. (2008) try to identify the impact of network quality on labor market outcomes by studying the impact of close neighbors, while using somewhat more distant neighbors as a control group. They show that workers whose close neighbors, in particular neighbors with similar demographic characteristics, are employed have higher employment rates themselves. Similarly, Cingano and Rosolia (2012) show that displaced workers, who lose their jobs during a mass-layoff, have shorter ensuing spells of unemployment if the employment rate within their network of former coworkers is higher.⁴ None of these studies pertain specifically to youths, however.

A second vein of studies documents the relationship between the time-to-employment and the means by which these jobs were found. Here, we would like to emphasize two studies which focus on young workers. The first is Bentolila et al. (2010) who use survey data from the US and 13 EU countries (including Denmark and Finland but not Sweden). They show that youths who found their jobs through social contacts had shorter preceding unemployment spells, even after accounting for fairly detailed individual and firm-level characteristics. The second study is Kramarz and Skans (2013) who use Swedish register data, focusing on the role of family ties. Consistent with Bentolila et al. (2010), they find that youths who found employment through family ties did so half a year faster than other youths. This remarkably large effect is present regardless of whether the comparison is made relative to other graduates from the same class or relative to other graduates entering the same firm. Even when estimated relative to other graduates from the same school and field entering the same firm, an effect of one quarter of a year remains.

⁴ We have also, within an ongoing project, replicated this finding on Swedish data, finding comparable results.

Thus, overall, we take the existing evidence as suggesting that networks do not only provide a quantitatively important job finding channel but also that a stronger network in general tends to increase employment and re-employment among displaced workers. Most importantly, youths who (are able to) use social contacts to find employment appear to substantially reduce their exposure to joblessness after graduation.

3.2 Match quality

A second important aspect is to what extent the use of networks affects the match quality. Most studies trying to measure this effect have relied on wages as a measure of match quality, and our reading of the literature is that the results are enormously dispersed. A number of well-executed studies have found negative wage effects, but at least as many studies found positive wage estimates. One possible reason for this diverging evidence is that the population varies across studies, which complicates the overall picture if there is heterogeneity in the usefulness of networks across settings.

In line with the latter interpretation, a number of studies have found diverging wage estimates depending on the type of contacts that have been used (e.g. Brown et al., 2012 and Hensvik and Skans, 2013). Consistent with the demand-side oriented models, [Loury \(2006\)](#) argues that contacts must be able to convey relevant information to employers about the potential worker in order to generate wage effects, suggesting that the effects of networks may depend on e.g. the degree to which they overlap with the field of education.

The most relevant studies for the purpose of this article are, again, [Bentolila et al. \(2010\)](#) and [Kramarz and Skans \(2013\)](#) since they both focus on youths. In fact, both these studies suggest that the impact on initial wages is negative. In the case of [Kramarz and Skans](#), wages are, in particular, lower compared to other entrants within the same firms. Interestingly, however, [Kramarz and Skans](#) also find that wage growth and ensuing tenure within the plant are substantially higher among those that enter through parental contacts, which suggests that the employing firms reward these workers over time. In effect, the wages after three years are at par with comparable youths who enter through other means.

In our view, this last result also shows that wages provide an incomplete measure of match quality. A more direct measure of the impact on the quality of recruited workers is provided by Hensvik and Skans (2013) who show that firms that recruit via (co-worker) networks in general are able to recruit workers with better unobserved abilities as compared to the average recruited worker entering the same firms. Although this analysis only looks at match quality from the firm perspective (finding good workers) and not from the worker perspective (finding a good job), it illustrates that networks can provide a tool for firms to increase match quality, and that this can be studied directly, given appropriate data. Notably, the mirror image of this picture can be inferred from the results presented in Kramarz and Skans (2013): They find that firms which hire children of employees tend to, on average, pay higher entry wages (to all entrants). These firms are also, on average, more profitable and more productive. Thus, workers who enter the labor market through family networks appear to be matched to “better” firms, just as employers who rely on co-worker networks appear to be matched to better workers.

4. Labor market contacts and the school-to-work transition

So far, this text has reviewed existing evidence regarding job search networks in general, with an emphasis on studies that are relevant for the school-to-work transition in a Nordic context. Our reading of this evidence is that the recent literature has provided a wide set of informative results, but that many things remain undocumented.

In terms of entering workers’ networks, we conjecture that they mainly appear in three forms: family contacts, contacts established during summer jobs or extra jobs while in school, and contacts established through social programs such as activities within schools or public employment services. Kramarz and Skans (2013) provide a very detailed analysis of parental contacts using Swedish data. To complement that analysis, this section provides parallel evidence regarding the role of contacts obtained through summer/extra jobs while in school. In the next section, we provide a discussion regarding the possibility of generating useful networks through the design of social programs.

The analysis in this section is divided into three parts. First, we briefly describe the empirical-set up and the data. Second, we analyze the probability that recent high-school graduates start working in the same plants as the contacts obtained through high-school jobs. Third, we examine how the employment rate of job contacts influences the job finding rate upon high-school graduation.

4.1 Empirical set-up and data

Our analysis in this section is focused on graduates from Swedish vocational high school programs (*Gymnasieskolans yrkesprogram*). These are three-year programs and students graduate at the age of 19. Roughly half of the Swedish high school graduates take part in the vocational (rather than “Academic”) programs which provide the main direct formal route to the labor market for young workers in Sweden. Each program is directed to a specific occupation, where examples include “construction work”, “health care”, and “auto mechanics”. The programs primarily provide class-room training but at least five weeks per year on average should be spent on site with actual employers. In contrast to e.g. Denmark, the Swedish system does not rely on apprenticeship programs to any substantial extent.⁵

High school graduates

For the analysis, we use a register based data set covering all 39 000 19-year old graduates from vocational high schools during the summer of 2006. For this population, we identify all jobs held by the graduates during the preceding year (2005). We identify all co-workers who are at least 20 years old (to avoid classmates) and think of these co-workers as contacts. To facilitate the interpretation, we also exclude contacts who are the graduate’s parents.

Our analysis will compare the job outcomes of “classmates”. Classmates are defined by the interaction of a school identifier and a field-of-study code. Although a school may have several classes within a field, so that our concept in a strict sense includes measurement errors, it is im-

⁵ However, since 2011 a minor fraction of students receive their vocational training through apprenticeship tracks.

portant to note that this is of minor importance since we only use the concept as a means to control for the types of skills the classmates receive as well as the local valuation of these skills.

Table B1 in Appendix B shows summary statistics for the most relevant variables in our analysis. 46 percent of the graduates are women and 6 percent have parents born outside Sweden (“immigrant background”). The average graduate finishes high school with 29 other classmates, suggesting that the combination of the school and the program is a fairly good measure of a class. The table also lists the ten largest programs in terms of the number of students, which illustrates the rather mixed selection of occupations under study.

Jobs before graduation and high-school job contacts

An important feature of the data is the full characterization of the jobs held by the graduates both *before* and *after* graduation. Table B1 in Appendix B shows that 68 percent of the graduates had some form of employment during the year before graduation. Unsurprisingly, the in school-jobs typically generate a very low income, the majority being either summer jobs which start in June or July and last 1-3 months, or evening/weekend jobs which start in January and last the full year. The table also suggests that the graduates who obtained some work experience during high school do, on average, have higher final grades.

Jobs after graduation

We measure the graduate’s post-graduation employment status in November 2006, i.e. five months after graduation. Since we are interested in high school job contacts and their role for the transition from school into the labor market upon graduation, it makes sense to focus on their first *stable* job, in particular in contrast to the jobs held when at school. Any definition of a stable job will be somewhat arbitrary, but we follow Krmarz and Skans (2013) and require that the job lasted for at least four months and generated a total income greater than three times the monthly minimum wage in order to be considered as “stable”.⁶ The purpose of this restriction is to define a level of employment that is substantially greater

⁶ The minimum wage is defined from the 10th percentile of the actual wage distribution.

than what is sustainable during the school years. Table 1 reports that 66 percent of the graduates received an income during the year of graduation whereas 31 percent had a stable job in November according to our definition. It should also be noted that the post-graduation employment rate is seven percentage points higher among graduates who held a summer or extra job during the last high school year as compared to the average graduate.

Table 1. Employment outcomes after graduation (2006)

	Fraction
All graduates:	
Positive income five months after graduation	0.66
Stable job five months after graduation	0.31
Graduates with summer/extra job in high school:	
Positive income five months after graduation	0.73
Stable job five months after graduation	0.38

Source: Own calculations based on data from Statistics Sweden.

Final datasets

The first stage of our analysis considers the propensity that a graduate finds his/her first stable job in the same plant as a high school job contact. For this analysis, we keep (i) all graduates who found a stable job at the end of 2006, (ii) the identity number of the graduate’s employer and (iii) the id number of all plants employing the job contacts at the end of 2006. In addition, we keep the combination of the school and vocational track (the “high school class”) for each graduate in order to use class-mates as a comparison group.

The second stage of the analysis examines the association between the employment rate in the network of high-school job contacts and the propensity that the graduate has found a stable job five months after graduation. For this analysis, we add the graduates who did not find employment in 2006. For each graduate, we also calculate the share of contacts employed in November 2006 and retain one observation per high-school graduate.

4.2 Sorting: High school job contacts and place of work

We start by examining whether graduates find their first stable employment in plants where they have high-school job contacts. The empirical strategy which closely mimics that of Kramarz and Skans (2013) is described in further detail in Section A.1 in Appendix A. But the intuition is fairly simple: We analyze whether graduates with a contact within a particular establishment are more likely to find a stable job there than his or her classmates are.

In order to think of the estimates as measuring the causal impact of social contacts, it is necessary to assume that the differences in how well graduates are matched towards different establishments are small within classes, or at least not too heavily correlated with the probability of having a summer job within the plant. This is not an innocuous assumption, in particular when analyzing the probability of returning to the same establishment as the summer or extra job, and it should be kept in mind when interpreting the estimates. However, we will also estimate the impact of finding stable employment at another establishment to which a former co-worker from a summer or extra job has moved. Although alternative explanations can be raised also in this case, we believe that those are less likely to be of any major concern. In addition, to validate that the effect captures the role of contacts, we later show that the graduate's propensity to be matched to the same establishments as someone who was employed in the summer/extra job establishment *prior to* (i.e. not at the same time as) the graduate is of a trivial magnitude.

The results reported in the first row of column 2 in Table 2 show that there is a positive and significant increase in the propensity to find a job in an establishment if a contact works there. On average, each contact increases the probability of starting working in a particular establishment by about one percentage point. This may not sound much, but as seen by column (1), the median graduate has contacts in 16 different establishments (the average is 49), so the estimated effect would imply a 16 percentage-point increase in the propensity to work with at least one contact (column 3).⁷ Here it could also be noted that we have estimated the same

⁷ We calculate this probability by following Bayer et al. (2008). They treat the likelihood of working in a plant with a contact as an independent event and calculate the aggregated effect as $0.158 = (1 - 0.00024)^{16} - [1 - (0.00024 + 0.0107)]^{16}$, where 16 is the median number of plants to which the graduate has at least one contact, 0.00024 is the baseline propensity to work in a particular establishment in the absence of a contact and 0.0107 is the estimated effect reported in column

model using a less stringent definition of stable employment, finding similar patterns.

The establishment or the contact?

In the lower panel of the table, we differentiate between the establishment where the graduate worked in high school and establishments to which a contact (former coworker) has moved between 2005 and 2006. The former case captures the propensity that an employer offers a stable job to a graduate who has work experience within the plant through a previous summer or extra job. Thus, this effect also includes mechanisms through the acquisition of firm-specific human capital. The second case measures the willingness of employers to hire young workers with a link to an employee when the graduate him/herself has not worked in the establishment. This latter effect could be thought of as a relatively “pure” network effect.

The results imply that establishments are much more likely to offer a stable job to graduates who worked there during summers or weekends in the final high school year. The fact that we find a positive estimate here is probably not particularly surprising, although the magnitude is quite striking: Graduates who worked at a particular establishment during the year preceding graduation are 35 percentage points more likely to find a stable job there than their classmates are (within the sample that finds employment).

The results derived from contacts that have changed employers imply that establishments are much more likely to offer a stable job to graduates who are linked through former co-workers even if the graduates have not been employed there in the past. While the point estimate here is much smaller in comparison (0.3 percentage points), the implied additional propensity for a graduate to end up in an establishment to which a contact has moved is about four percentage points using a similar calculation as in the above case. The reason is that most graduates have contacts that move into a fairly large number (15 for the median) of different establishments and the total effect is an aggregate across all of these estab-

(1). Since a few graduates have contacts with a very large number of establishments, we choose to base the calculation on the median in order to obtain a more accurate estimate of the importance of contacts.

lishments.⁸ An impact of four percent is around two thirds of the parental network effect for a similar population reported in Kramarz and Skans (2013).

Table 2. High school job contacts and place of work, baseline.

	(1)	(2)	(3)
		Average effect	Aggregated effect from the median number of contacts
	Median number of contacts	<i>All contacts</i>	<i>All contacts</i>
Total effect of contacts	16	0.0107*** (0.0005)	0.158
Summer/extra job establishment	1	0.3558*** (0.0076)	0.356
Establishments where the link is through contacts that have moved	15	0.0027*** (0.0001)	0.040
Class fixed effects		Yes	
Observations		6 175 541	

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the class (school×field) level reported in parentheses. Column (1) reports the median number of establishments where a graduate has at least one contact. Column (2) reports the average probability of working in the same establishment as *one of his/her contacts* (each column is from a separate regression) and Column (3) reports the probability of working with *at least one of his/her contacts*.

A robustness check: Evidence from placebo-contacts

A potential concern regarding the interpretation of our estimates is that the graduates and the contacts may have accumulated similar (and certain) types of skills in the summer/extra job establishment that are valued by certain types of employers. Another possibility would be that employers always prefer to hire workers from a particular establishment. Both of these stories could potentially have been generating the results presented above on observed patterns in our data. However, these (and similar)

⁸ Here it could also be noted that Kramarz and Skans (2013) find that the impact of low-tenured insiders is much smaller than the impact of workers who just entered the firm. By construction, we are identifying the effect from fresh entrants since we rely on contacts who moved during the preceding year.

explanations would predict a correlation in sorting patterns between graduates and contacts irrespective of whether they actually interacted in the summer/extra job establishment. In Table 3, we have therefore estimated the same model as before, but instead of looking at the role of actual contacts, we focus on a set of workers (or “placebo-contacts”) who left the summer/extra job establishment the year before the graduate was hired. Hence, these “placebo-contacts” had a nearly identical work history as the actual contacts (who, by definition, left one year after), but without interacting with the graduating student. The estimated effect, which is reported in column (2), suggests that the probability of ending up in the same establishment as a placebo-contact is very close to zero, which clearly highlights the importance of actual interaction in order to generate a contact effect. In our view, this is very strong support for our interpretation of the results

Table 3. Difference in the contact effect for true and placebo contacts

	(1)	(2)
	True contacts	Placebo contacts
Establishments where the link is through contacts that have moved	0.0027*** (0.0001)	-0.0003** (0.0001)
Class fixed effects	Yes	Yes
Observations	6 175 541	1 648 190

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the class (school×field) level reported in parentheses. Each column is obtained from a separate regression. The estimates and standard errors of the combined effects have been calculated using nlcom in Stata (see Appendix A for more details).

Contact relevance: Occupations and age

In column (1) of Table 4, we let the estimate differ depending on if the contact is specialized within the same (“relevant”), or a different (“not relevant”) field of study defined by his/her education.⁹ Notably, both types of contacts matter, but the estimated effect is substantially (three times on average) larger from contacts specialized in the same field as the

⁹ Since one graduate may have multiple contacts within the same establishment, we define an interaction term measuring the fraction of contacts within the establishment having the same field of education and report the inferred estimates when this fraction is projected at zero and one as the relevant versus irrelevant. Note that we do not require the same level of education, to allow for changes in the education system over time.

graduating student. This is an important insight since it shows that the occupational relevance of contacts reinforces the impact of the networks. This result is mirrored in Kramarz and Skans (2013) who find that the impact of parent-child links is larger when the parents share fields with graduating youths, although the effect is far from zero even in the cases where the fields are very different.

In the second column, we look at the differential impact depending on the age of the contact. Overall, it appears clear that older contacts matter more and this difference is clearly driven by the cases when the contacts have moved. One possible interpretation is that the reemployment probability in the original establishment is larger if the original firm uses younger staff. On the other hand, moving contacts may be more relevant if they are more experienced and therefore trusted to refer other workers into their new establishments.

Table 4. High school job contacts and place of work, by contact type

	(1) Effect by contact relevance:		(2) Effect by contact age:	
	Relevant contacts	Not rele- vant contacts	Old age>25	Young age<25
Total effect of contacts	0.0320*** (0.0021)	0.0092*** (0.0004)	0.0149*** (0.0007)	0.0044*** (0.0002)
Summer/extra job establishment	0.7470*** (0.0318)	0.2915*** (0.0080)	0.3343*** (0.0099)	0.4303*** (0.0243)
Establishments where the link is through contacts that have moved	0.0094*** (0.0008)	0.0023*** (0.0001)	0.0034*** (0.0002)	0.0015*** (0.0001)
Class fixed effects	Yes		Yes	
Observations	6 175 541		6 175 541	

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the class (school×field) level reported in parentheses. The two columns report the differential impact by the type of contact obtained from a pooled regression in each panel with interactions. The estimates and standard errors of the combined effects have been calculated using nlcom in Stata (see Appendix A for more details).

Are contacts more important for some graduates than for others?

Table 5 shows the differential contact effect depending on the graduate's final grades, gender and immigrant background. We focus on the importance of moving contacts, as this effect is more interesting from a

social network perspective. Overall, there are sizable effects for all graduates but the effect is slightly larger in the upper part of the final grade distribution. Consistent with much of the earlier literature, we also find the effect to be stronger for males. In addition, we find that the effect is larger for natives within our sample.

We have also estimated the effect for the ten most common vocational tracks, finding an economically and statistically significant effect of contacts in all segments of the labor market. However, the effects are more pronounced for graduates specializing in tracks directed towards the manufacturing and auto-industry, where the occupational relevance of contacts also appears to be particularly important. We report these estimates in Table B2.

Table 5. High school job contacts and place of work, by graduate type

	(1)	
	High school GPA	
	<i>Above median</i>	<i>Below median</i>
Establishments where the link is through contacts that have moved	0.0028*** (0.0002)	0.0026*** (0.0002)
	Gender	
	<i>Male</i>	<i>Female</i>
Establishments where the link is through contacts that have moved	0.0030*** (0.0002)	0.0024*** (0.0002)
	Immigrant background	
	<i>Native</i>	<i>Immigrant</i>
Establishments where the link is through contacts that have moved	0.0027*** (0.0001)	0.0022*** (0.0003)
Class fixed effects	Yes	
Observations	6 175 541	

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the class (*school×field*) level reported in parentheses. Each panel reports the differential impact by graduate type obtained from pooled regressions with the interactions between graduate type and the variable of interest. The estimates and standard errors of the combined effects have been calculated using *nlcom* in Stata (see Appendix A for more details).

4.3 Employment: High school job contacts and the job finding rate

The analysis presented so far shows that high school job contacts are an important predictor of the sorting patterns across establishments among

graduates from Swedish vocational high schools. Now, we turn to a second analysis aimed at shedding some light on how these contacts affect the job finding rate.

Previous work documents that a higher employment rate among former co-workers increases the probability of re-employment for recently displaced workers (Cingano and Rosalia, 2012). Here, we apply a similar estimation strategy in our analysis of high school graduates and examine if the probability of finding stable employment after graduation is associated with the employment rate in the network of high-school job contacts. As before, we restrict the comparison to individuals graduating from the same class in the same year using a class-fixed effects model. The inclusion of the fixed effects is important since we compare individuals entering the same occupation-specific local labor market at the same point in time and hence, account for common shocks affecting all these graduates. In addition, we control for the graduate's gender and immigrant background as well as the (log) size of the high-school job network.

As in the sorting equation above, a causal interpretation of this analysis hinges on an assumption regarding differences within classes. Here, the assumption is that the employment rate among previous coworkers (from summer or extra jobs) is uncorrelated with the unobserved ability to find employment conditional on the class-fixed effects and other covariates. To reduce the risk of remaining bias, we augment the specification with the graduate's income from the high-school job as well as the final grades. If the employment prospects are associated with unobserved characteristics that were also reflected in the previous income or the high school grades, these controls should reduce the correlation between employment outcomes and network characteristics. Reassuringly, the estimated effect of the contact employment rate is unaffected by the inclusion of controls for the grade percentile rank (which is shown to be significantly related to the outcome).

Table 6. High school job contacts and the job finding rate

	(1)	(2)	(3)
Graduates with work experience from 2005			
Contact employment rate	0.2219*** (0.0241)	0.1429*** (0.0239)	0.1426*** (0.0239)
Number of contacts (per one hundred)	-0.0027*** (0.0004)	-0.0032*** (0.0004)	-0.0032*** (0.0004)
Female	-0.0022 (0.0093)	-0.0066 (0.0090)	-0.0114 (0.0090)
Immigrant background	-0.0550*** (0.0154)	-0.0495*** (0.0149)	-0.0458*** (0.0150)
Log(lagged income)		0.0837*** (0.0028)	0.0828*** (0.0028)
Grade percentile rank			0.0473*** (0.0123)
Class fixed effects	Yes	Yes	Yes
Mean dependent variable	0.3785	0.3785	0.3785
Observations	26 460	26 457	26 457
R-squared	0.2004	0.2321	0.2326

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the class (school*field) level reported in parentheses.

The results, reported in Table 6 and Table 7, show that there is a positive association between the probability of stable employment in 2006 and the employment rate in the network of high-school job contacts. The magnitudes imply that the employment rate of graduates should increase by at least 14 percentage points if all contacts were employed relative to a case where none of the contacts were employed. A more relevant magnitude is calculated by using the “normal” variation in the employment rate provided by a one standard deviation increase in the network employment rate (15 percentage points) which would lead to an increased probability of employment among the graduates by around two percentage points.¹⁰ This is a fairly substantial effect. In comparison, it amounts to about half

¹⁰ For comparison, Cingano and Rosalia find a seven percent reduction in unemployment duration which corresponds to three weeks. They also estimate the effect on the probability of still being unemployed after 9/12/15 months. Their estimates suggest a 18/13/10 percentage point reduction from an increase in the network employment rate from 0 to 1.

of the impact of having an immigrant background or half the two standard deviations (half the full range) in the grade distribution.

Table 7. High school job contacts and the job finding rate, differential effects

	(1)	(2)
	Contact relevance:	
	<i>Relevant</i>	<i>Not relevant</i>
Contact employment rate	0.2722*** (0.0331)	0.1134*** (0.0241)
	Contact age:	
	<i>Old</i>	<i>Young</i>
Contact employment rate	0.1584*** (0.0241)	0.2658*** (0.0344)
	High school GPA	
	<i>Above median</i>	<i>Below median</i>
Contact employment rate	0.1363*** (0.0318)	0.1615*** (0.0352)
	Gender	
	<i>Male</i>	<i>Female</i>
Contact employment rate	0.1702*** (0.0358)	0.1217*** (0.0318)
	Immigrant background	
	<i>Native</i>	<i>Immigrant</i>
Contact employment rate	0.1371*** (0.0248)	0.2225** (0.0994)
Class fixed effects		yes
Mean dependent variable		0.3785
Observations		26 457

Source: Own calculations based on data from Statistics Sweden.

Notes *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors clustered at the class (school \times field) level reported in parentheses. Each column reports the differential impact by graduate type obtained from pooled regressions interactions between the contact employment rate and type. We include the same controls as in column (3) of Table 6 as well as the interaction between each of the controls and graduate type. The estimates and standard errors of the combined effects have been calculated using nlcom in Stata (see Appendix A for more details).

As in the above sorting models, the effect is substantially larger (about twice) when focusing on contacts with a relevant field of study as well as for male graduates (shown in Table 7). However, in contrast to the earlier results, where we found stronger sorting for graduates with high grades and native origin, here it appears that graduates with lower grades and immigrant background seem to benefit more from a higher employment share in the network. We also find that the employment rate among younger linked workers is a better predictor of the graduate's job finding rate.

4.4 Summary of the empirical results

In this section, we have provided new evidence on the role of job contacts established during high school for young workers' entry into the labor market. In sum, our results suggest that social networks are indeed important, both in determining which particular establishments students sort into after high school and the time it takes to find a stable job.

The magnitudes of these effects are non-trivial: graduates who had a summer/extra job at a particular establishment have a 35 percentage-point higher probability of finding a stable job there compared to other students from the same class; and they have a four percentage point higher probability of ending up in an establishment to which someone from the summer/extra job has moved. In addition, the employment rate of graduates is estimated to increase by at least 14 percentage points if all high school job contacts were employed relative to a case where none of the contacts were employed.

A consistent result is that the network effect appears to be substantially larger if the contacts are specialized in the same field as the graduating student.

5. Summary and policy discussion

In this section, we summarize the results reported in this article and discuss some tentative lessons for policy design. In this article, we have first reviewed existing studies analyzing the role of networks on the labor market. It is shown that, although the magnitudes tend to vary between studies, research consistently shows that informal recruitments channels in general, and social networks in particular, are quantitatively important for the matching of job seekers and firms. Our reading of existing evidence for Sweden, drawing on multiple sources on both the worker and firm side, suggests that about one third of the realized matches appears through *i)* formal channels, *ii)* direct applications and *iii)* social networks, respectively. In general, the informal channels also tend to be relatively more important for the young and low educated.

Then, we proceeded to review different potential explanations for why networks may play such an important role on the labor market. We primarily distinguished between two types of explanations, one supply-side

oriented and one demand-side oriented. The first explanation focuses on the role networks can play as a tool for spreading information about vacancies among job seekers and the other focuses on the employers' desires to find suitable workers. As noted in Section 3, we believe that a host of recent studies suggest that the supply-side perspective provides an incomplete description for the very widespread use of networks in the labor market and therefore, we will emphasize an interpretation based on demand-oriented models in the below discussion.

The emphasis on demand-oriented models of employee selection also links the study of networks to a broader research field focusing on uncertainty at the time of recruitment and the strategies firms use in order to overcome this uncertainty, a research field which has recently received increasing attention (see e.g. Oyer and Shaeffer, 2011). In particular, if firms have a strong aversion towards recruiting the wrong workers, it is likely that uncertainty at the time of recruitment will provide an obstacle for untested workers who are bound to rely on a scarce set of formal merits to signal their productivity. Such risk-aversion could be one potential explanation for the relatively modest uptake on some of the very generous employment subsidies that are available for Swedish employers.

Since demand-side explanations are based on arguments related to fundamental uncertainty about worker traits, there also are good reasons to suspect that these models are particularly relevant for young workers who are entering the labor market. Existing evidence suggests that employers do find it difficult to predict the individual productivity of young workers and instead make an inference regarding individual productivity relying on group characteristics. In an interesting recent Swedish field experiment, Eriksson and Rooth (2011) show that employers prefer not to interview young workers who are searching for a job from current unemployment. In addition, Altonji and Pierret (2001) show evidence suggesting that firms overprice formal merits (such as schooling) and that they underprice abilities that are difficult to screen early in workers' careers. This result has also been replicated by Hensvik and Skans (2013) using Swedish data.

The article also discusses evidence suggesting that networks do appear to be beneficial for youths, in the sense that they provide faster access to jobs among those that are able to use this job-finding channel. The evidence on job quality is fairly mixed, however, where young workers are

often found to receive lower wages when finding employment through networks (although the overall evidence is mixed), although wage growth and ensuing tenure suggest a good match quality. In addition, workers entering through family networks appear to enter high-wage firms that are both more productive and more profitable.

5.1 Policy discussion

Since this article is prepared for the *Nordic Economic Policy Review*, a natural next step is to discuss how the existing evidence maps into policy design. For this purpose, it is clear that we need to extrapolate to some extent from the settings of existing studies.

Much of our discussion regarding the empirics of labor market networks for youths has focused on lessons from the analysis of family ties provided by Kramarz and Skans (2013) and to this we add novel evidence on the role played by in-school work contacts arising from summer jobs or extra jobs during high school. We believe that it is likely that these two types of contacts are important parts of young workers' labor market relevant networks. But none of these networks are directly related to policy. Although Swedish municipalities often provide summer jobs that are directly targeted towards high school students, these tend to be provided in an artificial form which is unlikely to provide the youths with any particularly useful networks.

However, activities within a range of social programs have the ability to provide a venue for youths to establish contacts of various forms. Unfortunately, we are not aware of any systematic evidence regarding the effects of contacts provided through social programs. But a systematic finding in the program evaluation literature is that social programs that involve actual employers tend to be more successful, which indicates that the provision of contacts with employers may be a key element of success in the design of social programs.¹¹ In addition, comparative evaluations tend to suggest that program types that are set closer to employers provide faster access to jobs than those that are class-room based, see e.g. the

¹¹ Swedish examples of interventions which explicitly introduce employers' involvement include the evaluation of employer involvement in training programs (SWIT, see Johansson 2008), evaluations of employer-based programs for immigrants (SIN, see Åslund and Johansson, 2011) and employer-oriented job search assistance (Jobbnätet, see Liljeberg and Lundin, 2010).

survey by Forslund and Vikström (2010).¹² Finally, it is often suggested that apprenticeship based vocational training at the upper secondary level of schooling provides a more efficient route into employment than class room based training (see e.g. Lindahl, 2010).

Our reading of both our own results and results in the literature at large is therefore that they jointly suggest that the provision of platforms where youths can form useful labor market contacts may be a powerful tool to ease the transition from school to work. In addition, we believe that the evidence forcefully suggests that one important reason for why social networks appear to play such an important role for the job-matching process is that they convey information about worker traits to prospective employers. It is likely to be difficult to find good substitutes for this type of information transmission since, as noted by Casella and Hanaki (2006), networks provide privileged information at a low cost and firms may prefer privileged information to public information. In addition, it is evident that different firms may prefer to recruit workers with different types of specific traits (see Simon and Warner, 1992) and it may be difficult to transmit information about such traits through other means than social networks or direct contacts.

A first, and obvious, policy lesson from these results is that they suggest that it is useful to integrate meetings with potential future employers into the design of social programs targeted towards entering workers. There are also good reasons to believe that meetings between youths and prospective employers that last for a non-trivial amount of time are more useful. In particular, if we believe that contacts matter because they convey information to prospective employers about worker traits, then contacts are likely to matter more if they carry more information about these traits. This could either be because firms can make an inference through an assumption about similarity (as with relatives), because there is a trusted intermediary (such as a public employment service officer who is known by the employer) or because firms (or particular employees) have been able to assemble direct information from a work-related context. It is also noteworthy that results in Kramarz and Skans (2013), where strong tie networks (the family) are compared to weak tie networks (classmates

¹² As an example of particular relevance for youths, Forslund and Skans (2006) provide a comparative study showing that work practice programs provide faster access to jobs than training programs for Swedish youths.

parents, neighbors, other plants within parents' firms), suggest that tie strength is particularly important for youths with poor formal merits (low schooling or poor grades). Although policies are unlikely to provide contacts that resemble those provided by family ties, the results, if taken at face value, suggest that policies should be weighted towards few but long-lasting contacts, in particular if targeted towards the weakest youths.

The results discussed in this article also suggest that contacts with employees, and not just employers, are useful. Most notably, the original research presented in this paper shows that co-workers (from summer jobs) who change employers before the young worker graduates continue to provide a useful bridge into employment.¹³ Results from previous research do, however, suggest that individual contacts are more important if these are trusted by their employers. Notably, Kramarz and Skans (2013) show that experienced workers with higher wages matter more. Similarly, Hensvik and Skans (2013) show that firms more often recruit former coworkers of employees with higher test scores. This suggests that it is important to facilitate contacts with workers outside of the youths' own social spheres, since workers within these spheres are likely to be young and inexperienced. In line with this argument, our own results also show that moving contacts are more relevant if they are older (although the reemployment probability is higher if the original workplace had a younger staff).

Here, it is also important to note that solely relying on the youths' non-professional social contacts may be an inefficient policy route. As noted by Bentolila et al. (2010), a heavy reliance on contacts from the social domain may distort occupational choices into fields where the youths have access to better social networks and away from the fields of their productive advantage. In practice, it may therefore be difficult for vocationally inclined youths with academic parents to find employment within a vocational field if the only route to employment within this field goes through non-professional social contacts.

The empirical results also show that contacts (parents and former coworkers) who are trained within similar occupational fields as the graduates are more important. An implication of this result is that policies that are carefully targeted to accomplish a good match between the fields

¹³ Similarly, it should be noted that the results in Kramarz and Skans (2013) exclude self-employed parents.

of education and the types of firms where the contacts are established are likely to be more useful. For example, a closer cooperation between schools and local employers in order to ease the transition into summer jobs placed within a relevant segment of the labor market could further the usefulness of summer jobs as a stepping-stone towards regular employment.

References

- Altonji, J. and Pierret, C. (2001), Employer learning and statistical discrimination, *Quarterly Journal of Economics* 116, 313-350.
- Bayer, P., Ross, S. and Topa, G. (2008), Place of work and place of residence: Informal hiring networks and labor market outcomes, *Journal of Political Economy* 116, 1150-1196.
- Beaman, L. and Magruder, J. (2012), Who gets the job referral? Evidence from a social networks experiment, *American Economic Review* 102, 3574-3593.
- Behrenz, L. (2001), Who gets the job and why? An explorative study of employers' recruitment behavior, *Journal of Applied Economics* 4, 255-278.
- Behtoui, A. (2008), Informal recruitment methods and the disadvantages of immigrants in the Swedish labour market, *Journal of Ethnic and Migration Studies* 34, 411-430.
- Bentolila, S., Michelacci, C. and Suarez, J. (2010), Social contacts and occupational choice, *Economica* 77, 20-45.
- Bewley, T.F. (1999), *Why Wages Don't Fall During a Recession*, Harvard University Press, Cambridge.
- Boorman, S.A. (1975), A combinatorial optimization model for transmission of job information through contact networks, *Bell Journal of Economics* 6, 216-249.
- Brown, M., Setren, E. and Topa, G. (2012), Do informal referrals lead to better matches? Evidence from a firm's employee referral system, Federal Reserve Bank of New York Staff Report 568.
- Calvo-Armengol, T. and Jackson, M.O. (2004), The effects of social networks on employment and inequality, *American Economic Review* 94, 426-454.
- Casella, A. and Hanaki, N. (2006), Why personal ties cannot be bought, *American Economic Review* 96, 261-264.
- Castilla, E.J. (2005), Social networks and employee performance in a call center, *American Journal of Sociology* 110, 1243-1283.
- Cingano, F. and Rosolia, A. (2012), People I know: Job search and social networks, *Journal of Labor Economics* 30, 291-332.
- Dustmann, C., Glitz, A. and Schönberg, U. (2011), Referral-based job search network, IZA Working Paper 5777, Bonn.
- Ekström, E. (2001), *Arbetsgivarnas rekryteringsbeteende*, IFAU Rapport 2001:3, Uppsala.

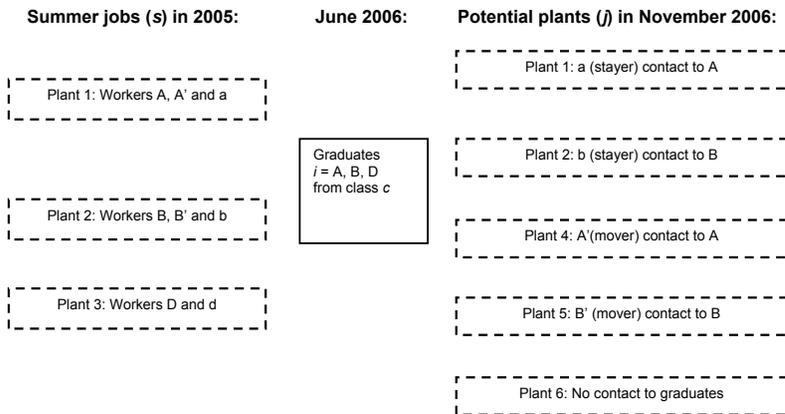
- Eriksson, S. and Rooth, D.O. (2011), Do employers use unemployment as a sorting criterion when hiring? Evidence from a field experiment, IZA Working Paper 6235, Bonn.
- Fernandez, R.M., Castilla, E.J. and Moore, P. (2000), Social capital at work: Networks and employment at a phone center, *American Journal of Sociology* 105, 1288-1356.
- Forslund, A. and Skans, O.N. (2006), Swedish youth labour market policies revisited, *Quarterly Journal of Economic Research* 75, 168-183.
- Forslund, A. and Vikström, J. (2010), Arbetsmarknadspolitikens effekter på sysselsättning och löner – en översikt, Bilaga 1 till Långtidsutredningen 2011, in SOU 2010:88, Fritzes förlag, Stockholm.
- Granovetter, M. (1973), The strength of weak ties, *American Journal of Sociology* 78, 1360-1380.
- Hensvik, L. and Skans, O.N. (2013), Social networks, employee selection and labor market outcomes: Toward an empirical analysis, IFAU Working Paper 2013:15, Uppsala.
- Ioannides, M.Y. and Loury, L.D. (2004), Job information, neighborhood effects and inequality, *Journal of Economic Literature* 42, 1056-1093.
- Jackson, M. (2010), *Social and Economic Networks*, Princeton University Press, Princeton.
- Johansson, P. (2008), The importance of employer contacts: Evidence based on selection on observables and internal replication, *Labour Economics* 15, 350-369.
- Klingvall, M. (1998), Företagens rekryteringsmetoder – hur arbetslösa får jobb, *Arbetsmarknad & Arbetsliv* 4, 291-303.
- Kramarz, F. and Skans, O.N. (2013), When strong ties are strong: Networks and youth labor market entry, forthcoming in *Review of Economic Studies*.
- Kramarz, F. and Thesmar, D. (2013), Social networks in the boardroom, *Journal of the European Economic Association* 11, 780-807.
- Lindahl, L. (2010), Den gymnasiala yrkesutbildningen och inträdet på arbetsmarknaden, Bilaga 2 till Långtidsutredningen 2011, in SOU 2010:88, Fritzes förlag, Stockholm.
- Loury, L.D. (2006), Some contacts are more equal than others: Informal networks, job tenure, and wages, *Journal of Labor Economics* 24, 299-318.
- Liljeberg, L. and Lundin, M. (2010), Jobbnätet ger jobb: effekter av intensifierade arbetsförmedlingsinsatser för att bryta långtidsarbetslöshet, IFAU Rapport 2010:2, Uppsala.
- Montgomery, J. (1991), Social networks and labor-market outcomes: Toward an economic analysis, *American Economic Review* 81, 1408-1418.
- Nilsson, P. (2011), Arbetssökande som lämnat Arbetsförmedlingen för arbete, *Arbetsförmedlingen, Working Paper 2011:2*, Stockholm.
- Nutek (2000), *Kompetens – en bristvara? Företagens syn på kompetensförsörjning*, R 2000:14, Nutek, Stockholm.
- Oyer, P. and Shaeffer, S. (2011), *Personnel economics: Hiring and incentives*, *Handbook of Labor Economics* 4, 1769-1823.
- Pellizzari, M. (2004), Do friends and relatives really help in getting a good job?, *Industrial & Labor Relations Review* 63, 494-510.
- Riksrevisionen (2010), *Arbetsförmedlingens arbete med arbetsgivarkontakter*, RiR 2010:6, Riksrevisionen, Stockholm.

- Simon, C.J. and Warner, J.T. (1992), Matchmaker, matchmaker: The effect of old boy networks on job match quality, earnings, and tenure, *Journal of Labor Economics* 10, 306-330.
- Svenskt Näringsliv (2010), Så rekryterar företagen medarbetare – Svenskt Näringslivs rekryteringsenkät 2010, del 2, Svenskt Näringsliv, Stockholm.
- Åslund, O. and Johansson, P. (2011), The virtues of SIN? Can intensified efforts help disadvantaged immigrants?, *Evaluation Review* 35, 399-427.

Appendix A. Empirical setup and estimated models

Figure A1 below clarifies the setup for our analysis of the role of high school job contacts for entry into the labor market. The data consist of four components: (i) the students (A, B and C) who graduate from a specific vocational track and school in June 2006; (ii) the establishments (“plants”) where the students had a summer or extra job in 2005 (1, 2 and 3); (iii) the former co-workers, or high-school job contacts obtained through these jobs (a, b, c, A’ and B’) and (iv) all potential employers available to students after graduation (1, 2, 4, 5 and 6).

Figure A1. Empirical setup



Source: Original production.

A.1 High school job contacts and place of work

In the first analysis, we examine the propensity that a graduate finds his first stable job in a particular establishment, comparing this propensity for graduates with and without high school job contacts in the establishment. Framed according to the set-up illustrated above, we analyze whether graduate A has a higher propensity to find a stable job in one of the establishments where the contacts work (Establishment 1 and Establishment 4), compared to where they do not work (Establishments 2, 5 and 6).

We restrict our sample to graduates who obtained a stable job after graduation. Given that we know all possible pairs of graduates and potential establishments, we can estimate the following equation:

$$W_{icj} = \gamma \text{Contact}_{icj} + \theta_{cj} + \varepsilon_{icj} \quad (\text{A1})$$

where W_{icj} takes the value of 1 if graduate i from class c is employed by establishment j five months after graduation and Contact_{icj} is an indicator for whether the graduate has a job contact to at least one worker in establishment j . The parameter of interest, γ , captures if a graduate has a higher propensity to sort into establishments where he has high-school job contacts compared to establishments where he does not.

To improve the identification of the parameter of interest, we include a dummy for each combination of class and potential establishment, represented by θ_{cj} . The inclusion of the *class*×*establishment* fixed effects ensures that we exploit variation in job contacts between individuals graduating from the same class and is designed to account for any correlation in unobserved characteristics that could generate a spurious correlation between the presence of a contact and the propensity to find a job at a particular establishment.

Following Kramarz and Thesmar (2013) and Kramarz and Skans (2013), we limit the sample under study to cases where there is within-establishment variation in the contact-variable of interest. Thus, we exclude establishment-class combinations where no contacts to any of the class's graduates are employed, such as the combination between class c and Establishment 6 in the example illustrated in Figure A1.

We can also enrich equation (A1) by allowing the contact effect to be different depending on whether the contact stayed or moved from the establishment where he/she interacted with the graduate. Hence,

$$W_{icj} = \gamma_1 \text{Contact who stayed}_{icj} + \gamma_2 \text{Contact who moved}_{icj} + \theta_{cj} + \varepsilon_{icj} \quad (\text{A2})$$

where γ_1 captures the propensity that a graduate is employed in the summer/extra job establishment (i.e. graduate A finds stable employment in Establishment 1), and γ_2 captures the propensity to end up in an establishment to which the summer/extra high school job contact has moved (i.e. graduate A finds stable employment in Establishment 4).

A.2 High school job contacts and the job finding rate

In the second analysis, we assess whether the employment rate in the network of high school job contacts is associated with the graduate's job finding rate after graduation. Therefore, we add all graduates who had not yet found a job in November 2006 (e.g. graduate D) and estimate the following model:

$$E_i = \lambda_1 ER_i + \lambda_2 N_i + X_i \beta + \theta_c + \varepsilon_i \quad (\text{A3})$$

where E_i measures the employment status of graduate i after graduation; ER_i is the employment rate among the high-school contacts (a, b and d) in 2006; X_i is a vector of individual characteristics (gender, immigrant background, the student's income from 2005 and the final high school grades); N_i is the total size of the network of high-school contacts and θ_c is class-fixed effects.

The assumption required for the identification is that the network employment rate is uncorrelated with the error term once we control for the observable characteristics and the class-fixed effects. The class-fixed effects are important since they ensure that we compare students facing the same local labor market, which mitigates the potential issue of common exogenous unobserved factors affecting the employment prospects for all individuals in the same industry and location. Furthermore, we check the sensitivity of the identifying assumption by stepwise including the student's previous income and the final grades, as it is reasonable to suspect a correlation between these controls and the unobserved abilities potentially correlated with the network employment rate.

Appendix B. Additional tables

Table B1 All graduates in 2006

	mean	sd	median	min	max
All graduates:					
Age	19	0	19	19	19
Female	0.46	0.50	0.0	0	1
Immigrant background	0.06	0.24	0.0	0	1
Grade percentile rank	0.50	0.23	0.50	0	1
Number of class mates	290.2	290.2	22	0	261
<i>Vocational track (10 most common):</i>					
Media	0.08	0.28	0	0	1
Hotel- and restaurant	0.08	0.27	0	0	1
Electronics, data and telecommunication	0.07	0.26	0	0	1
Childcare	0.07	0.26	0	0	1
Energy and electricity	0.07	0.25	0	0	1
Music and drama	0.06	0.25	0	0	1
Construction	0.06	0.25	0	0	1
Business	0.06	0.23	0	0	1
Technical	0.05	0.23	0	0	1
Health care	0.05	0.22	0	0	1
Other	0.33	0.47	0	0	1
<i>Contacts</i>					
Summer/extra job during high school	0.68	0.47	1	0	1
Observations			39 093		
Graduates with summer/extra job during high school:					
Female	0.47	0.50	0	0	1
Immigrant background	0.04	0.21	0	0	1
Grade percentile rank	0.52	0.29	0.52	0	1
Average monthly earnings from summer/extra job (SEK)	3 374	3 915	2 172	80.3	92 067
Number of contacts	315	923	68	1	11 701
Employment rate of contacts	0.80	0.15	0.83	0	1
Observations			26 598		

Source: Own calculations based on data from Statistics Sweden.

Note: Descriptive statistics of the data sets used.

Table B2. High school job contacts and place of work, by vocational track

	(1)	(2)	(3)
	Average effect	Effect by contact relevance:	
	All contacts	Relevant contacts	Not relevant contacts
Establishments where the link is through contacts that have moved:			
Construction	0.0058*** (0.0009)	0.0283*** (0.0033)	0.0033*** (0.0006)
Energy and electricity	0.0033*** (0.0005)	0.0227*** (0.0041)	0.0022*** (0.0004)
Hotel and restaurant	0.0022*** (0.0003)	0.0053*** (0.0019)	0.0019*** (0.0003)
Health care	0.0010*** (0.0003)	0.0033*** (0.0009)	0.0006*** (0.0003)
Business	0.0039*** (0.0005)	0.0069*** (0.0013)	0.0036*** (0.0005)
Auto	0.0026*** (0.0006)	0.0116*** (0.0037)	0.0021*** (0.0005)
Media	0.0022*** (0.0003)	0.0058** (0.0025)	0.0021*** (0.0003)
Machinery	0.0009 (0.0007)	0.0022** (0.0008)	0.0008 (0.0007)
Electronics and computer science	0.0012*** (0.0004)	0.0009 (0.0015)	0.0013** (0.0004)
Childcare	0.0015*** (0.0004)	0.0028* (0.0017)	0.0015*** (0.0004)

Source: Own calculations based on data from Statistics Sweden.

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors clustered at the class (*school*field*) level reported in parentheses. Each row in column (1) reports the estimate from a separate regression for each of the ten largest vocational tracks in the sample. Columns (2) and (3) report the differential impact of relevant and non-relevant contacts obtained from one pooled regression for each row panel with interactions. The estimates and standard errors of the combined effects have been calculated using nlcom in Stata (see Appendix A for more details).

Comment on Hensvik and Nordström Skans: Networks and youth labor market entry

Daniel le Maire*

Finding the right job match is a time-consuming and costly process for both workers and firms. It is a problem of information about job seekers and vacancies which implies that idle resources exist on both sides of the market. To reduce the costs of the job matching process, it may be profitable for the agents involved to use social networks. The paper by Hensvik and Skans provides an insightful literature review on the effect of networks on the probability of finding a job as well as a novel empirical analysis. In this comment, I will discuss the identification used in the analysis and the potential policy implications of the findings.

It is a fact that informal contacts including networks are important for the job matching process. According to Swedish survey evidence, between 40 and 70 percent of all jobs are found through informal contacts and this percentage is even higher for young people.¹ Hensvik and Skans consider the effects of a network from a summer job the year before graduating from Swedish vocational schools. The first result is that graduates working at a particular establishment during the summer have a 35 percentage point larger probability of being hired later at the same establishment. However, as the authors are fully aware of, this higher probability is not only a pure network effect, but is also likely to reflect the accumulation of firm- or match-specific human capital during the summer job.

* Department of Economics, University of Copenhagen, Daniel.le.Maire@econ.ku.dk.

¹ Similar percentages are found for other countries. For example, in Denmark 61 percent found a job through informal contacts in 2012 (Danish Labour Force Survey).

Instead, a cleaner network effect is obtained by considering whether co-workers from the summer job who change jobs increase the probability for the graduate of finding a job in the co-workers' new establishments. It is found that each co-worker moving to another establishment increases the probability of the graduate getting a job there by 0.3 percentage points. With a median of 15 co-workers changing jobs, the increased probability of getting a job in a new establishment where a former co-worker was hired is about 4 percentage points.

For the interpretation of this effect, it is not a problem that the co-worker has better knowledge of the graduate from the summer job as this is essentially part of the network effect, but it is a concern if the higher employment probability reflects that certain types of skills are accumulated in summer jobs in a particular firm and that these particular skills, e.g. industry-specific skills, are always very useful for specific firms. To verify that this is not the case, Hensvik and Skans use placebo co-workers who changed jobs before the students had their summer job and there is a zero effect on the probability of getting a job at the placebo co-workers' new establishments.

Therefore, the human capital accumulation in the summer job needs to be of a very specific type to challenge the identification: If a graduate during his summer job accumulates a particular set of skills that enhances the productivity of the co-worker, this may in principle confound the estimation. Furthermore, if firms have a fixed number of vacancies, a graduate having a co-worker in a firm may decrease the classmates' chance of getting a job in the same firm, that is, the so-called stable treatment unit value assumption may be violated. However, neither of these two potential violations of the identifying assumptions seems powerful enough to really impact the estimated effects, and we can safely consider the estimated effects as causal effects.

The results in the paper are not directly related to labor market policies, but the significance of the results suggests that it may be useful to consider network effects in a broader labor market policy context. From a policy perspective, it is important how networks play a role in the job matching process. Hensvik and Skans distinguish between supply-side and demand-side oriented effects of networks, and this distinction is crucial for the design of a policy which aims at mimicking the effect of networks. In the supply-side oriented explanation, information about a va-

cancy is spread out in networks without the firm and its employees actively using their social contacts. In contrast, with the demand-side oriented explanation, firms use social networks actively in the hiring process in order to target the right person.

If the supply-oriented explanation is most relevant, the focus should be on improving the databases on available jobs and unemployed workers. If the demand effect is the main channel through which networks operate, it seems more difficult to design policies that directly function as networks. Could a sort of middleman, e.g. a caseworker at a job center, facilitate job matching through a network? One problem is that job-, person- and skill-specific knowledge of the middleman may not be adequate, but this could presumably be solved if the middleman had fewer clients than the typical caseworker. A more critical problem is that the incentives of such middlemen are likely to be different compared to individuals in a usual network. For example, it may be required that a case worker helps facilitate a specific number of job matches in order to qualify for a bonus which potentially gives rise to a credibility problem.

It is unfortunately very difficult to quantify the relative importance of, respectively, the demand- and supply-oriented explanations of how networks affect the job matching process. Kramarz and Skans (2011) provide compelling evidence for the network effect through strong social ties being more important for finding a job than the network effect through weak social ties which indicates that the demand-oriented explanation is the most relevant. Furthermore, Kramarz and Skans (2011) find that only network effects through strong social ties vary with the education of the young worker, such that the network effect is larger for graduates with less education. As the unemployment rate usually decreases in education, a policy aiming at reducing youth unemployment should therefore incorporate the demand-oriented network effect rather than the supply-oriented network effect.

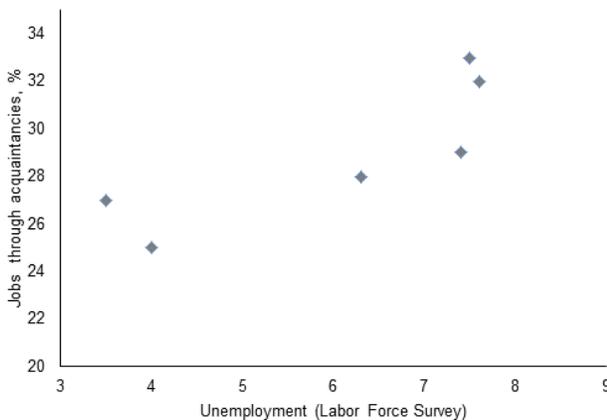
Hensvik and Skans discuss different types of policies which could be considered to exploit networks. First, the apprenticeship system for vocational education compared to training in schools is more likely to ease the transition from school to work.² Second, it is possible that active labor market policies can incorporate a network dimension and, more specifically, that the fact that most studies tend to find that private job activation

² See Albæk (2009) for a review of the apprenticeship system with a focus on Denmark.

works better than education activation can be due to the unemployed getting a network in the former.

Interestingly, Kramarz and Skans (2011) find that the county-level unemployment rate is positively related to the size of the network effect.³ Although we should be somewhat cautious with too strong conclusions based on the few data points in Figure 1, it also seems to suggest a positive relationship between the share of jobs found through networks and the unemployment level in Denmark for 2007-2012. This is especially interesting as this is in contrast to active labor market policies which tend to be less effective in recessions.⁴ If anything, in a recession it seems more important to incorporate a network aspect in the active labor market policy.

Figure 1. Jobs found through acquaintances and unemployment, 2007-2012



Source: Danish Labor Market Survey, Statistics Denmark.

Whereas it may turn out to be difficult to incorporate network effects in traditional active labor market policies, it is possible in small-scale initiatives. An example of a policy directly using networks is “From the

³ In Kramarz and Skans (2011), the focus is on the network effect of a parent employed in an establishment rather than former co-workers.

⁴ The Danish Economic Council (2012) finds that the combined lock-in and program effect of active labor market policies tends to shorten the unemployment spells in the recent economic boom between 2005 and 2007 and lengthen the unemployment spells in the recent economic downturn from 2009 to 2011. Taking the motivation effect into account, the Danish Economic Council finds that unemployment is also reduced in the recession.

Bench to the Pitch” which was conducted by the Danish football club Brøndby IF between 2002 and 2008.⁵ The idea behind the project was that Brøndby IF has some knowledge about the strengths and weaknesses of its young players, while at the same time having access to high-level company representatives among its sponsor firms. Since about 30 percent of Brøndby IF’s amateur players have an immigrant background, the project was set-up in cooperation with the Danish Ministry of Integration as well as the Municipality of Brøndby. Between 2002 and 2008, 700 regular jobs and apprentice type jobs were created, but unfortunately no real policy evaluation has been conducted and we cannot tell how many of the members of the football club would have found a job without the initiative.

In sum, it seems to be relevant to bear network effects in mind when designing active labor market policies, especially when policies target young workers whose job finding relies more on informal channels. In this respect, the results from the project “From the Bench to the Pitch” seem encouraging.

References

- Albæk, K. (2009), The Danish apprenticeship system, 1931-2002: The role of subsidies and institutions, *Applied Economics Quarterly* 55, 39-60.
- Danish Economic Council (2012), *Dansk Økonomi Efterår 2012*, Danish Economic Council, Copenhagen.
- Kramarz, F. and Skans, O.N. (2011), When strong ties are strong – Networks and youth labor market entry, manuscript, IFAU, Uppsala.
- OECD (2007), *Jobs for Immigrants, Vol. 1: Labour Market Integration in Australia, Denmark, Germany and Sweden*, OECD, Paris.

⁵ See also OECD (2007, p. 141).

Effects of payroll tax cuts for young workers^{*}

Per Skedinger^{**}

Summary

In response to high and enduring youth unemployment, large payroll tax cuts for young workers were implemented in two Swedish reforms in 2007 and 2009. This paper analyses the effects of the reforms on worker outcomes and firm performance in the retail industry, an important employer of young workers. In general, the estimated effects on job accessions, separations, hours and wages, are small. For workers close to the minimum wage the estimates suggest larger, but still modest, effects on the probability of job accession. There is also some evidence of increasing profits in a subsample of firms that employed a relatively large number of young workers before the first reform, with estimated effects commensurate with small behavioural effects of the payroll tax cuts. The conclusion is that reducing payroll taxes is a costly means of improving employment prospects for the young.

Keywords: tax subsidy, labour costs, minimum wages, retail industry.

JEL classification numbers: H21, H25, H32, J38.

^{*} I am grateful to Johan Egebark, Caroline Hall, Niklas Kaunitz, Michael F. Maier, Håkan Selin, Mikael Stenkula, Joacim Tåg, Thomas Tangerås and seminar participants at IZA, IFAU and IFN for helpful suggestions and comments, Aron Berg, Joakim Jansson, Louise Johanneson and Dina Neiman for expert research assistance and Björn Lindgren and Pär Lundqvist for kindly providing the payroll data. Financial support from IFAU and Jan Wallander and Tom Hedelius' Research Foundations is gratefully acknowledged.

^{**} Research Institute of Industrial Economics (IFN), e-mail: per.skedinger@ifn.se.

Against a backdrop of high and rising youth unemployment, the Swedish government adopted two payroll tax reforms, in 2007 and 2009. The purpose of the reforms was to increase the opportunities for young workers to gain entry to the labour market. The payroll tax reduction was relatively large – 11.1 percentage points after the first reform and 15.9 percentage points after the second one – and targeted towards young workers. The size of the reduction and the fact that it was not generally applied to all segments of the labour force should help identifying the effects of the reforms.

This paper analyses the effects of the payroll tax reductions on employment, wages and profits in a specific industry, namely retail. There are many young workers in this industry and the share of labour costs in relation to total costs is high. The detailed payroll data used in this study also allow an analysis of the extent to which minimum wages play a role in how payroll taxes affect employment for young workers. Collectively agreed minimum wages are binding for blue-collar workers in retail, which speaks for the possibility that workers with the lowest wages may be differently affected than other workers. For a subset of firms, the payroll data have been linked with a database containing accounting information which makes it possible to analyse whether the reforms also affected firms' profits.

Standard theory on payroll taxation predicts that the consequences for employment depend on the extent to which such a tax, if levied on the employers, is shifted onto employees in the form of wage increases. However, a number of institutional factors might prevent such shifting. In the short run, wages may be fixed by collective bargaining for a number of years, so that payroll tax reductions will translate into higher wages only in the long run, ultimately eroding any increases in employment. Even in the long run, wage adjustment may be prevented for some marginal workers by the presence of statutory minimum wages. It has often been argued that payroll tax cuts should be targeted towards marginal groups, such as youth, the low-skilled, the work disabled and the long-term unemployed (see, for example, the OECD, 2003). With binding minimum wages, a case can be made for such a policy, since it is less likely that changes in payroll taxes will affect wages.

A number of empirical studies have investigated the links between payroll taxes, employment and wages. Reductions of payroll taxes in

regional ‘support areas’ in the Nordic countries have been examined by Benmarker et al. (2009), Korkeamäki and Uusitalo (2009) and Korkeamäki (2011). None of the studies finds any evidence that employment increased in the target regions as a consequence of the payroll tax cuts, which amounted to 10 percentage points in Sweden and 3-6 percentage points in Finland.¹ However, wages seem to have increased in the support areas according to these studies (with the exception of Korkeamäki (2011), where the effects are mostly insignificant). Huttunen et al. (2013) examine a payroll tax reduction – up to 14 percentage points depending on the wage – targeted towards older, low-wage workers in Finland. They report no effects on employment or wages, but a slight increase in hours worked among those already in employment before the reform. Much of the evidence based on reductions of general, flat rate payroll taxes yields similar conclusions, namely partial shifting of wages and weak employment effects.² These empirical studies thus support the predictions of the standard theory.

Few studies, however, consider reductions targeted towards groups that may be especially susceptible to labour market rigidities. Kramarz and Philippon (2001) analyse the employment effects of the substantial reduction of payroll taxes in France – up to 15 percentage points – for workers on or close to the statutory minimum wage, most of whom are young. Their results indicate that increases in wage costs (including payroll taxes) were associated with more transitions from employment to non-employment. The results for decreasing wage costs were less clear cut; the effect on transitions from non-employment to employment seems to have been dampened by labour-labour substitution, in favour of workers whose wage costs were reduced in connection with the cut in payroll taxes.

There seems to be only one previous empirical study examining how payroll taxes affect firm performance, namely Korkeamäki (2011). He finds negligible effects on profits of the Finnish regional payroll tax re-

¹ The Swedish payroll tax cut studied by Benmarker et al. (2009) was in fact rather small, since the full reduction only applied to wage bills not exceeding SEK 852 000 per year, which roughly corresponds to three full-time blue-collar workers.

² See, for example, Cruces et al. (2010) for Argentina, Gruber (1997) for Chile, Bauer and Riphahn (2002) for Germany, Holmlund (1983) and Pencavel and Holmlund (1988) for Sweden, and Anderson and Meyer (1997) and Murphy (2007) for the US. An exception is Kugler and Kugler (2009) who find modest wage effects and relatively large decreases in employment following payroll tax increases in Colombia.

duction, which is puzzling considering the small employment and wage effects reported in the study.

The most closely related study to the present one is Egebark and Kaunitz (2013), which also examines the effects of the 2007 and 2009 reforms of payroll taxes in Sweden. In general, they find evidence of a modest increase in employment, but little impact on wages. Unlike me, they are able to study heterogeneous effects with respect to country of birth and education. My analysis differs from the one in Egebark and Kaunitz (2013) in several additional ways: only those employed in a specific industry are included, rather than all employees; the analysis differentiates between entry into and exit from employment and also considers effects on hours per worker; an analysis of heterogeneity in treatment effects for workers bound by minimum wages is undertaken; and effects on firms' profits are considered.

Another related study is Benmarker et al. (2013) which examines the wage effects of the payroll tax reductions of 2007 and 2009, also taking into account other labour market reforms during the period of study. Their results mostly suggest insignificant effects of the payroll tax cuts (in contrast to the wage effects found for earned income tax credits and reductions of the replacement rate).

My key finding for the post-reform period 2007-2011 is that employment in general was only modestly increased, at best, by the payroll tax reforms. For workers bound by minimum wages I estimate larger, but still modest, effects on entry. Hours and wages seem to have been little affected. I find that the profits increased in a subsample of firms employing many young workers before the first reform, which is consistent with my other results, but strong conclusions regarding profits cannot be drawn without a larger sample of firms. While the financial crisis obviously poses problems for an evaluation of the long-term effects, my results for 2007 and 2008 – before the crisis set in with full force – do not suggest more than slight increases in employment.

The paper is organised as follows: The next section discusses the payroll tax reforms of 2007 and 2009 in more detail as well as describing the most important features of the Swedish payroll tax system in general. Other reforms during the period of study that may have impinged on labour market prospects for young workers are considered in Section 2. Section 3 provides a brief account of the theoretical arguments regarding

the effects of payroll taxes, with special attention paid to the case with minimum wages. Section 4 deals with the specifics of wage formation in retail for blue- and white-collar workers. The data for the retail industry and the empirical specification are presented in Section 5. In Section 6, the econometric results are discussed, while Section 7 summarises the results and deals with policy implications.

1. The Swedish payroll tax system and the reforms of 2007 and 2009

Swedish payroll taxes are levied on employers and are basically proportional to the wage bill. The legally mandated system of payroll taxes covers all employers. Employers bound by collective agreements with trade unions are also subject to collectively agreed payroll fees, on top of the taxes. In the private sector, there are separate agreements for blue- and white-collar workers. Separate agreements also apply for workers employed in the public sector.

The payroll tax reforms in 2007 and 2009, implying substantial reductions in the tax rates for young workers, were initiated as a response to growing concerns about rising youth unemployment. At the time, the relatively high unemployment rate among the young in comparison with other countries was often pointed out in the public debate.

The explicit purpose in the bill behind the first reform, presented to the *Riksdag* on March 15 2007, was to increase the opportunities for young people to gain entry to the labour market (Government Bill 2006/07:84). The cut in payroll taxes, from 32.42 to 21.32 per cent for workers aged 19 to 25, gained legal force on July 1 2007. Limiting eligibility to individuals aged at least 19 years was motivated by concerns that a lower age threshold would increase the incentives to drop out of high school, which is normally finished in the year during which the pupils turn 19. The motivation behind the upper age limit was that young workers supposedly have gained sufficient labour market experience by the age of 25, so a tax reduction should have little importance.

The second reform was implemented on January 1 2009. The payroll tax rate was decreased further, from 21.32 to 15.52 per cent, and the lower age threshold was abolished and the upper one extended to 26. An

explicit purpose in the bill of September 25 2008 was to create permanently higher employment in the target group through the tax cut (Government Bill 2008/09:7). The government's motives for abolishing the lower age limit for eligibility was that the rules would be simpler to apply and that the demand for younger workers, including those seeking holiday work, would increase. The motivation for increasing the upper age limit was rather vague, simply given as a way of 'reinforcing the efforts of getting more young people into work'.

Statutory payroll taxes consist of the following components (with the rates before the first reform for January 1 2007, totalling 32.42 per cent, in parentheses):

- sickness insurance fee (8.78 per cent)
- parental insurance fee (2.20 per cent)
- old-age pension fee (10.21 per cent)
- pension for surviving family members fee (1.70 per cent)
- labour market fee (4.45 per cent)
- work injury fee (0.68 per cent)
- employers' fee (4.40 per cent)

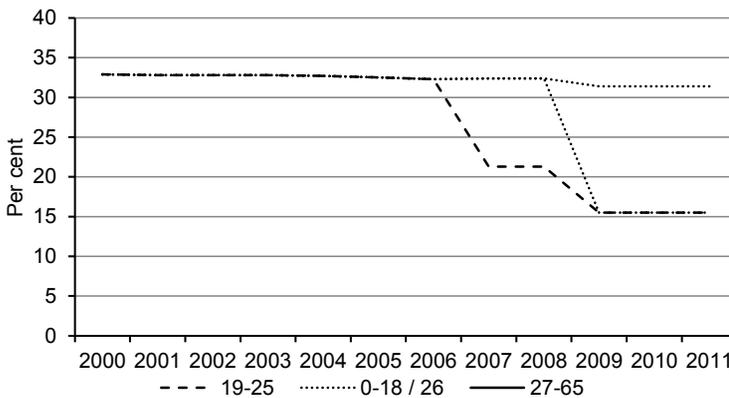
All these components are linked to benefits conditional on labour force participation, except the employers' fee which then acts as a pure income tax. However, as discussed by Flood et al. (2013), the link to benefits is not direct for the other components of the payroll tax. Earnings below or above certain thresholds (varying depending on component and related to 'basic amounts') do not generate additional benefits, but these thresholds apply to few young workers. Even for earnings between the thresholds, the link to benefits is not complete. Sometimes the collected fees have been used for other purposes than social insurance benefits and deficits have been covered by other taxes. Flood et al. (2013) estimate that for income earners between the two thresholds, about 40 per cent of the payroll taxes constitute a pure income tax and this estimate may well be a good approximation also for young workers.

According to the reform implemented on July 1 2007, the rates applying to all components except the old-age pension fee were reduced by 50 per cent for young employees. This implied a reduction of $(1-0.5)(32.42-10.21) = 11.1$ percentage points in total payroll taxes for

this group. Since both total payroll taxes and the old-age pension fee remained the same in 2008, the formula implied an 11.1 percentage point reduction also during this year. The reduction of payroll taxes became more generous on January 1 2009, as only 25 per cent of the components besides the old-age pension fee had to be paid, implying a reduction of $(1-0.25)(31.42-10.21) = 15.9$ percentage points. Since 2009 the formula, and the reduction in percentage terms, have remained the same. The payroll tax cuts for young workers were not associated with any reductions in the benefits linked to these taxes.

Figure 1 depicts the evolution of payroll tax rates over the period 2000-2011.³ The regular rate has not changed much – the variation over time is only 1.5 percentage points. The rate stood at 32.9 per cent in 2000 and had declined to 31.4 per cent by 2011. The first payroll tax reform of 2007 implied a rate of 21.3 per cent for 19-25-year-olds and in the second reform of 2009, the rate was reduced further, down to 15.5 per cent, and the group of eligible workers was extended to include all individuals up to age 26.

Figure 1. Payroll tax rates, by age group, 2000-2011. Per cent



Source: National Mediation Office.

Notes: Regional reductions, implemented in 2002 in mainly the northern parts of Sweden, are not accounted for.

For evaluation purposes, it is of some interest to examine the letter of the law and how legal formulations may have influenced public percep-

³ The regional reduction of 10 percentage points, in effect from 2002, is not accounted for in the figure.

tions regarding the eligibility for payroll tax reductions. The legal document specifying the details of the first reform in 2007 contains the following core sentence: ‘On the compensation to persons who *at the commencement of the year* have turned 18 years of age but not 25, the full old age pension fee but only half of the other payroll taxes should be paid’ (SFS 2007:284, my translation and italics). The implication of this somewhat complicated formulation – it seems more straightforward to refer to someone’s birth year instead⁴ – is that the payroll tax cut applied to those who turned at least 19, but not 26 or more, during the year when the reform was first implemented on July 1 2007. A similar formulation was used when the tax cuts were extended in 2009: ‘On the compensation to persons who at the commencement of the year have not turned 26 years of age, the full old age pension fee but only a quarter of the other payroll taxes should be paid’ (SFS 2008:1266).

The legal formulations may have invited misunderstandings regarding eligibility for the payroll tax cuts. For example, several press reports stated that the first reform applied to 18-24-year-olds and the second to individuals below the age of 26. It is difficult to assess how widespread any misunderstanding has in practice been among employers in the retail industry, and to what extent the take-up rates have been affected. Evidence on special payroll tax reductions for disadvantaged groups from Belgium and the Netherlands, reported in Marx (2001), suggests that mainly three factors contribute to the non-take-up among firms: (i) unawareness of the reduction; (ii) perceptions that the reduction is temporary; and (iii) perceptions that the take-up is associated with large administrative costs. Moreover, non-take-up turned out to be more prevalent among small firms, possibly due to the fact that fixed costs of information-gathering and administration are spread out over fewer employees than in large firms.

These findings may be of relevance also in the context of the Swedish reforms, except that the associated administrative costs should be negligible in relation to reductions in total wage costs, since no application procedures were necessary. The government never explicitly stated that the reductions were of an experimental or temporary nature, but the political parties in opposition were against them before the general elections in 2010 (which they lost). Any misperceptions regarding the eligibility of

⁴ This kind of formulation would require that it be changed every year, though.

payroll tax cuts in terms of age may have been more pronounced among small firms, especially those with no or few young employees before the reform. However, such misperceptions should abate over time as the likelihood of gaining access to the correct information increases.

2. Other reforms in 2007

Three additional reforms were undertaken during the period of study that could potentially impinge on labour market outcomes for young people. The reforms concerned income taxation, employment protection legislation and active labour market policy.

First, earned income tax credits were introduced in 2007, that is, in the same year as payroll tax rates were cut for the first time. The tax reductions applied to all earned income for all workers, regardless of age, and were extended in three additional stages during 2008-2010. As low-income earners received somewhat larger tax credits in relation to their income than individuals with higher income, it is conceivable that employment and wages among young workers were affected in a different way than those for older workers. For example, the tax credits could have contributed to an increase in labour supply, lower wages and increased employment, especially so among the young. If this is the case, employment estimates of the payroll tax reform could be biased upwards. From the analysis of Edmark et al. (2012), labour market effects of the tax credits cannot be established with any certainty. However, Bennmarker et al. (2013) conclude that the reforms contributed to lower wage pressure.

Second, another reform in 2007 made it easier for employers to hire workers on a temporary basis. New legislation allowed employers to use fixed-term contracts for any reason and for a period of up to 24 months (the previous maximum was 12 months). The loosening of the regulation may have had an impact on the employment of the young, among whom temporary work is relatively more widespread. It is difficult to determine the direction of the potential bias on the estimated employment effect of the payroll tax reform, since more use of temporary contracts could contribute to an increase in both hirings and firings. On paper, the reform was far-reaching. This is reflected in the OECD's index of regulation of temporary work for Sweden, which was reduced from 1.6 to 0.9 (on a scale

from 0 to 6). However, as the Swedish system of employment protection legislation allows employers and unions to depart from substantial parts of the legislation in collective agreements, legal changes do not necessarily translate into changes in practice. According to Skedinger (2012b), only 4 per cent of the temporary workers were employed with the new contracts in 2010, which suggests that the reform had little impact on actual hiring practices in the labour market during the period of study.

Finally, the New-Start Job scheme (*nystartsjobb*) was introduced in 2007. The scheme is targeted towards people who have been unemployed or received sickness or disability benefits for at least one year, waiving all payroll taxes for employers who hire someone in the targeted group, for as long as the non-employment spell lasted and up to five years. In 2009, the size of the employment subsidy was doubled (amounting to 62.8 per cent of the wage). From the start, special rules applied to those aged 21-26: eligibility already after six months of non-employment and a maximum period of one year in the scheme. It is not possible to identify participants in the New-Start Job scheme in the data, which means that its employment effects could be wrongly attributed to the payroll tax reductions under investigation in the empirical analysis, implying an upwards bias. Very few young people took part in the scheme. It increased in size from 10 000 participants in July 2007 to 45 500 on average in 2011, of which only 3 900 were 18-24 years old (according to the Public Employment Service). During the period 2007-2011, at most around one per cent of all employed individuals aged 18-24 participated in the New-Start Job scheme (according to Statistics Sweden). However, to the extent that the treatment and controls in my analysis are treated differently by the New-Start Job scheme, this might affect the results.

In conclusion, due to the few young workers involved, there is little to suggest that the loosening of the regulation of fixed-term contracts or the introduction of the New-Start Job scheme should seriously distort my evaluation of the payroll tax reform. Since the tax credit reform applied to all young workers, it cannot be ruled out that it had an impact on the labour market for the young.

3. Payroll taxes in economic theory

A core result in the standard theory on payroll taxation states that the consequences for employment depend on the extent to which such a tax, if levied on the employers, is shifted onto employees. If, say, a reduction of the payroll tax rate is fully shifted to employees in the form of a wage increase, equal to the payroll tax reduction in percentage terms, no impact on employment is expected. In the case of partial shifting, where the wage increases by less than the percentage reduction in the payroll tax, the demand for labour will increase. The more closely tied payroll taxes are to benefits valued by workers, which tends to be the case for components related to social security contributions, the more shifting is likely to occur (Summers, 1989).

A number of institutional factors might prevent shifting to wages in the short run, however. For example, with collective bargaining, wage rates may be set at fixed levels for a number of years and adjustments will only occur in the longer run as wages are re-negotiated when the agreement expires. In this context, the degree of shifting to workers in the longer run is also likely to depend on the bargaining power of trade unions vis-à-vis employers.

In the standard textbook model of tax incidence, with perfect competition in factor and product markets, it does not matter whether a tax subsidy is provided to the employer or employee. The equilibrium quantities of labour are determined by the elasticities of demand and supply and factor substitutability. However, if one exogenously imposes a statutory minimum wage that exceeds the equilibrium wage for a certain segment of the labour force, that is, a binding minimum wage, the effects of income tax cuts and payroll tax cuts will no longer be similar. With binding minimum wages, a shift in the supply curve induced by an income tax cut will not necessarily increase employment. For a given pre-tax wage, workers want to supply more labour than what employers demand. However, if the government reduces the payroll tax, the demand curve shifts so that employment increases. At any given pre-tax wage, employers want to hire more labour since it has become less expensive to do so.

It is thus far from obvious that the reasoning regarding payroll taxes for the labour market in general applies with equal force to the labour market for the low-paid (Lee and Saez, 2012; Nickell and Bell, 1997;

Pissarides, 1998). With collectively agreed minimum wages, as in Sweden, it remains an open question how these rates evolve in response to changes in payroll taxes. If the payroll tax cut triggers a minimum wage hike, employment will not necessarily increase in the long run.

In effect, an implicit zero-profit condition assumes away any effects on profits in tax models that rely on perfect competition in the product market. However, in the short run and with imperfect competition, lower payroll taxes may well translate into higher profits.

4. Wage formation in retail

In the Swedish retail sector, wages for blue-collar workers are determined in collective agreements between the Commercial Employees' Union (*Handelsanställdas förbund*) and the Swedish Trade Federation (*Svensk Handel*). White-collar workers in retail may be covered by different collective agreements. The employers' agreement with *Tjänstemannaförbundet HTF* (merged into *Unionen* in 2008) was the major agreement in the sector during the period of study, covering lower-level white-collar occupations requiring secondary education. Employees in white-collar occupations requiring tertiary education are covered by employers' agreements with different associations, depending on occupation, within the Swedish Confederation of Professions (SACO).

Of major interest in this study are the agreements covering the majority of young workers, namely those involving the blue-collar workers in the Commercial Employees' Union and white-collar workers in *Unionen*. During the reform period analysed in the study, two agreements for blue-collar workers have been effective. The first such agreement covered the period April 1 2007 to March 31 2010 and the second relates to the period April 1 2010 to March 31 2012. The main agreements for white-collar workers were also two by number during the reform period and implemented at about the same times and with the same lengths as those for blue-collar workers (from May 1 2007 to April 30 2010 and from May 1 2010 to April 30 2012). According to my conversations with representatives of the employer organisation, negotiators on both sides were well aware of the forthcoming cut in payroll taxes for young workers during wage negotia-

tions in the spring of 2007.⁵ Thus, it cannot be ruled out that the reform had an impact on the outcome of the negotiations even before the reform was implemented.

The two above mentioned agreements specify contractual wage increases as well as minimum wage levels at the industry level for various categories of workers.⁶ Regardless of contract length, contractual wage increases and minimum wage levels are determined on a year-to-year basis. In the agreement for blue-collar workers, minimum wage rates are differentiated by age and experience.⁷ Typically, the same minimum wage increase in SEK per hour or month applies to most age groups, so rates for younger and more inexperienced workers usually increase more in percentage terms. Similarly, contractual wages tend to increase by the same amount in SEK for all workers, regardless of age. Minimum rates for white-collar workers are conditional on age only and two different rates apply, to workers aged 20-23 and 24 or older. Minimum wages for blue-collar workers in retail are binding, with distinct spikes at the minimum wages in the wage distribution (Skedinger, 2013). As only few of the white-collar workers are thus affected by minimum wages, it seems unlikely that minimum wage increases should have any effect on actual wages for this category of workers in the retail industry.

Local wage formation is another source through which the payroll tax reforms could have an impact on the wages of young workers. For blue-collar workers in retail, contractual wage increases do not only consist of a general increase, applying to all workers, but also of a ‘wage pot’ to be distributed at the local level to all workers at least 18 years of age (National Mediation Office, 2012). Over the period 2007-2010, the amounts allotted to the wage pot have constituted 40 per cent of the total wage increase in the agreements. In the agreement for 2011, the share increased to 50 per cent. For white-collar workers covered by the *Unionen* agree-

⁵ The Centre-Right coalition announced their intention to reduce payroll taxes for young workers in the 2006 election campaign. The first reports in the press mentioning July 1 2007 as a possible date for the reform seem to be dated October 5 2006, two weeks after the coalition had won the elections (Brors, 2006).

⁶ Due to the high coverage of collective agreements, there are *de facto* minimum wages in Sweden, despite their absence *de jure*. The rates for blue-collar workers are in general among the highest in the world, both in terms of absolute levels and in relation to other wages in the economy (Skedinger, 2010).

⁷ Different scales apply for workers aged 16, 17, 18 and those aged 19 or older. For workers aged 18 or older who have acquired industry-specific experience, the rates are differentiated by such experience (1, 2 or 3 or more years).

ment, wage formation is more decentralised than for blue-collar workers as the agreement specifies a 'wage pot' for local distribution supplemented with rules guaranteeing increases also at the individual level.

It is inherent in the design of the wage bargaining system for blue-collar workers that a larger wage increase in per cent accrues to young workers than to older ones (it seems unlikely that this is undone through the distribution of the wage pot at the local level). However, jacking up contractual wages as a means of taking advantage of a cut in payroll taxes targeted at the young is quite a blunt instrument for the union, since older workers would also receive a higher wage. A minimum wage hike seems to be a more plausible outcome.⁸ The more decentralised wage bargaining system for white-collar workers implies a wider scope for firm-level bargaining to affect wages for the young, but even in this context, it may be difficult for unions at the local level to implement targeted wage increases. The difficulty may apply to blue- and white-collar unions alike and arise from relative wage concerns – an increase for the young may trigger wage demands from older workers in order to keep the relative wages intact. To the extent that unions instead try to raise wages for *all* workers, regardless of age, the size of such wage increases is likely to be smaller than with an across-the-board payroll tax cut.

5. Data and empirical specification

The payroll data set has been obtained from the Confederation of Swedish Enterprise (*Svenskt Näringsliv*) and covers all member firms of the employer organisation the Swedish Trade Federation over the period 2000-2011. According to the website of the Federation, there are 13 000 member firms with a total of 300 000 employees, implying a coverage of about two thirds of all employees in Swedish retail. The firms are bound by the collective agreements signed by the Federation and these cover all employees, regardless of union membership.

In the data set, workers are observed once a year, in September. Thus, a worker is included in the data only if he or she worked in a member firm in retail during the month of September in a given year. The data are

⁸ Skedinger (2012a) documents an increase in the minimum wage relative to the average wage in retail during the period 1995-2010, from 75 to 81 per cent.

based on payroll records and include information on employee category (blue- or white-collar), various components of pay, actual and usual hours worked, gender, age, occupation, region and number of employees in the firm.

The payroll data set contains unique identifiers for firms and workers. The definition of accessions and separations follows standard procedures in the kind of data used here. An accession in year t is defined as the worker being present in the data in year t , but not in $t-1$, while the firm is present in both t and $t-1$ (but not necessarily in other periods). Accordingly, a non-accession in year t is defined as the worker being present in the data in both t and $t-1$, with the firm also being present in both periods. Observations for workers in year t for which the firm is not present in $t-1$ are assigned missing values for the accession variable. Since some, mostly small, firms may not report data in a given year for various reasons, even though they are still members of the Federation, this procedure ensures that the employees of non-reporting firms are not erroneously classified as entrants. Analogously, a separation in year $t+1$ is defined as the worker being present in the data in year t , but not in $t+1$, while the firm was present in both t and $t+1$. It is not possible to distinguish between voluntary and involuntary separations in the data. It should be noted that transitions between firms in the retail industry are not counted as accessions or separations, only those that involve a worker entering or leaving the industry. Given that involuntary separations cannot be identified, separations defined in this way capture relatively more exits into non-employment than a measure which includes intra-industry transitions. Similarly, the measure of accessions captures relatively more of transitions from non-employment to employment.

For comparability across samples, the computation of hours and wages is also conditioned on the presence of the firm in the data in two subsequent years. The measure of hours is based on *usual* hours per week (which could be part-time or full-time), not actual hours during the measurement period, in order to filter out disturbances specific to the reporting month. The data contain a direct measure of the *regular* hourly wage (*fast timlön*), which is likely to be measured with little error.⁹ The wage con-

⁹ A minority of blue-collar workers in the retail industry and most white-collars are salaried (see Table 1.a). For these workers, regular full-time monthly wages (*fast heltidsmånadslön*) have been transformed into regular hourly wages under the assumption of a 40-hour working week.

cept used thus excludes premiums for unsocial hours, overtime pay, bonuses and fringe benefits.

Minimum wages for blue-collar workers have been collected from the Retail Agreement (*Detaljhandelsavtalet*). Each blue-collar worker in the data set has been assigned a minimum wage, depending on the relevant personal characteristics, such as worker category, age and professional experience within the industry. This procedure was not performed for white-collar workers, since minimum wages are not binding for them.

A worker's attachment to the job is likely to influence mobility. There is unfortunately no direct information on the use of fixed-term contracts, which is widespread in the industry, but there is a variable in the data set indicating whether the worker is salaried. Salaried workers are typically less mobile, with more long-term attachment to the job.

Some observations have been excluded from the payroll data: (i) observations in municipalities within the regional support areas, subject to a different payroll tax regime (these observations were also excluded in the firm-level analysis of profits); (ii) observations for individuals with multiple jobs, due to a difficulty in defining the dependent variables; and (iii) in the wage regressions, observations with very low wages (below 75 per cent of the lowest minimum wage for blue-collar workers) in order to minimise the influence of measurement errors.¹⁰

For the purpose of analysing firms' profits, the payroll data set has been linked to the IFN Corporate Database, a data set containing accounting information. The information has been validated by the consulting firm PAR, based on original data from the Swedish Companies Registration Office (*Bolagsverket*), a government agency that records accounting information of limited liability corporations in Sweden. Not all firms in the payroll data set had the appropriate firm identifier for linking with the IFN dataset, so it was only possible to match a subset of firms (10 per cent of firm-year observations) in the payroll data.

The profit margin, basically profits (revenue minus costs) over revenue, is a standard measure in econometric analyses of profitability. I use two different variants of profit margins, before and after financial items, in the empirical analysis. The first measure (*rörelsemarginal*) is the profit

¹⁰ Exclusion (i) reduces the number of observations by around 4 per cent, (ii) by 1 per cent and (iii) by 7 per cent, depending on the specification. The total reduction is roughly 5 per cent in the employment and hours regressions and about 12 per cent in the wage regressions.

margin before (i) financial items, such as interest, capital gains and losses; (ii) non-recurring items; and (iii) company taxes (but, of course, *after* payroll taxes). The second measure (*vinstprocent*) is the profit margin before (ii) and (iii), but after (i). The two measures are intended to capture ‘normal’ operating profits, which may differ substantially from the bottom-line profits (*rörelseresultat*) also reported by firms. In line with common practice, two exclusions were performed. First, I discarded observations for firms with a financial year straddling the date of the first reform, July 1 2007. Second, a small number of outlier observations on profit margins, that is, values smaller than -1 or larger than 1, were excluded.¹¹ Other useful variables in the IFN Corporate Database include the firm’s payroll tax contributions and management salaries.

Figures 2-5 show the evolution of job accessions, separations, hours and wages in retail for the treatment group, 19-25-year-olds, in relation to 27-29-year-olds over the period 2000-2011.¹² Since the 26-year-olds were subject to treatment as a consequence of the second reform in 2009, they are not included in the comparison group. To shed some light on any differences within the treatment group, I have split it into two age groups, 19-20-year-olds and 21-25-year-olds. For white-collar workers, the younger treatment group contains few observations so only the older one is included in the figures.

Figure 2 shows accession rates for blue- and white-collar workers. The years before the payroll tax reforms in 2007 and 2009 are indicated by vertical lines. The figure for blue-collar workers reveals that the rates are considerably higher for the younger treatment group than for the older one and that cyclicalities are more pronounced among the 19-20-year-olds. For blue-collar workers, there is a downward trend in accession rates, reflecting the deteriorating labour market situation for the young during the recent decade. This is highlighted by the sharp decline in rates for all groups in 2009, with the onset of the financial crisis. Pre-reform trends before 2007 seem reasonably parallel for the older treatment group and the control, but the trends are quite different depending on worker category. In the first two years after the reform, there is an overall decline in accession

¹¹ The exclusions resulted in losses of observations of around 14 per cent (straddling financial year) and 0.5 per cent (outliers).

¹² For consistency with the eligibility requirements in the payroll tax reforms, age in the figures and in the econometric analysis is based on birth year, and not on actual age at the time of measurement.

rates, but among blue-collar workers, the decline is somewhat smaller in the treatment group. The drop in rates in 2009 is more accentuated in the younger treatment group among blue-collar workers. After 2009, the treatment groups seem to recover after the decline, while accession rates in the comparison group remain at a low level. The picture is somewhat different among white-collar workers, as the decline in rates is sharper in the treatment group than in the comparison group during 2007-2009, but the recovery in the ensuing period is stronger.

Separation rates in retail are presented in Figure 3. Pre-reform trends do not appear as parallel as was the case for accessions. It is of some interest to note that separations did not increase in connection with the financial crisis initiated in 2009. On the contrary, exits *declined* during this year (note that separations refer to year $t+1$ in Figure 3, so the observation for 2008 indicates whether the individual was separated in 2009). The brunt of adjustment during the crisis thus fell on the new recruits in retail, rather than on the young people already employed there.

Figure 4 shows the evolution of weekly hours. Once more, the 19-20-year-olds exhibit a high cyclical volatility relative to the other age groups. Post-reform development for blue-collar workers does not suggest that hours increased in the treatment groups relative to the control. Among white-collar workers, pre-reform trends diverge sharply. Whereas hours in the control group remained stable (and were close to full-time work on average), hours declined in the treatment group. A partial recovery occurred just before the reform, in 2006, and continued up until 2008. In 2009, there was a decline in hours among both groups, especially among the treated. The relative decline continued into 2010-2011.

Table 1. Descriptive statistics**a. Individual-level data, 2004-2011**

Variable	Blue-collar		White-collar	
	19-25	27-29	19-25	27-29
Accession rate	0.422	0.281	0.473	0.271
Separation rate	0.385	0.340	0.358	0.250
Weekly hours	23.1	28.6	35.9	38.7
Real hourly wage (SEK)	100.9	110.5	121.8	155.7
Age	21.9	27.9	23.1	28.1
Male	0.331	0.371	0.421	0.464
Sales work	0.832	0.822	0.179	0.085
Salaried	0.154	0.342	0.809	0.973
Close to minimum wage: At most 5 % above	0.657	0.467	-	-
Close to minimum wage: At most 1 % above	0.387	0.202	-	-
Metropolitan counties	0.601	0.598	0.623	0.645
Forest counties	0.108	0.116	0.111	0.074
Other counties	0.291	0.286	0.266	0.281
Year 2004	0.094	0.092	0.094	0.103
Year 2005	0.107	0.104	0.102	0.109
Year 2006	0.119	0.117	0.113	0.116
Year 2007	0.124	0.119	0.118	0.117
Year 2008	0.135	0.130	0.133	0.134
Year 2009	0.136	0.139	0.132	0.140
Year 2010	0.138	0.146	0.140	0.136
Year 2011	0.147	0.152	0.166	0.145
No. of employees in firm	957.9	1098.1	819.7	808.6
No. of obs.	213 536	52 168	19 688	26 896

Source: Own calculations.

Table 1. Continued....**b. Firm-level data, 2004-2010**

Variable	Matched firms	Non-matched firms
Profit margin, before financial items	0.032	-
Profit margin, after financial items	0.039	-
Payroll tax contributions relative to wage bill	0.289	-
Managerial pay relative to wage bill	0.037	-
Pre-reform share of workers aged 19-25	0.221	0.242
Share of blue-collar workers	0.491	0.727
Metropolitan counties	0.681	0.547
Forest counties	0.055	0.120
Other counties	0.263	0.334
Year 2004	0.120	0.167
Year 2005	0.177	0.177
Year 2006	0.185	0.170
Year 2007	0.027	0.136
Year 2008	0.159	0.125
Year 2009	0.178	0.116
Year 2010	0.154	0.110
No. of employees	144.0	21.4
No. of firms	354	2,996
No. of obs.	1 229	11 308

Source: Own calculations.

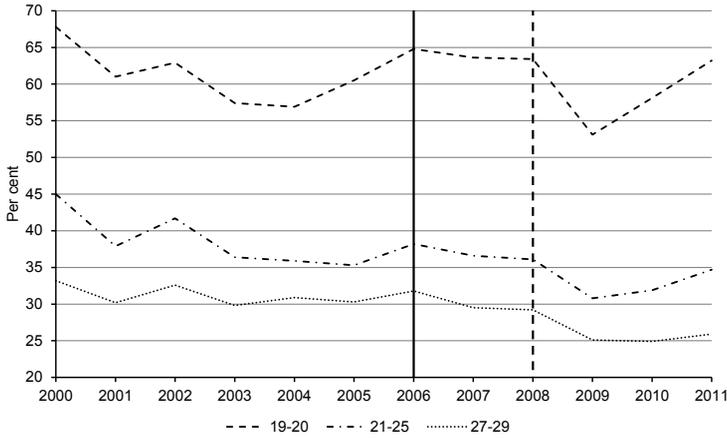
Over the period 2000-2011 there were increases in real hourly wages in the retail industry, as evidenced in Figure 5. Among blue-collar workers, wages rose by 30 per cent for those aged 19-20, by 25 per cent for the 21-25 age group and by 21 per cent for the 27-29-year-olds. Since wages increased faster among the youngest, the figures also imply wage compression across the three age groups, which is consistent with the rising minimum wages in relation to median wages in the industry that have been documented in Skedinger (2013). Real wages increased more in percentage terms after the first reform than before, for all age groups and even continued to increase at the onset of the financial crisis in 2009. In the wake of the crisis, wages remained rather stable, with a small decrease in 2011. There was no wage compression across age groups among white-collar workers, although wages increased overall over the period. In con-

nection with the crisis, white-collar workers exhibited more of wage moderation than blue-collar workers, which suggests important differences in wage formation between the two worker categories.

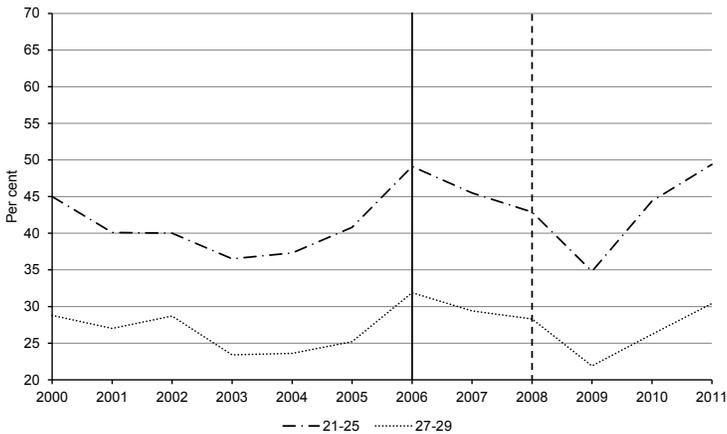
Descriptive statistics in the individual-level sample of 19-25-year-olds and 27-29-year-olds are shown in Table 1.a. The table confirms that there are considerable differences between blue- and white-collar workers. For example, the former tend to be younger and have a smaller proportion of males (although males form the minority also among white-collar workers). Moreover, blue-collar workers are bound by minimum wages, which is not the case for white-collar workers. About 65 per cent among the youngest blue-collar workers have a wage that is at most 5 per cent above the minimum wage that is relevant for that individual (dependent on age and experience in the industry). For the older age group, the corresponding figure is 47 per cent. Almost 40 per cent of the younger and 20 per cent of the older workers have a wage that is at most 1 per cent above the minimum wage. Table 1.b presents descriptive statistics for the firm data, separately for matched and non-matched firms. The profit margins in matched firms are 3-4 per cent on average, depending on the definition. The subset of matched firms merely represents 10 per cent of all firm-year observations in the data, but accounts for 42 per cent of total employment, since the firms are relatively large on average. The matched and non-matched firms are similar in the pre-reform intensity of using workers aged 19-25 in their workforces, with shares on average amounting to 22 and 24 per cent, respectively.

Figure 2. Accession rates in the retail industry, by age group, 2000-2011. Per cent

a. Blue-collar workers



b. White-collar workers

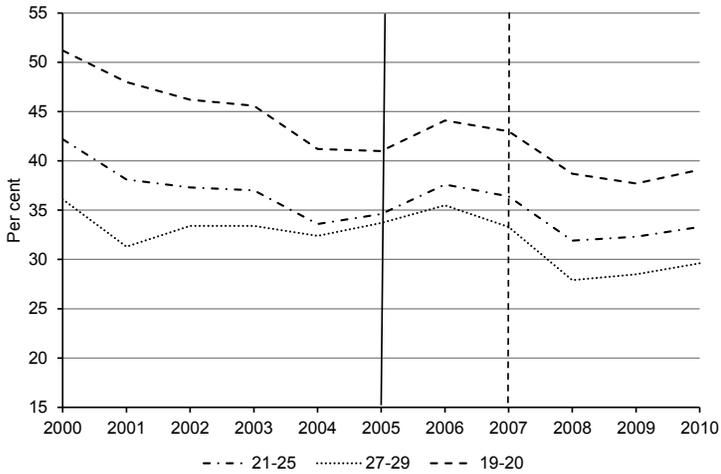


Source: Own calculations.

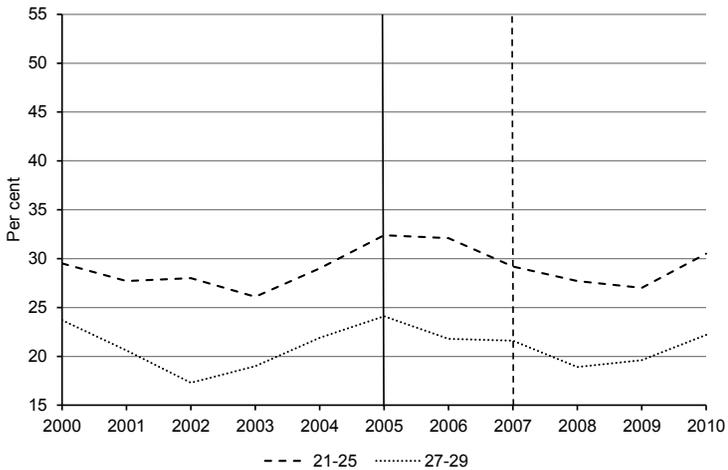
Notes: Accession rates refer to year t . The year before a payroll tax reform is indicated by a vertical line.

Figure 3. Separation rates in the retail industry, by age group, 2000-2010. Per cent

a. Blue-collar workers



b. White-collar workers

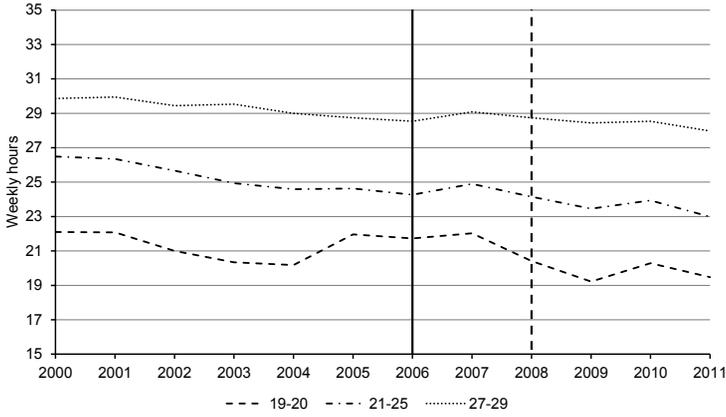


Source: Own calculations.

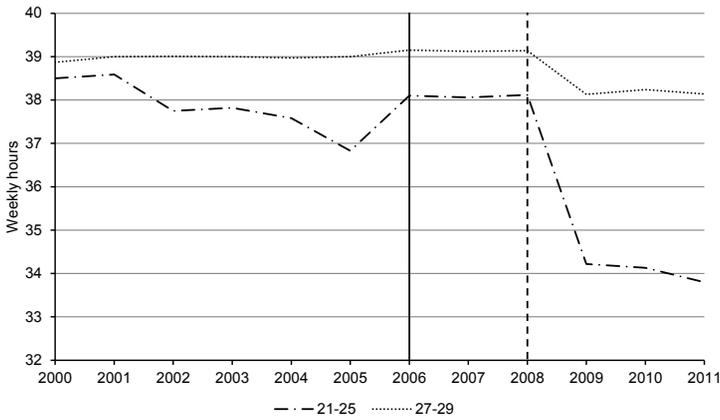
Notes: Separation rates refer to year t+1. Two years before a payroll tax reform is indicated by a vertical line.

Figure 4. Weekly hours in the retail industry, by age group, 2000-2011

a. Blue-collar workers



b. White-collar workers

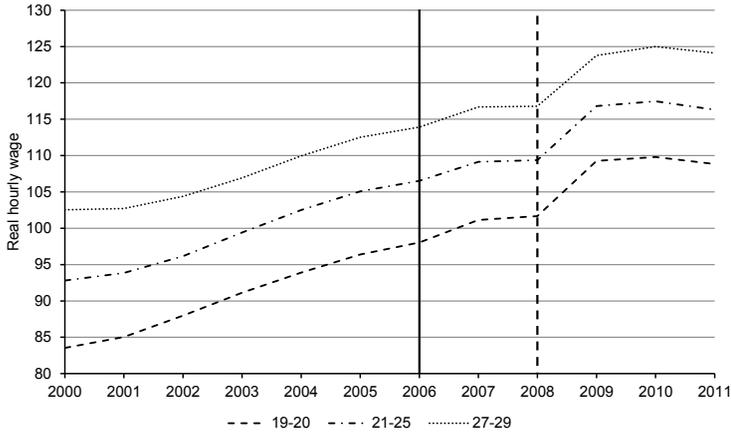


Source: Own calculations.

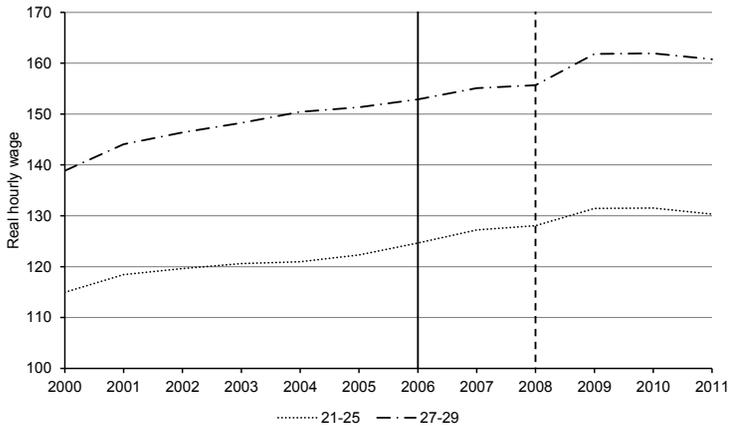
Notes: The year before a payroll tax reform is indicated by a vertical line.

Figure 5. Real hourly wage in the retail industry, by age group, 2000-2011. SEK

a. Blue-collar workers



b. White-collar workers



Source: Own calculations.

Notes: 2011 prices. The year before a payroll tax reform is indicated by a vertical line.

The empirical strategy in the analysis of worker outcomes is to use a difference-in-difference (d-i-d) approach to compare changes in the variables (accessions, separations, hours and hourly wages) before and after the changes in payroll taxes. The high cyclicality of workers aged 19-20, revealed in the previous figures, makes it problematic to use them as a

treatment group in the empirical analysis of the payroll tax reforms. I have chosen to instead use workers aged 21-25 as the benchmark treatment group, with 27-29-year-olds as the control, but also experiment with both larger and more narrowly defined treatment and control groups in terms of age.

Based on the data on individuals and firms, I estimate the following linear regression for worker outcomes:

$$Y_{it} = \alpha_0 + \alpha_1 (\text{Treated_Age_Group})_{it} + \alpha_2 \text{Post}_t + \alpha_3 (\text{Treated_Age_Group} * \text{Post})_{it} + x'_{it} \beta + \varepsilon_{it}, \quad (1)$$

where subscripts i and t represent the individual worker and time, respectively. In the analyses of accessions, the dependent variable is equal to one if a worker is newly hired in the industry at time t and zero otherwise. In regressions on separation behaviour, the dependent variable equals one if an individual is separating from the industry at time $t+1$ and zero otherwise. *Treated_Age_Group* is a dummy variable for belonging to the treated age group at time t , *Post* is a dummy variable for the post-reform period and *Treated_Age_Group*Post* is an interaction term between *Treated_Age_Group* and *Post*. The coefficient for the interaction term is the d-i-d estimate of the reform effect, reflecting the differential effect on the age group affected by the change in payroll taxes. The d-i-d estimator allows for both group-specific and time-specific effects.

Furthermore, x_{it} is a vector of individual characteristics, namely dummies for gender, age, region, occupation, salaried position and year (which controls for common shocks to the business cycle). The additional explanatory variables account for the possibility that characteristics are systematically different between the age groups before and after the policy change (compositional bias), but should not be affected by the treatment.

Using the appropriate treatment and control groups is a key issue in identification. Egebark and Kaunitz (2013) contain a useful discussion of this issue in the context of the payroll tax reforms under study. The ideal control group should be as similar to the treatment group as possible, but should not be affected by the treatment. The usual approach in evaluations of policies targeted towards young workers is to use slightly older workers as a control.

First, there is the well-known argument that if employers substitute young workers for slightly older ones in response to the payroll tax cut, estimates of the treatment effect will be biased upwards due to a *substitution effect*. The magnitude of this effect will depend on the extent to which employers regard workers of different ages in the two age groups as close substitutes in production. From a policy viewpoint, some substitution may be acceptable as long as employment in the targeted group increases, but the fact remains that estimates of the reform will be distorted.

Second, the reduction in the cost of a factor of production also results in a *scale effect*. Under plausible assumptions, this effect implies an expansion of output, which could potentially result in the employers hiring more of older and more productive workers than of younger workers. If this is the case, the scale effect counteracts the substitution effect. A scale effect of this type could be more likely in firms already employing a large share of young workers, but it is probably small in relation to the substitution effect.

Third, as pointed out by Egebark and Kaunitz (2013), treatment is not uniform across age groups within the treatment group. On the one hand, a younger worker is subject to treatment over a longer period than an older worker, which increases the incentives to hire the former instead of the latter in the presence of fixed costs of recruiting a new worker, due to hiring and training costs. On the other hand, it is a stylised fact that quits are relatively more common among younger workers, which strengthens the incentives to hire older workers within the treatment group. In general, the expected present value to the employer of the payroll tax reduction will be larger for younger workers, unless the quit rates among them are not too high. Figure 6 illustrates separation rates (from the firm, not the industry) by age at the time of hiring and tenure before the payroll tax reforms, as an average over the period 1998-2005. Separation rates are consistently higher among workers with shorter tenure and among blue-collar workers. Among the latter, separation rates decline more steeply with tenure for younger workers than what is the case for older ones. This implies, for example, that younger workers with at least three years' tenure in most cases exhibit lower separation rates than older workers with the same tenure. Figure 7 attempts to describe how these differences in separation rates impinge on the expected present value of receiving the

payroll tax subsidy (set to unity for simplicity) at the time a worker is hired, depending on the age of the worker.¹³ The age profiles reveal that the expected present value of the payroll tax subsidy reaches its maximum for 20-year-olds among blue-collar workers and for 22-year-olds among white-collar workers. The present values then diminish for older workers among both worker categories, especially so for 25-year-olds. Taken at face value, these calculations imply that 25-year-olds have been subject to 58 and 54 per cent less treatment, respectively, relative to the age group treated most intensively among blue- and white-collar workers.

When examining the effects on wages of the payroll tax reforms, it may be the case that wages for *all* workers are driven up, subject or not subject to tax reductions. Then, it is only possible to capture the relative wage effect with the methodology used in this paper.

A somewhat different approach than in (1) is used in the examination of profits. Since the vast majority of all firms are treated, in the sense of employing at least one young worker, there is no suitable control group. The characteristics of the data thus limit the usefulness of d-i-d but allow an analysis exploiting the variation in treatment intensity across firms, as in the linear specification below:

$$Y_{it} = \alpha_0 + \alpha_1 (\text{Pre-reform_Treatment_Intensity})_j + \alpha_2 \text{Post}_t + \alpha_3 (\text{Pre-reform_Treatment_Intensity} * \text{Post})_{jt} + z'_{jt} \delta + \varepsilon_{jt}, \quad (2)$$

with subscript j referring to the firm. The variable *Pre-reform_Treatment_Intensity* is the percentage of workers aged 19-25 in the firm, calculated as an average over the pre-reform period 2004-2006. The reform effect, namely the extent to which pre-reform treatment intensity is reflected in higher profits after the reform, is captured by the coefficient of the interaction *Pre-reform_Treatment_Intensity * Post*, although its interpretation is less straightforward than in the d-i-d approach.

¹³ The expected present value is calculated according to the formula

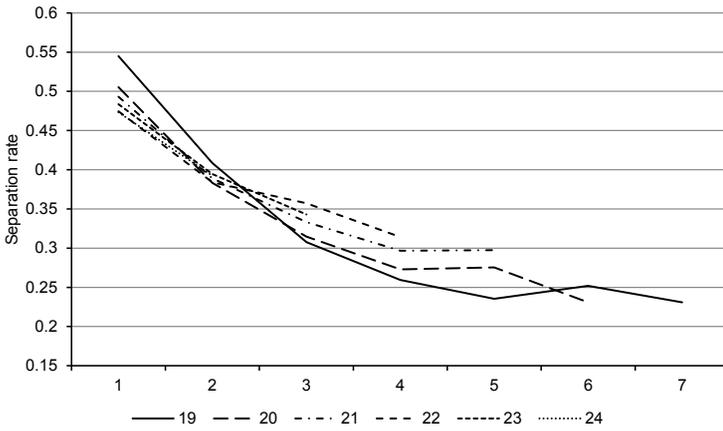
$$EPV_{j,s} = S \sum_{k=1}^7 \beta^k \prod_{i=1}^k (1 - \delta_{i,j,s})$$

where index j represents type of worker (blue-collar or white-collar), s represents age at the time of hiring, S is the payroll tax reduction, set to unity, β is the discount factor, set to 0.95 and δ is the separation rate. The sum is calculated for tenures of length i , between 1 and 7 years, depending on the age at the time of hiring. By using historically observed separation rates in the calculations, it is assumed that relative separation rates across treated age groups are not affected by the payroll tax reduction.

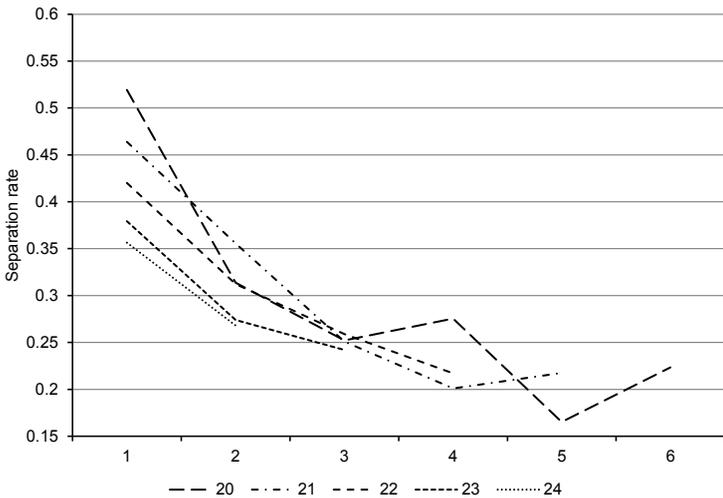
Firm characteristics include the share of blue-collar workers, to account for the skill-intensity of the firm, and dummies for region and year.

Figure 6. Separation rates by age at hiring, conditional on tenure. 1998-2005

a. Blue-collar workers



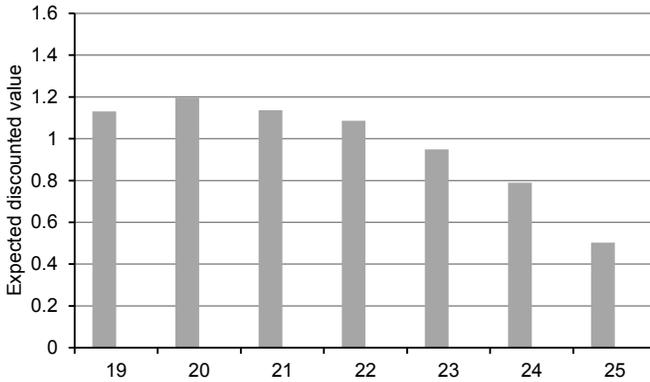
b. White-collar workers



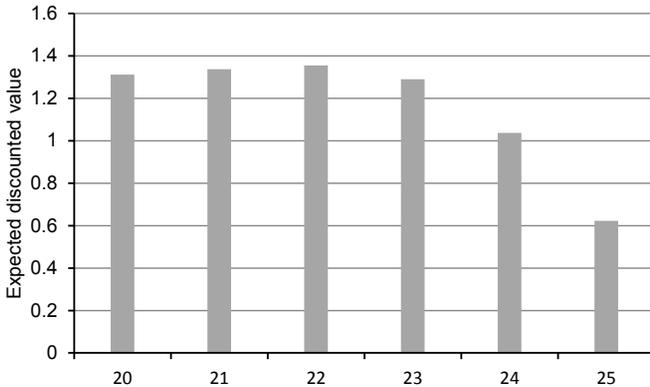
Source: Own calculations.

Figure 7. Expected discounted value of payroll tax cut, by age

a. Blue-collar workers



b. White-collar workers



Source: Own calculations.

Notes: See text for details about the calculations.

6. Econometric results

This section is divided into two parts. The first part contains regressions at the individual level, dealing with effects on job accessions, separations, hours and wages, while the firm-level analysis in the second part is con-

cerned with profits, managerial pay and payroll tax contributions. The before-period is 2004-2006 throughout the estimations in this section. Using 2004 as the starting year is suitable since the occupational codes changed during that year.

6.1 Employment and wages

The after-period is prolonged successively by one year, so the first regression refers to the estimation period 2004-2007, the second to 2004-2008, and so on up to 2011. Due to the differences in wage formation between blue- and white-collar workers, separate regressions will be run for the two groups. T-statistics have been clustered at the firm level, which is the most conservative alternative.

When interpreting the potential employment effects of the accession and separation variables, it is important to keep the following in mind. On the one hand, it is not unlikely that young entrants into the industry transit from non-employment to employment to a larger extent than slightly older entrants. On the other hand, separations to non-employment may well be relatively more prevalent among the treated. To the extent that such asymmetries are time-invariant, the d-i-d approach ensures that the estimated effects on entry and exit will not be distorted. However, if the treated are more cyclically sensitive than the control, the estimates may be biased. The upshot of this is that estimates for the period including the financial crisis (from 2009) are more problematic to interpret than estimates for preceding years.

To save space, only the estimate of the most relevant variable, the d-i-d estimator (*Treated_Age_Group*Post*), is presented (full regressions are available from the author upon request). Table 2 shows regressions with the benchmark, 21-25-year-olds, as the treated age group and 27-29-year-olds as the control. The first column refers to entry into the industry (job accessions in year t), the second to exit from the industry (job separations in year $t+1$). Concerning hours and wages, it seems useful to distinguish between effects for new hires and effects for incumbent workers, with the superscripts *new* and *inc*, respectively, since the effects are not necessarily identical. Thus, the third column refers to the log of weekly hours among new recruits in year t , the fourth to the log of weekly hours among incumbent workers in year t (who were employed both in t and $t-$

I), the fifth to the log of hourly wages among new recruits in year *t* and, finally, the sixth column refers to the log of hourly wages among workers in year *t* (who were employed both in *t* and *t-1*).

Table 2. Effects on employment, hours and wages. Treated age group: 21-25. Control age group: 27-29

	P(Entry)	P(Exit)	ln H ^{new}	ln H ^{inc}	ln W ^{new}	ln W ^{inc}
Blue-collar						
2004-2007	0.017* (1.80) [89 989]	0.002 (0.28) [87 273]	0.001 (0.04) [31 081]	0.009 (0.48) [57 816]	0.003 (0.94) [28 375]	0.001 (0.35) [53 130]
2004-2008	0.015* (1.82) [116 579]	0.008 (1.15) [112 948]	-0.015 (0.46) [40 112]	-0.012 (0.68) [75 076]	0.004 (1.32) [36 772]	0.001 (0.29) [69 200]
2004-2009	0.010 (1.32) [144 209]	0.012** (2.04) [139 948]	-0.031 (0.94) [48 131]	-0.028 (1.59) [94 392]	0.006** (2.29) [44 192]	0.003 (1.36) [87 252]
2004-2010	0.011 (1.57) [173 104]	0.014** (2.26) [168 092]	-0.026 (0.85) [56 698]	-0.030 (1.64) [114 432]	0.007*** (2.65) [51 943]	0.004* (1.71) [105 925]
2004-2011	0.015** (2.23) [204 045]	0.015*** (2.59) [197 846]	-0.029 (1.04) [66 606]	-0.034* (1.79) [135 067]	0.006** (2.51) [61 171]	0.004* (1.69) [125 222]
White-collar						
2004-2007	0.015 (0.73) [19 334]	0.031* (1.82) [19 781]	0.010 (0.34) [6 453]	0.023** (2.16) [12 716]	0.008 (0.73) [6 282]	0.018** (2.31) [12 604]
2004-2008	0.009 (0.57) [25 245]	0.019 (1.42) [25 424]	0.002 (0.08) [8 452]	0.024*** (2.73) [16 593]	0.005 (0.50) [8 239]	0.016** (2.01) [16 448]
2004-2009	-0.006 (0.40) [31 395]	0.018 (1.46) [32 069]	-0.033 (0.90) [10 093]	-0.006 (0.32) [21 049]	0.005 (0.46) [9 857]	0.019** (2.25) [20 872]
2004-2010	-0.000 (0.02) [37 508]	0.013 (1.04) [38 059]	-0.025 (0.62) [12 123]	-0.016 (0.65) [25 074]	0.012 (1.16) [11 864]	0.021** (2.21) [24 876]
2004-2011	0.004 (0.29) [44 309]	0.008 (0.72) [44 278]	-0.056 (1.16) [14 737]	-0.017 (0.67) [29 227]	0.014 (1.28) [14 327]	0.023** (2.40) [29 005]

Source: Own calculations.

Notes: Only the estimated reform effects in the OLS regressions are shown. In the regressions for exits, the estimation periods are 2003-2006, 2003-2007 and so on until 2003-2010. The regressions include dummies for the treated group, the post period, gender, age, occupation, salaried position, region and year. For hours and wages, different regressions are performed for new hires and incumbent workers. Absolute, robust t-statistics, clustered at the firm level, within parentheses. Number of observations within brackets. * denotes significance at 10%, ** significance at 5%, *** significance at 1%.

The estimates for blue-collar workers, in the upper panel, indicate modest effects on the probability of entry into the retail industry, regardless of the time period considered. The short-run estimate for 2007 is 0.017, which is significant only at the 10 per cent level. This implies that the probability of job accession increased by 1.7 percentage points in the treated group relative to the control. For the longest observation period, 2004-2011, the coefficient indicates that the probability of entry increased by 1.5 percentage points. The coefficients for the probability of exit are also small, but are increasing over time and reach 0.015 by 2011. A rough estimate of the short-term increase in net employment is $1.7 - 0.2 = 1.5$ per cent and zero in the long run.

The results in the third and fourth columns do not suggest that the reforms were associated with more hours worked, either among new recruits or incumbents. On the contrary, the estimates are mostly negative, although predominantly insignificantly so. The short-run effect for 2007 indicates no effect at all on hours. There is some evidence in the fifth and sixth columns of increasing wage pressure over time, but the effects are small. In the short run, the estimated effect is close to zero and for the longest observation period it is around 0.5 per cent.

The combined results in Table 2 for blue-collar workers imply a short-run elasticity with regard to total wage costs of about -0.19.¹⁴ The magnitude of this elasticity is within the range of those estimated for all industries by Egebark and Kaunitz (2013) for the more narrowly defined treatment group of 25-year-olds (-0.14) and the wider group aged 19-25 (-0.30). Thus, consistent with the intentions behind the reforms, there seems to be an increase in employment in retail among the young, but the effects are not persistent and small in relation to the sizeable reductions of payroll taxes that were implemented.

The smaller samples for white-collar workers, in the bottom panel of Table 2, make the estimates less precise. The results are hardly more encouraging, though. There is no discernible positive effect on net employment even in the short term. Hours among incumbent workers increased in the first few years after the 2007 reform, but this result must be viewed with some skepticism due to the non-parallel pre-reform trends

¹⁴ The elasticity, which is based on a zero wage effect and includes the union-negotiated payroll fee for blue-collar workers, is calculated as $0.015 / [-11.1 / (132.42 + 7.1)] = -0.19$. Unlike conventional elasticities, it is conditional on employment.

exhibited in Figure 4. For incumbent workers, there is also a notable increase in wages already in the short run, which is consistent with more decentralised wage setting among white-collar workers than among blue-collar workers.

In 2009, the financial crisis erupted and this seems to have had repercussions on the estimated reform effects in Table 2. The probability of entry and hours of work are both reduced in the estimates for 2004-2009, compared to estimates for the previous period, while there is no sign of more wage restraint. The additional payroll tax cut implemented in 2009 may have mitigated the decline of labour market prospects for young workers during the crisis but did certainly not eliminate it. However, the estimates of the reform effects including the period 2009-2011 may be less reliable than the estimates for previous periods, and not only because of the financial crisis. During 2009-2011, the control group includes some previously treated workers, namely those aged 23-25 in 2007. For example, a 25-year-old treated in 2007 is included in the control 2009-2011 as a 27-29-year old, while a 23-year-old in 2007, part of the treatment group 2007-2009, turns up in the control in 2011, aged 27. To the extent that previous treatment affects subsequent labour market outcomes, the estimated reform effects for 2009-2011 may thus be distorted.

The results in Table 2 were subject to several robustness checks, not reported in full due to space constraints (but available from the author). First, I estimated the same specifications with a panel of firms and firm fixed effects. I also tried specifications without the control variables for region, occupation and salaried position, in response to the concern that these variables may be endogenous. As a check on cyclicalities, I added the output gap, that is, the difference between actual GDP and an estimate of potential GDP, interacted with the age dummies, since the employment of young workers is typically more volatile over the business cycle than the employment of older workers.¹⁵ Estimations excluding the year 2007 were also tried. Finally, specifications including workers in regional support areas were estimated. These exercises did not alter the conclusions from Table 2 in any substantial way.¹⁶

¹⁵ The source for the data on the output gap is the National Institute of Economic Research (*Konjunkturinstitutet*).

¹⁶ With the output gap included, the effect on hours turns positively significant in some estimations for blue-collar workers (new recruits 2004-2007).

Table 3. Effects on employment, 2004-2007. Various age groups and subgroups

	All		Bound by minimum wage		In small firm	
	P(Entry)	P(Exit)	P(Entry)	P(Exit)	P(Entry)	P(Exit)
T: 21-25, C:27-29						
Blue-collar	0.017* (1.80) [89 989]	0.002 (0.28) [87 273]	0.033** (2.33) [43 377]	0.028** (2.39) [39 129]	0.015 (0.88) [19 381]	0.019 (1.12) [19 785]
White-collar	0.015 (0.73) [19 334]	0.031* (1.82) [19 781]			0.013 (0.35) [4 187]	0.065* (1.91) [4 624]
T: 19-25, C:26-29						
Blue-collar	0.022** (2.57) [127 342]	-0.002 (0.32) [124 131]	0.030*** (2.61) [69 330]	0.017* (1.65) [63 612]	0.006 (0.38) [28 938]	0.001 (0.07) [29 651]
White-collar	0.016 (0.90) [23 518]	0.037** (2.32) [23 934]			0.004 (0.11) [5 101]	0.064** (2.02) [5 560]
T: 22-25, C:26-28						
Blue-collar	0.012 (1.43) [78 819]	0.002 (0.29) [76 067]	0.028** (2.27) [36 258]	0.019 (1.63) [32 403]	-0.004 (0.25) [16 471]	-0.012 (0.67) [16 776]
White-collar	0.014 (0.74) [17 359]	0.021 (1.15) [17 656]			0.010 (0.24) [3 739]	0.035 (0.94) [4 103]
T: 25, C:26						
Blue-collar	-0.001 (0.07) [21 363]	-0.008 (0.60) [20 562]	-0.009 (0.45) [9 309]	-0.004 (0.18) [8 184]	-0.012 (0.36) [4 338]	-0.094*** (2.77) [4 396]
White-collar	0.011 (0.38) [5 596]	0.007 (0.26) [5 655]				

Source: Own calculations.

Notes: T denotes treatment and C control. There were too few observations for T:25, C:26, white-collar in small firms. See also note to Table 2.

Table 3 presents a number of experiments with extending and narrowing the treatment and control groups in terms of age and their impact on entry and exit. The table also tests for heterogeneous treatment effects

depending on the position in the wage distribution and firm size. Since it can be argued that the most credible estimates pertain to 2004-2007 – no previously treated workers are included in the control group and the crisis had not yet gained momentum – I restrict the estimates to this period and I am then also able to include 26-year-olds in the control group since they were not treated until 2009. The first rows repeat the analysis for the benchmark group in Table 2 and the others show results for the following age groups in the treatment and control: 19-25 versus 26-29, 22-25 versus 26-28 and 25 versus 26.

Two patterns specifically relating to blue-collar workers emerge in Table 3. The first is that the impact on entry is larger, the wider the definition of the included age groups; the estimates vary between 0.022 (19-25 versus 26-29) and -0.001 (25 versus 26) and the latter is not significant. It is not obvious how one should interpret these results. It is difficult to ascertain whether the difference is due to the reform or because the youngest workers did better anyway because of their being more responsive to an improving labour market in 2007. On the one hand, to the extent that closer age groups are more comparable in unobserved productivity characteristics, one may regard the results for these groups as yielding a more reliable identification of the effects of the payroll tax reform. On the other hand, the smaller effects are consistent with the relatively lower expected discounted value of the tax reduction for older workers in the treatment group, indicated in Figure 7. If substitution across age groups is important, we would expect larger effects in the more restricted samples, but that is apparently not the case.

The second pattern is that workers close to the minimum wage – with a wage up to 5 per cent above the minimum – seem to be affected more than other workers. The estimates for entry turn out to be around 0.030, with the exception of the most narrowly defined group for which there is no significant effect. However, the effect on entry must still be considered to be modest and net employment is not necessarily more favourably affected, since exits are also more prevalent than in the full samples. For white-collar workers, there is little evidence of a differential impact on entry across age groups (and minimum wages are not relevant as white-collar workers are subject to substantially lower, and non-binding rates, see Skedinger, 2013). The results for white-collar workers also indicate that the (positive) effect on exits disappears with more narrow age groups. Endogenei-

ty of minimum wages is a concern that was discussed in Section 3, but since minimum wages are never negotiated at the firm level, it seems reasonable to assume that firms take minimum rates as given when deciding on hirings and firings.

Table 3 also looks at workers in small firms, namely those with 50 employees or less. Somewhat surprisingly, neither entry nor exit seems to have been much affected by the 2007 reform (a conclusion which is robust to using lower thresholds to define small firms). It is conceivable that non-take-up is more prevalent among small firms, but it seems unlikely that this is the whole explanation.

One method to check for parallel trends, a crucial assumption behind the d-i-d estimator, is to use placebo periods. By using data on prior periods, the d-i-d regressions can be re-estimated by studying the years during which there were no payroll policy changes. If the placebo estimators are statistically significant, there is a risk that the estimated d-i-d coefficients are biased. As a check for robustness, a large number of different placebo regressions for entry and exit have been estimated for the age groups in Table 3. Placebo reforms for the years 2004, 2005 and 2006 are presented in Table 4. For blue-collar workers in the regressions with 19-25-year-olds and 21-25-year-olds as treatment groups, there is evidence of ‘pre-treatment’, which casts some doubt on the regressions for these groups in Tables 2 and 3. However, there is much less or no such evidence in the placebo regressions using those aged 22-25 and 25 as the treatment. In an attempt to make the treatment and controls more similar, I experimented with using various restrictions on weekly hours – at least 10 hours, 20 hours and 35 hours – in the regressions with 19-25-year-olds and 21-25-year-olds as controls, but this had little effect on the placebo estimates. Based on the exercises in Table 4, I conclude that my estimates should be viewed as *at best* indicating modest effects on entry and exit following the 2007 reform.

Table 4. Placebo tests. Effects on employment, $t-3 - t$

Placebo reform, t=2004		Placebo reform, t=2005		Placebo reform, t=2006	
P(Entry)	P(Exit)	P(Entry)	P(Exit)	P(Entry)	P(Exit)
T: 21-25, C:27-29					
Blue-collars					
-0.030*** (2.94) [73 870]	-0.023** (2.11) [71 093]	-0.024** (2.50) [78 884]	-0.037*** (4.42) [74 506]	0.009 (0.96) [84 145]	-0.021** (2.26) [80 428]
White-collars					
0.001 (0.07) [18 517]	-0.010 (0.66) [19 205]	0.016 (0.92) [18 537]	-0.032** (2.08) [19 419]	0.031 (1.58) [18 789]	-0.014 (0.75) [19 598]
T: 19-25, C:26-29					
Blue-collars					
-0.020** (2.19) [107 466]	-0.017* (1.74) [104 914]	0.000 (0.00) [113 175]	-0.040*** (5.32) [108 330]	0.020** (2.02) [119 565]	-0.019** (2.22) [115 417]
White-collars					
-0.009 (0.50) [22 121]	-0.011 (0.80) [22 993]	0.014 (0.86) [22 208]	-0.031* (1.88) [23 231]	0.036** (2.02) [22 754]	-0.019 (1.03) [23 508]
T: 22-25, C:26-28					
Blue-collars					
-0.015 (1.44) [63 028]	-0.001 (0.09) [60 554]	-0.009 (1.00) [68 000]	-0.036*** (4.14) [63 589]	-0.000 (0.02) [73 259]	-0.017 (1.64) [69 422]
White-collars					
-0.014 (0.75) [16 320]	-0.009 (0.61) [17 009]	0.026 (1.52) [16 381]	-0.042** (2.50) [17 161]	0.034* (1.71) [16 776]	-0.015 (0.84) [17 358]
T: 25, C:26					
Blue-collars					
0.013 (0.77) [16 543]	0.005 (0.36) [15 854]	0.013 (0.90) [18 151]	-0.018 (1.06) [16 687]	-0.002 (0.17) [19 818]	0.014 (0.93) [18 552]
White-collars					
-0.029 (0.94) [5 071]	0.006 (0.21) [5 248]	0.033 (1.24) [5 200]	-0.050* (1.77) [5 293]	0.028 (1.01) [5 420]	-0.024 (0.84) [5 478]

Source: Own calculations.

Notes: See notes to Tables 2 and 3.

6.2 Payroll tax contributions, managerial pay and profits

In this section, the after-period is the entire period of 2007-2010, and it is not prolonged successively by one year, since profits typically contain a great deal of idiosyncratic year-to-year variation. As in the previous section, only the estimate of the reform effect is shown. There are two model specifications, one without and one with firm-fixed effects.

Table 5. Effects on firms' payroll tax contributions, management salaries and profits, 2004-2010

Payroll taxes		Management pay		Profit margin, before financial items		Profit margin,after financial items	
w/o FE	FE	w/o FE	FE	w/o FE	FE	w/o FE	FE
-0.038***	-0.045***	0.012	-0.013	0.050**	0.036*	0.050**	0.029
(4.23)	(6.34)	(0.98)	(1.17)	(2.38)	(1.72)	(2.29)	(1.63)
[1 221]	[787]	[1 221]	[787]	[1 229]	[792]	[1 225]	[789]
{354}	{196}	{354}	{196}	{354}	{196}	{354}	{196}

Source: Own calculations.

Notes: Only the estimated reform effects, defined as the pre-reform percentage of 19-25-year olds in the firm interacted with a dummy for the post-reform period, are shown. The models also include a dummy for the post-reform period, the pre-reform share of 19-25-year olds in the firm, the share of blue-collar workers and dummies for year and region. Payroll tax contributions include pension costs and are measured as a percentage of total wage and pension costs. Management pay includes performance pay and is measured as a share of the pay of all employees including performance pay. Profit margin is profits, before taxes and non-recurring items, divided by sales. The end of the financial year corresponds to the year used for other variables in the regressions. The models are estimated without and with firm-fixed effects (FE). The time-invariant pre-reform share of 19-25-year olds is excluded in the fixed-effects regressions. Absolute, robust t-statistics, clustered at the firm level, within parentheses. Number of observations within square brackets, number of firms within curly brackets. See also notes to Table 2.

Table 5 displays results for payroll tax contributions, managerial pay and profits. To begin with, it is necessary to demonstrate that payroll tax contributions actually decreased more in those firms employing a relatively large number of young workers before the reform in 2007. This procedure serves as a check on the quality of the data and also indicates whether non-take-up is a serious problem. To this end, the first columns in Table 5 show regressions with the firm's payroll tax contributions including pension costs, measured as a percentage of total wage and pension costs, as the dependent variable. The evidence provided in the table is clear: The larger the share of young workers in the firm before the reform, the smaller the payroll tax contributions in relation to the wage bill after the reform. The estimates are robust across specifications and signif-

icant throughout, suggesting that a firm with a one percentage point larger share of 19-25-year-olds than another firm experienced a decrease in the share of contributions by 0.038 to 0.045 percentage points, relative to the other firm.

The regressions for the share of managerial pay, including performance pay, in the wage bill do not indicate that the salaries of managers increased following the reform, over and above any increases of wages for non-managerial staff. The measure of managerial pay is crude, though, since the number of employees among managers and other staff is not taken into account.

Turning to the effects on profit margins in the final columns of Table 5, it should be recalled that two different measures are used, namely before and after financial items. The two variants of profit margins yield similar results. The estimates are always positive, but only borderline significant in one case with fixed effects. It is conceivable that firm-specific factors, such as location, are important for profit margins in retail and these factors may also be related to the intensity of using young workers. With fixed effects, the magnitudes of the estimates are reduced, from 0.050 to 0.029 and 0.036, depending on the specification. Accordingly, a firm with a one percentage point higher pre-reform share of young workers than that in another firm increased its profit margin by an additional 0.03-0.05 percentage points after the reform in 2007. Unlike Korkeamäki (2011), I thus find some evidence of increasing profits following the payroll tax reform, although the estimates are less precise in the fixed-effects specification.

A concern with the approach in Table 5, which captures an intention to treat rather than actual treatment, is that it may bias the effect on profits if firms adjust their intensity of using young workers after the reform. However, since the reforms seem to have had little effect on employment, the estimates probably represent a good approximation. The results are also consistent with previous results regarding worker outcomes in this paper, showing modest effects. In the absence of behavioural effects, it can be argued that the estimated reform effect on profits should be equivalent to the share of the wage bill in total revenue.¹⁷ Examining the data and adjusting the wage bill with the average intensity of using young

¹⁷ See Draca et al. (2011) who could not reject this hypothesis in their analysis of the introduction of the National Minimum Wage in the UK.

workers, this share turns out to be $0.160 \times 0.221 = 0.035$, which is within the range of the estimates presented in Table 5.¹⁸

The two placebo tests in Table 6 assume that reforms were undertaken in 2003 and 2004, respectively. All estimates are very close to zero, indicating no spurious effects on profit margins whatsoever.

Table 6. Placebo tests. Effects on firms' profits

a. Placebo reform 2003, 2000-2006

Profit margin, before financial items		Profit margin, after financial items	
w/o FE	FE	w/o FE	FE
-0.014	-0.013	-0.008	-0.009
(0.52)	(1.07)	(0.30)	(0.69)
[1,038]	[734]	[1,035]	[734]
{269}	{163}	{269}	{163}

b. Placebo reform 2004, 2001-2006

Profit margin, before financial items		Profit margin, after financial items	
w/o FE	FE	w/o FE	FE
-0.002	-0.008	0.000	-0.001
(0.06)	(0.66)	(0.01)	(0.05)
[943]	[712]	[940]	[712]
{273}	{182}	{273}	{182}

Source: Own calculations.

Notes: See note to Table 5.

A potential problem with the subset of firms used in the examination of profits is that it includes relatively large firms on average, although it accounts for more than 40 per cent of all observations of workers, implying that the results may not be representative for smaller firms. To shed some light on this issue, I split the sample into matched and non-matched firms and re-ran the benchmark regressions in Table 2 for the period 2004-2010 (the results are unreported for brevity). There were no significant differences in the reform effects for matched and non-matched firms, with one exception: for blue-collar workers, the increase in wages was

¹⁸ The share of the wage bill being within the range of estimates may be regarded as a permissive criterion, since the confidence intervals are rather large.

smaller and close to zero in the non-matched sample. These results speak against the possibility that the reform effect on profits in the non-matched firms is very different from the one that was found for matched firms. However, when I ran separate regressions for firms operating in the regional support areas, as a robustness check, the estimated effects on profits turned out to be negative, which is difficult to explain. This result along with the fact that I only cover a subset of firms in the industry cautions against far-reaching conclusions as to the wider applicability of my findings regarding profits.

7. Conclusions

This paper has exploited a Swedish payroll tax reform targeted at young workers, implemented in two stages in 2007 and 2009. The analysis considers effects on worker outcomes as well as firm performance in the retail industry.

Using a d-i-d approach, with slightly older workers as the control, the results on worker outcomes indicate that – on average – the effects on entry, exit, hours and wages have been small, both in absolute magnitudes and in relation to the sizeable cuts in taxes. My results on worker outcomes are in accordance with much of the previous literature on the employment effects of changes in payroll taxes, which has mostly concerned itself with reforms of flat rate or regionally differentiated payroll taxes. The findings are also similar to those obtained by Egebark and Kaunitz (2013) who examine the effects of the same payroll tax reforms as in this paper, but for the entire labour market. It is worth noting that they use a different evaluation method, combining d-i-d with matching and analyse effects on net employment instead of gross flows. Egebark and Kaunitz (2013) perform a cost-benefit analysis of the 2007 reform and estimate that each new job in the age group 19-25 is associated with a cost of SEK 0.9-1.5 million (USD 140 000-230 000). Their conclusion that reducing payroll taxes is a costly means of improving the employment prospects for the young is likely to hold also for the industry I analyse.

For workers bound by minimum wages, the estimated effects of the payroll tax reforms in my study suggest larger, but still modest, effects on the probability of entry. This result is consistent with the view that high

minimum wages represent a serious obstacle to labour market entry among the young. Since cutting payroll taxes for the youngest and the lowest-paid is also less expensive than cuts for other young workers, this result could be helpful in improving the design of payroll tax reforms. However, the findings may at least partly be explained by a higher cyclical sensitivity among the youngest and among minimum wage workers. It should also be kept in mind that the results derive from a particular industry, with high and binding minimum wages, so any policy implications from this study do not necessarily carry over to industries with different characteristics in this respect.

This study is one of the first to examine the effect on firm performance of payroll tax cuts. The analysis is based on comparing firms with marginally larger pre-reform shares of young workers with performance after the reform. There is some evidence of increasing profit margins following the reform, for the subset of firms analysed. While the subset accounts for a substantial part of all observations of workers in the industry, it covers relatively large firms on average, so the conclusions do not necessarily hold for other firms in retail or for other industries, especially those with a less intensive use of young workers. It should also be noted that I do not explicitly consider the effects of the payroll tax reforms on the entry or exit of firms, due to data limitations. This is unlikely to be a major problem in the short run but in the long run, profits could be eroded if more firms enter the industry or survive longer. Despite these shortcomings, the findings regarding profits are commensurate with the absence of large behavioural effects of the payroll tax cuts demonstrated in this study. This includes the absence of a large general wage increase, not only affecting treated workers, which is difficult to rule out in the d-i-d analysis of relative wage increases for the treated in the first part of the paper.

My analysis has considered several outcomes for workers and firms but is still far from complete. Non-wage personnel costs, like training, may have been affected by the reforms and these costs have not been accounted for. Retail prices, on which I have no suitable information, may also have adjusted in response to the payroll tax cuts.

References

- Anderson, P.A. and Meyer, B.D. (1997), The effects of firm specific taxes and government mandates with an application to the U.S. unemployment insurance program, *Journal of Public Economics* 65, 119-145.
- Bauer, T. and Riphahn, R.T. (2002), Employment effects of payroll taxes – an empirical test for Germany, *Applied Economics* 34, 865-876.
- Benmarker, H., Calmfors, L. and Larsson Seim, A. (2013), Earned income tax credits, unemployment benefits and wages: Evidence from Sweden, IFAU Working Paper 2013:12, Uppsala.
- Benmarker, H., Mellander, E. and Öckert, B. (2009), Do regional payroll tax reductions boost employment?, *Labour Economics* 16, 480-489.
- Brors, H. (2006), Sänkta avgifter skjuts på framtiden, *Dagens Nyheter*, October 5, 2006.
- Cruces, G., Galiani, S. and Kidyba, S. (2010), Payroll taxes, wages and employment: Identification through policy changes, *Labour Economics* 17, 743-749.
- Draca, M., Machin, S. and van Reenen, J. (2011), Minimum wages and firm profitability, *American Economic Journal: Applied Economics* 3, 129-151.
- Edmark, K., Liang, C-Y., Mörk, E. and Selin, H. (2012), Evaluation of the Swedish earned income tax credit, IFAU Working Paper 2012:1, Uppsala.
- Egebark, J. and Kaunitz, N. (2013), Do payroll tax cuts raise youth employment?, manuscript, Department of Economics, Stockholm University.
- Flood, L., Nordblom, K. and Waldenström, D. (2013), Dags för enkla skatter!, *Konjunkturrådets rapport 2013*, SNS Förlag, Stockholm.
- Government Bill 2006/07:84, *Nedsättningar av socialavgifter för personer som fyllt 18 men inte 25 år*, Ministry of Finance, Stockholm.
- Government Bill 2008/09:7, *Kraftfullare nedsättning av socialavgifter för unga*, Ministry of Finance, Stockholm.
- Gruber, J. (1997), The incidence of payroll taxation: Evidence from Chile, *Journal of Labor Economics* 15, S72-S101.
- Holmlund, B. (1983), Payroll taxes and wage inflation: The Swedish experience, *Scandinavian Journal of Economics* 85, 1-15.
- Huttunen, K., Pirttilä, J. and Uusitalo, R. (2013), The employment effects of low-wage subsidies, *Journal of Public Economics* 97, 49-60.
- Korkeamäki, O. (2011), The Finnish payroll tax cut experiment revisited, Working Paper 22, Government Institute for Economic Research, Helsinki.
- Korkeamäki, O. and Uusitalo, R. (2009), Employment and wage effects of a payroll-tax cut – Evidence from a regional tax experiment, *International Tax and Public Finance* 16, 753-772.
- Kramarz, F. and Philippon, T. (2001), The impact of differential payroll tax subsidies on minimum wage employment, *Journal of Public Economics* 82, 115-146.
- Kugler, A. and Kugler, M. (2009), Labor market effects of payroll taxes in developing countries: Evidence from Colombia, *Economic Development and Cultural Change* 57, 335-358.
- Lee, D. and Saez, E. (2012), Optimal minimum wage policy in competitive labor markets, *Journal of Public Economics* 96, 739-749.

- Marx, I. (2001), Job subsidies and cuts in employers' social security contributions: The verdict of empirical evaluation studies, *International Labour Review* 140, 69-83.
- Murphy, K.J. (2007), The impact of unemployment insurance taxes on wages, *Labour Economics* 14, 457-484.
- National Mediation Office (2012), *Avtalsrörelsen och lönebildningen*, Medlingsinstitutets årsrapport 2011, National Mediation Office, Stockholm.
- Nickell, S.J. and Bell, B. (1997), Would cutting payroll taxes on the unskilled have a significant impact on unemployment?, in D.J. Snower and G. de la Dehesa (eds.), *Unemployment Policy: Government Options for the Labour Market*, Cambridge University Press, Cambridge, New York and Melbourne.
- OECD (2003), *Employment Outlook*, OECD, Paris.
- Pencavel, J. and Holmlund, B. (1988), The determination of wages, employment, and work hours in an economy with centralised wage-setting: Sweden, 1950-83, *Economic Journal* 98, 1105-1126.
- Pissarides, C. (1998), The impact of employment tax cuts on unemployment and wages: The role of unemployment benefits and tax structure, *European Economic Review* 21, 155-183.
- SFS 2007:284, Lag om ändring i socialavgiftslagen (2000:980), June 5, 2007.
- SFS 2008:1266, Lag om ändring i socialavgiftslagen (2000:980), December 12, 2008.
- Skedinger, P. (2010), Sweden: A minimum wage model in need of modification, in D. Vaughan-Whitehead (ed.), *The Minimum Wage Revisited in the Enlarged EU*, Edward Elgar, Cheltenham, UK, Northampton, MA, USA, and ILO, Geneva.
- Skedinger, P. (2012a), Minimilöner i tjänstesektorn, in H. Jordahl (ed.), *Den svenska tjänstesektorn*, Studentlitteratur, Lund.
- Skedinger, P. (2012b), Tudelad trygghet, in A. Teodorescu and L-O. Pettersson (eds.), *Jobben kommer och går – behovet av trygghet består*, Ekerlids förlag, Stockholm.
- Skedinger, P. (2013), Employment effects of union-bargained minimum wages: Evidence from Sweden's retail sector, forthcoming in *International Journal of Manpower*.
- Summers, L.H. (1989), Some simple economics of mandated benefits, *American Economic Review, Papers and Proceedings* 79, 177-183.

Comment on Skedinger: Effects of payroll tax cuts for young workers*

Peter Skogman Thoursie**

The high unemployment rates among young people have lately received great attention. Whether or not unemployment among youth is high is not a controversial issue but the magnitude of the problem is. First, it is well known that the total unemployment rate can be decomposed into inflow and duration. Long periods of unemployment are often considered as a greater problem than short but repeated unemployment spells. One reason for the high unemployment rate among youth is that, on average, they have a higher inflow into unemployment than older workers – the length of the unemployment spells is, however, often shorter.

Second, there are different ways of measuring unemployment which, in my opinion, sometimes causes a somewhat confused debate. For example, when university students are searching for temporary jobs, they are classified as unemployed. Categorising unemployed in this way will, of course, overstate the problem. That said, it does not mean that there is no labour market problem if individuals take part in higher education because there are no jobs available. Also the lack of side-jobs implies lack of work experience, which may hinder entrance to the regular job market for students.

Another limitation when assessing the magnitude of the unemployment problem is when comparing unemployment rates across countries. The Swedish situation is, for example, exaggerated when considering the

* I am grateful to Mahmood Arai, Niklas Kaunitz and Anna Thoursie for thoughtful discussions.

** Department of Economics, Stockholm University, peter.thoursie@ne.su.se.

apprentice system. In Sweden, an apprentice is categorised as unemployed rather than employed. In many other countries it would be the opposite (Statistics Sweden, 2013).

With these introductory comments, I would like to point out that unemployment among youth is a rather complex issue. This means that we need to learn more about the labour market situation for young workers. In other words, more research on this topic is required. Research in economics has been very supply-side oriented during the last decades but research on labour demand has been more limited. Education and skills are certainly important for success in the labour market, but firm behaviour cannot be neglected when understanding the labour market functioning for young workers. For this reason, the paper by Skedinger is certainly a contribution. In the following, I will discuss a few issues that I hope can improve the paper.

In general, I think it is problematic to characterise youth into one single group since young people are very heterogeneous. Depending on their previous labour market experience, education level, skills, motivation, health, ability etc., their labour market situation can be very different. For example, how do youth with different characteristics differ in their unemployment situation? A more elaborate discussion on this issue could help us understand why the small effects occur.

The labour market situation is most likely most severe for groups such as high-school dropouts and individuals with disabilities. For these groups, the pay-roll tax reduction might not bite. Their current productivity (including the employer's perception of future productivity) is perhaps far below what the reduced pay roll tax compensates for. For other groups of young workers with a less severe labour market situation, maybe firms would hire them anyway – with and without the reduced pay-roll tax reduction – just a little cheaper now? This would imply great deadweight losses. One important policy question is then whether the policy should be more targeted towards more problematic groups who are not affected by the current pay-roll tax reduction?

The paper analyses only one industry, namely the retail industry. An interesting and important issue is to what extent young workers change jobs between industries. For example, how common is it that a young individual works as a shop assistant for a period and then changes to work at a restaurant? If, prior to the reduced pay roll tax, it was relatively easier

for young workers to get employment in the retail than in the restaurant industry, the reform could actually have increased their opportunities of being employed relatively more in the restaurant industry. One question is then if the policy opened up for more intensified search in other industries, such as for example in the hotel and restaurant industry. The point here is that by only focusing on one industry, one can potentially miss important general equilibrium effects. A suggestion is to introduce additional “relevant” industries in order to estimate heterogeneous effects with respect to industries

The above point is related to another issue that has to do with the possibility that young workers “leave” the sample used for other reasons. For example, are young workers (21-25) concentrated in certain types of firms, which were more affected by the financial crisis? A suggestion is to estimate survival probabilities for firms and investigate if there are differential effects depending on whether firms have a really young work force compared to a slightly older workforce.

The analyses estimate relative employment effects, i.e., did employment for the age group 21-25 change relative to the control group, 27-29? Such an analysis does not say anything about the elasticity of substitution between different age groups. For example, are those aged 27-29 replaced by the younger? Substitution effects could actually easily be estimated by estimating reform employment effects for those aged 27-29 compared to, for example, those aged 30-34. If employment due to the reform decreased for the age group 27-29 in such an analysis, this would indicate that they were to some extent replaced by workers aged 21-25.

A simple alternative analysis would be to compare the firms’ age distribution before and after the reform. If there were substitution effects, the age distribution would change.

Many reforms took place at the same time as the reduced pay-roll tax (see, for example, Nordström Skans, 2009). First, the “Work Guarantee” (*Jobbgarantin*) for youth was introduced in December 2007 and implied that unemployed individuals younger than 25 were offered intensified job coaching and charting of individual needs followed by active measures such as work training. The Work Guarantee replaced the previous programme called the “Youth Guarantee” (*Ungdomsgarantin*) but the Work Guarantee has more focus on job search. For this reason, the Work Guarantee might have created greater labour market opportunities for youth.

Second, the economic incentives had been changed since 2006. The income-related unemployment benefit was reduced relatively more quickly for young unemployed. After 100 days in the Work Guarantee programme, the replacement rate was reduced from 80 to 70 per cent during 100 days and after that further reduced to 65 per cent. This change in the economic incentives could have intensified the job search among young and increased employment. The same type of effect could also exist due to the abolishment of the right to receive basic unemployment benefits after studies.

Third, an interesting policy that is closely related to the reduction in pay roll taxes is the “New Start Job Subsidy” (*Nystartsjobb*), which provides an additional subsidy (reduced pay roll tax) to an employer who employs a young worker who has been unemployed or absent due to sickness longer than six months. The reduced pay roll tax lasts between 6-12 months, depending on how long the individual has been unemployed (or on sick leave). As such, this policy is more directly targeted at groups of young workers with disadvantages on the labour market.

I think the paper would benefit from a more up front discussion of these policies. The question is whether the paper evaluates the whole package of reforms rather than only the effect of a reduced pay roll tax. Out of curiosity, one wonders why policy makers introduced two different policies where both reduce pay roll taxes. This is also related to the question of how informed employers were of these reforms. For example, to what extent was the “New Start job subsidy” used during this period? If most employers were not aware of this policy, it would not contaminate the effects estimated in the current study. To what extent were employers informed about the reform utilised in the current paper?

More technical issues regarding threats to identification is the assumption of parallel trends in outcomes for the two groups aged 21-25 and 27-29, respectively. Some placebo analysis in the paper suggests that this is not the case. It seems quite plausible that different age groups might have different employment trends since they might be differently affected by recessions (or booms). For example, the “after period” in the analysis covers the financial crisis period. I think the paper should be more precise on this by estimating yearly “reform-effects” for all years – before as well as after the reform. By doing this, one can clearly check whether the yearly reform-effects prior to the reform are all zero (in a statistical sense).

Only if this is the case it is possible to say anything causal about the reform effect.

An alternative strategy to the difference-in-differences (DD) approach used in the paper would be to apply a regression discontinuity (RD) approach, which is less sensitive to the parallel trends assumption and perhaps less sensitive to the presence of other reforms (unless these other reforms also have the same age cut off). Basically, the RD approach implies comparing employment between those aged 26 and 25 in 2007 when the reform was implemented. If one still worries about an effect on employment of being one year older, one could compare employment between those aged 26 and 25 in 2007 with the corresponding comparison before the reform, say in 2005.

I guess one reason why RD is not used is that, as the author points out, the treatment “dose” is larger the younger a worker is since the employer can keep the reduced tax for a longer time. If sticking to the DD analysis, I therefore suggest to more directly estimate age-specific treatment effects in the same model.

Another alternative approach that does not rely on the same outcome trends between older and younger is to use a DD set-up where both younger and older are controls (see Benmarker et al., 2013). In January 2009, the age threshold for receiving reduced pay-roll taxes was raised from 25 to 26. One can then compare employment for those aged 26 after and before the reform with the before-after contrast for the control group consisting of younger (aged 24-25) and older (aged 27-28). This would be more convincing due to the controls consisting of age groups on either side of 26. The strategy can be summarised in Table 1.

Table 1. Summary of treatment-control groups before and after

	Before 2009		After 2009	
	Control	Treated	Control	Treated
Ages	24,25/ 27,28	26	24,25/ 27,28	26
Reduced pay-roll	Yes/No	No	Yes/No	Yes

Source: Inspired by Benmarker et al. (2013).

This approach allows for differential employment trends across age groups in the absence of treatment, as long as the trends change linearly

with age groups. One cannot check this directly, but one can look at what after-before trends look like in general for ages below and above 26.

References

- Benmarker, H., Nordström Skans, O. and Vikman, U. (2013), Workfare for the old and long-term unemployed, forthcoming in Labour Economics.
- Nordström Skans, O. (2009), *Varför är den svenska ungdomsarbetslösheten så hög?*, Rapport till Finanspolitiska rådet 2009/6, Stockholm.
- Statistics Sweden (2013), *Ungdomsarbetslöshet – jämförbarhet i statistiken mellan ett antal europeiska länder*, Bakgrundsfakta, Arbetsmarknad- och utbildningsstatistik, Rapport 2013:1, Stockholm.

Sanctions for young welfare recipients^{*}

Gerard J. van den Berg^{**}, Arne Uhlendorff^{***} and Joachim Wolff^{****}

Summary

Social welfare systems usually imply specific obligations for benefit recipients. If a recipient does not comply with these obligations, a sanction involving a punitive benefits reduction may be imposed. We give an overview of the literature on the effects of sanctions in social welfare systems and present first results on sanction effects for young unemployed welfare recipients based on German administrative micro data. Our results suggest that mild and strong sanctions lead to an increased transition rate to work and that this effect is higher for strong sanctions. However, strong sanctions for young welfare recipients involve a complete withdrawal of the basic cash transfer payments.

Keywords: monitoring, welfare, youth unemployment, duration models, unemployment benefits, social assistance.

JEL classification numbers: J64, J65, I38, C41.

^{*} We thank Olof Åslund, Johan Vikström, the editor Michael Svarer, an anonymous referee and participants at the NEPR conference on “Youth and the labour market” for helpful comments.

^{**} University of Mannheim, IFAU Uppsala, IZA Bonn, gjvdberg@xs4all.nl.

^{***} University of Mannheim, IAB Nuremberg, DIW Berlin and IZA Bonn, uhlendorff@uni-mannheim.de.

^{****} IAB Nuremberg, Joachim.Wolff@iab.de.

Being on welfare for a long period of time has adverse effects on future employment prospects. At the early stages of the working career, a long welfare spell may lead to the loss of cognitive human capital skills as well as non-cognitive skills such as work motivation and discipline. Therefore, the activation and reintegration of young welfare recipients is an important policy goal in many European countries. In this context, monitoring and sanctions constitute a central policy tool. Social welfare systems usually define specific obligations for unemployed benefit recipients. If a recipient does not comply with his or her obligations, benefit rules may stipulate the imposition of a sanction. For example, for unemployed welfare recipients in Germany, sanctions can be imposed if the benefit recipient refuses a job opportunity or the participation in active labor market policy programs, or if the benefit recipient missed an appointment with her caseworker. By setting an incentive to comply with such job search requirements, the intention is to combat moral hazard and to increase the transition rate from welfare to work.

The German welfare system allows for *mild* and *strong* sanctions. The main reason for a mild sanction is missing an appointment with the caseworker, whereas the refusal to search for a job or participate in a training program may lead to a strong sanction. Both types of sanctions usually imply a reduction of the benefit payments for three months, but they differ with respect to the size of the reduction. Strong sanctions are particularly severe for young welfare recipients. As a rule, they involve a complete withdrawal of welfare for up to three months, apart from payments for rent and heating. The underlying idea is that the threat of such a sanction induces young welfare recipients to leave welfare as quickly as possible. Thus, these young welfare recipients do, on the one hand, have a higher probability of participating in active labor market programs due to special rules aiming at supporting this sub-population while, on the other hand, they are sanctioned more strongly if they do not comply with the benefit rules. In contrast to this, mild sanctions do not differ between recipients below and above 25 years of age. For both age groups, they amount to a reduction of the basic cash benefit payments by 10 percent.

Economic job search models incorporating sanctions in unemployment explicitly predict a faster transition rate to work once a sanction is imposed. This is because the reservation wage tends to fall and the search intensity tends to rise. This prediction has been confirmed in a number of

studies (see the overview in Section 2). There additionally exists evidence that an increased sanction probability leads to an increased probability of leaving the labor force and the welfare receipt (see e.g. McVicar and Podivinsky, 2009). Moreover, evidence based on qualitative surveys among caseworkers and young sanctioned individuals for Germany suggests that strong sanctions might have adverse effects and might, for example, lead to low paid and unstable jobs or increase debt problems and have a negative impact on nutrition (Götz et al., 2010 and Schreyer et al., 2012).

In this paper, we examine the effects of sanctions on the transition rate to work among young welfare recipients. We study the inflow into “welfare without employment” during the period January 2007 to March 2008. Our sample is drawn from administrative records. It is restricted to welfare recipients aged 18 to 24 years and to their first welfare spell during this observation window. The analysis is based on a sample of about 72 000 young men in West Germany. The majority of the individuals in our sample are singles without children. In Germany, the welfare level for individuals in this group is low compared to Nordic countries like Sweden and Denmark, and it is also lower than in the Netherlands (Immervoll, 2009). We argue that the monitoring system in Germany is special in the sense that it is unusually strict, with sometimes severe sanctions, and a high sanction rate.

Broadly speaking, our contribution to the literature on sanctions is twofold.¹ First, no previous study has focused on young welfare recipients, who are vulnerable in the current economic crisis and who may not enter stable employment careers for a long period if they do not move to work in the near future. Second, we jointly analyze the impact of mild and strong sanctions on the transition rate to work.

The empirical analysis takes the dynamic selection of young welfare recipients into the treatment into account by applying the “timing of events approach” following Abbring and van den Berg (2003). This approach allows us to control for selection into treatment based on observed and unobserved characteristics. We are interested in the impact of two types of imposed sanctions on the probability of leaving unemployment for a job. Therefore, we jointly estimate the hazard rate to mild sanctions, to strong sanctions and to unsubsidized work.

¹ See Section 1 for a literature overview.

Similar to other studies on the effect of sanctions on the transition rate to work, the effects we find are significantly positive. Perhaps more interesting are the magnitudes of the effects we find. In particular, it is interesting to compare the estimated effects of the two types of sanctions with each other. From a forward-looking individual's point of view, once a sanction has been given, it is irrelevant why the sanction was given. What matters are the benefits reduction and the increased exposure to future monitoring after a detection. If the estimated effects are not very different for different sanction types, we conclude that the effects are mostly driven by the fear of increased monitoring. If the effect of a strong sanction is much larger than the effect of a mild sanction, then we conclude that the effect is mainly driven by the benefits reduction. These inferences are relevant from a policy point of view. If the effect is mostly driven by the monitoring threat, then it is preferable to use mild sanctions for any violation, at least in case of a first punishment during a welfare spell. Van den Berg and Vikström (2013) argue that mild sanctions may be easier to apply by case workers if they have a personal bond with their clients, as is the case in e.g. Sweden. As we will see below – according to the results of a qualitative study – case workers of welfare recipients in Germany are likely to be less reluctant to apply mild compared to strong sanctions.

The paper is organized as follows: Section 1 provides an overview of the previous literature on sanctions in social welfare systems. Section 2 describes the institutional background in Germany. Section 3 presents the administrative data and descriptive statistics. Section 4 describes the econometric approach. The results of the empirical analysis are presented in Section 5 and Section 6 concludes the paper.

1. Previous evidence on the effects of sanctions in social welfare systems

There only exist a few studies on the effectiveness of sanctions in social welfare systems. In the following, we give a brief overview of the main empirical results, whereby we focus on European countries. We start by presenting results of quantitative studies based on data from Germany and

the Netherlands, before we discuss the results of two qualitative studies on sanctions in the German social welfare system.²

An early study of the effectiveness of sanctions for welfare recipients is provided by van den Berg et al. (2004). The analysis is based on an inflow sample into welfare benefit receipt in Rotterdam. Based on this inflow sample, the authors estimate timing-of-events-models. The duration and size of benefit reductions are rather low: most welfare sanctions are for 1 to 2 months and the maximum reduction of the welfare benefit is 20 percent. The results suggest a strong increase of the hazard rate from welfare to work (+148 percent). The authors investigate the effect of heterogeneity with respect to age. Their results suggest that the sanction effect varies over different age-intervals. The lowest point estimate for the sanction effect was found for unemployed workers aged below 26 years and the highest for those aged between 55 and 65 years. However, the effects of these two groups are not statistically different from each other.

Based on a more recent inflow sample into welfare receipt from the same municipality, van der Klaauw and van Ours (2013) compare the effects of re-employment bonuses and benefit sanctions on the exit rates from welfare to work. While they do not find a significant impact of re-employment bonuses, their results indicate a positive impact of sanctions on the job-finding probability. Compared to the study of van den Berg et al. (2004), their estimates suggest a relatively small increase by 21 percent for men and 47 percent for women. They argue that the increased use of sanctions compared to the 1990's may have reduced the effectiveness and may explain the difference in the effect sizes. They do not find any evidence for different effects depending on the age-group of the welfare recipients when they consider men. However, for women, the sanction effect increases considerably over four age intervals (<25, 25-34, 35-44, and 45-54 years) and then falls sharply for those aged at least 55 years. However, only one of the differences is statistically significant at the 10 percent level.

² There additionally exists a number of studies analyzing the impact of sanctions in unemployment insurance systems, see, e.g., Abbring et al. (2005), Lalive et al. (2005), Svarer (2011), Cockx et al. (2011), Roed and Weslie (2012), van den Berg and Vikström (2013), Arni et al. (2013) and van den Berg et al. (2013). These studies usually find that sanctions increase the probability of leaving unemployment for a job. Some of the studies additionally investigate the impact on job match quality, and their results suggest that jobs found after the imposition of a sanction go along with lower wages and are less stable.

There exist several studies on the impact of welfare benefit sanctions in Germany. Schneider (2008, 2009) uses matching methods to analyze survey data on the job search behavior and employment outcomes based on a survey of people who received welfare at the start of the year 2005 and who were interviewed at the end of 2005 or the start of 2006. Her results indicate no well-determined effects of benefit sanctions on reservations wages or the search effort of welfare recipients, but partially positive effects on their regular employment rate. One reason for not finding any effects on the first two outcomes might be that these variables are not available for the time period directly after the sanctioning, but just at the interview dates. Her sample includes young welfare recipients. However, the effectiveness of sanctions is not analyzed separately for this group.

Boockmann et al. (2009) investigate 154 (out of 439) German job centers. A job center is a local welfare agency, which is for example responsible for the provision of welfare benefits and active labor market policy programs for welfare recipients. Their analysis is based on combined data from a survey of welfare recipients conducted in two waves (at the beginning of 2007 and about one year later) and administrative records. Moreover, they match survey data on activation strategies of the 154 job centers to the individual data. They study sanction effects by an instrumental variable approach: individual sanctions are either instrumented by the sanction strategies of the job centers as reported by their managers or by their actual sanction rates. Based on linear models, they estimate the impact of sanctions on employment and welfare receipt separately for the first eight months after a first sanction was imposed. Their results point towards a reduction of the probability of welfare receipt and a rise in the employment rates due to intensified sanctioning. The authors do not investigate effect heterogeneity with respect to age.

In a more recent study, Hillmann and Hohenleitner (2012) analyze the effects of welfare benefit sanctions based on the household panel survey PASS (Labor Market and Social Security). The authors apply a timing-of-events approach and find positive effects of imposed sanctions on the probability of leaving welfare for work. However, the first wave of the PASS data consists of a stock-sample of households being on welfare giving retrospective information about previous sanctions. This sampling scheme has to be taken into account for the estimation of duration models and it implies that it is hard to interpret the results of a model which is

based on the assumption of having an inflow sample into the state of interest, in this case the welfare receipt.

1.1 Qualitative evidence on the effectiveness of sanctions for young welfare recipients

Götz et al. (2010) analyze a qualitative survey conducted in 11 job centers in Germany. 26 caseworkers or other people responsible for welfare recipients and sanctions were interviewed between October 2007 and April 2009. The interviews suggest a relatively positive view of the caseworkers on mild sanctions. They can lead to a more reliable behavior of sanctioned welfare recipients in the future. Strong sanctions lead to a withdrawal of the basic cash benefit, while the benefit to cover the costs of accommodation and heating remains untouched. The caseworkers seem to be rather skeptical about the effectiveness of this type of sanctions. Some of them reported examples of severe sanctions that apparently altered the welfare recipients' behavior in a desirable way. However, caseworkers often assessed critically that the entire benefit to cover daily expenses is withdrawn. In particular, the sanctions might have no impact on the behavior of drug addicts or people with alternative sources of income, e.g., when working in the shadow economy. Moreover, as far as welfare recipients change their behavior, the strong sanctions might induce them to quickly accept any kind of job available. The caseworkers stress potential adverse consequences for the sanctioned individuals, e.g. if they accept jobs that are low paid, unstable and that provide too little training for people at the beginning of their career. Repeated strong sanctions within one year imply a temporary loss of the entire welfare benefit including the benefit that covers rent and heating. Only four of the interviewed caseworkers regarded these sanctions as appropriate. The other respondents were more critical and rather interpreted them as a tool for extreme situations, e.g., if welfare recipients refuse any kind of cooperation. According to the analysis, two main reasons explain that caseworkers have a relatively hostile view of these sanctions. Their social conscience is one of them. Another reason is that the very severe sanctions can cause harm that makes it more difficult to achieve the key goal of reducing or ending the benefit recipients' welfare dependency, in particular by integrating them into work. That is, if the sanctioned people can no longer afford

their accommodation and have to move out, they do not get any closer to the goal of reducing or ending welfare benefit. It may rather become more difficult to reach this goal.

In a related study, Schreyer et al. (2012) conducted a survey in one selected job center. They interviewed young welfare recipients with at least one imposed strong sanction in the past. 10 male and 5 female welfare recipients took part in this survey during the period of May to November 2010. Their results indicate that many of the sanctioned respondents come from families that were abandoned early by their father. The mothers of some of them had severe health problems and some of the respondents talked about experiences of violence and/or imprisonment. All of them were characterized by a discontinuous education and employment history. About ten faced repeated sanctions and (temporarily) lost their entire welfare benefit. The results suggest that sanctions lead to restricted nutrition in the sense of decreased spending on healthy and fresh food. However, none of the respondents reported hunger as a consequence of sanctions. With respect to their accommodation, four respondents who faced a (temporary) complete withdrawal of their welfare benefit could no longer pay bills of electric power suppliers. As a consequence, the suppliers blocked their services. Moreover, four sanctioned respondents lost their apartments and had to temporarily move into a host of the homeless. Many of the respondents reported that sanctions increased their debt problems. Moreover, the responses provided some indication that due to the sanction, welfare recipients took up jobs without declaring them to the welfare agency or engaged in criminal activities in order to earn some money. Moreover, the analysis points out that sanctions can have a negative effect on psychosocial well-being and social inclusion. Overall, the results indicate that strong sanctions might go along with negative effects on the sanctioned individuals, which are usually not captured in studies focusing on the duration until finding a job and the corresponding quality of this job.

2. Institutional settings in Germany

This section describes the German welfare system, the Social Code II, and its sanction rules. The Social Code II was introduced at the start of

the year 2005. With its introduction two former means-tested benefits, the earnings-related unemployment assistance (UA) benefit and the flat rate social assistance (SA) benefit, were replaced by a new (mostly) flat rate benefit, unemployment benefit II. Households whose other sources of income are insufficient to achieve a minimum standard of living are eligible for the benefit. This implies that individuals who are running out of UI benefits are – depending on the household income – eligible for unemployment benefit II. A considerable move of the welfare system towards activating welfare recipients was implemented with the reform (see e.g. Hohmeyer and Wolff, 2012).

Since the reform in 2005, in principle, all members of a welfare recipient household who are capable of working are obliged to contribute to the goal of reducing the welfare dependency of their household by taking up employment. They should engage in job search and participate in active labor market programs (ALMPs). The Public Employment Service (PES) is supposed to support welfare recipients in their job search and assist them with suitable ALMPs. The actions that should be taken by a welfare recipient and by the PES to improve a welfare recipient's perspectives in the labor market are documented in an individual action plan. The PES should sanction welfare recipients, who do not comply with their obligations without any good cause. In the following, we focus on key features of the welfare system that are relevant to our analysis and on its rules that were in force in the period under review, i.e. the years 2007 to 2009.

2.1 The German welfare benefit system during the years 2007 to 2009

During 2007 to 2009, on average about five million people aged at least 15 years and who were capable of working received unemployment benefit II (see Table 1). This amounts to roughly one tenth of the German population aged 15 to 64. Less than one fifth of the welfare recipients were aged younger than 25 years. About two thirds of them lived in West Germany.

Table 1. Annual average stock of welfare recipients, who were capable of working, in the period under review (in Mio)

Year	All			Aged below 25 years		
	2007	2008	2009	2007	2008	2009
Germany	5.28	5.01	4.91	1.04	0.96	0.91
West Germany	3.39	3.24	3.22	0.66	0.63	0.62
East Germany	1.88	1.77	1.68	0.37	0.33	0.29

Source: Statistics Department of the German Federal Employment Agency.

The means-tested welfare benefit consists of several components. There is a basic cash benefit to cover a minimum level of regular expenditures of a welfare recipient apart from costs for rent and heating (Regelleistung zur Sicherung des Lebensunterhalts). At the beginning of the year 2007 the benefit amounted to EUR 345 a month for singles, single parents or a person whose partner was younger than 18 years. 80 percent of this amount were also given for additional household members aged at least 15 years who were capable of working.³ In a couple household of two adult partners, each partner received 90 percent of this amount. For household members who were not capable of working, the benefit was 60 percent of EUR 345 if a household member was younger than 15 years and 80 percent for a household member aged at least 15 years. During the period 2007 to 2009, the basic level of this benefit was raised each year in July and reached a level of EUR 359 in July 2009.

Another component of the welfare benefit is provided to cover costs for accommodation and heating. There is no general upper limit for this benefit, but it is determined by job centers by considering the relevant factors like the size and composition of the household, the size of the apartment, rents excluding cost of heating and the relevant local rent levels, which can vary considerably over different municipalities. In the period under review, the job centers also provided an allowance to cover contributions to old age pension, compulsory long-term care and health insurance. There are various other temporary components of the welfare benefit: Some allowances are available to finance temporary needs, e.g., such as costs related to pregnancy or costs for changing accommodation. Moreover, until the start of the year 2011, a temporary benefit was grant-

³ Social Code II considers people aged 15 to 64 years as capable of working, if they are able to work at least three hours per day.

ed to welfare recipients who had exhausted their unemployment insurance (UI) benefit during the last two years.

Immervoll (2009) provides some indicators that characterize the generosity of minimum-income like the one just described in 29 OECD countries for households with no other sources of income. He presents estimates of the benefit levels relative to the median household income for one-person households, single parent households with two children and married couples with two children in the year 2007. For the two latter household types, in Germany, the benefits (including the benefit for housing costs) amount to more than 55 percent of the median household income. Only a few countries are more generous. In most countries, these benefits constitute less than 50 percent of the median household income for these two household types. The benefits for single person households in Germany are far less generous, both in comparison to those for the other household types and from an international perspective. In Germany, they account for about 45 percent of the median household income of this household type. This is particularly relevant for our purposes because this household type comprises most of our population of interest.

2.2 Punitive sanctions for welfare recipients

The benefit rules specify several obligations for welfare recipients. They should not miss appointments with job center staff or an appointment for a necessary medical examination. The other obligations are mainly concerned with incentives to search for work or improving the job finding perspectives of welfare recipients. Welfare recipients, who are capable of working, are expected to provide sufficient effort on job search, participate in suitable ALMPs that should enhance their job finding perspectives, and accept suitable job offers. Moreover, they have to discuss and sign an individual action plan with their job center and have to comply with the requirements specified in the individual action plan. Finally, they should not deliberately reduce other sources of income than welfare or available assets, in order to remain or become eligible for welfare or raise their welfare benefit. If they do not comply with their obligations without good cause, a punitive sanction should follow. The duration of a sanction is fixed to three months; for welfare recipients aged less than 25 years,

the sanction period may be reduced to six weeks.⁴ As described above, a temporary benefit was granted to welfare recipients who exhausted their unemployment insurance (UI) benefit during the last two years. A sanction implies a complete cut of this temporary benefit during the sanction period.

The infringements against the obligations are divided into two broad categories that lead to different benefit cuts. The first broad category consists of missing an appointment with the job center or for a medical or psychological examination. A first noncompliance leads to a reduction of the welfare benefit by 10 percent of the full basic cash benefit to cover the regular expenditures of a welfare recipient apart from costs for rent and heating. Thus, in (the first half of) the year 2007 the reduction would have been EUR 34.5 for a single person, even if he did not receive the full EUR 345 cash benefit because he already achieved some earnings. Such a sanction therefore reduces the total sum of the benefit components. If welfare recipients are not yet 25 years old, the benefit component to cover accommodation and heating cannot be reduced by the first sanction. Repeated infringements of the same type within one year implied higher sanctions: The sanction is then determined by the amount of the last sanction plus an additional reduction by 10 percent of the basic full cash benefit.

The second broad category includes all other possible infringements. These mainly concern direct efforts to search for work and to improve job-finding perspectives. The share of sanctions due to deliberately reducing other sources of income than welfare or available assets is negligible.⁵ The consequences of non-compliance differ between welfare recipients aged at least 25 and those aged below 25. The former face a reduction of 30 percent of the basic full cash benefit for their first infringement. A second infringement within one year doubles the sanction.⁶ Any further

⁴ Case-managers can reduce the length of the sanction period under certain circumstances, e.g., if a young welfare recipient refuses a job offer, but convincingly demonstrates that he is available for work or if the welfare recipient is underage and was not fully aware of the consequences of his non-compliance.

⁵ In our observation period, they make up for around one percent of the sanctions due to infringements that belong to this second broad category (Source: Statistics Department of the Federal Employment Agency).

⁶ Provided that the benefit sanction exceeds 30 percent of the full cash benefit, the job centers can provide non-cash benefits, in particular food stamps to sanctioned welfare recipients. However, no official data are available that describe how frequently sanctioned welfare recipients receive such non-cash benefits.

repeated infringement within one year leads to a full (temporary) benefit loss. In contrast, welfare recipients aged younger than 25 face more severe sanctions. Already in case of a first non-compliance of this type, their welfare benefit is limited to the component that covers the costs for rent and heating. Any further infringement out of this second broad group of sanction reasons within a period of one year implies a full loss of the welfare benefit for three months. To sum up, sanctions against welfare recipients aged below 25 are particularly severe.

Table 2 shows the annual averages of the percentage of welfare recipients with at least one sanction. The share of sanctioned welfare recipients is around 2.5 percent and quite stable over the years 2007 to 2009. In the West, it is slightly higher than in the East. For men, the proportion of sanctioned welfare recipients ranges from 3.4-3.6 percent and is more than twice as high as for women. The share of sanctioned individuals is with its 3.8-4 percent the highest among young welfare recipients below the age of 25 years. In contrast, welfare recipients aged at least 50 years are very rarely sanctioned. Table 3 displays the monthly average number of new sanctions in relation to the average annual stock of welfare recipients in Germany. In contrast to Table 2, it distinguishes between the two broad categories of sanctions due to missing an appointment and sanctions due to any other infringement. The monthly rate of new benefit sanctions due to missing an appointment ranges from 0.66 to 0.71 percent in the years 2007 to 2009. There are fewer sanctions related to all other infringements. The flow rate for the stronger sanctions is about 0.5 to 0.6 percent; in the West, it is about 1.3 to 1.5 times as high as in the East. This reflects the fact that the unemployment rate in the West is considerably lower than in the East and hence, the scope for sanctioning welfare recipients due to insufficient job search or the refusal of placement proposals is higher.

Currently, no aggregate sanction statistics are available that allow us to describe these flow rates for different age groups. However, for different socio-demographic groups of welfare recipients, Wolff and Moczall (2012) analyzed the empirical transition rates into the first sanction due to missing an appointment and into the first sanction for other reasons. Their estimates show that for both types of sanctions, the transition rates of welfare recipients aged 16 to 24 years are usually far higher than those of welfare recipients from older age groups. A major reason for this is that

with the introduction of the new welfare regime in 2005, they were defined as a special target group. In particular, Social Code II expects that job centers place young welfare recipients immediately into work, training or work opportunities. Nivorozhkin and Wolff (2012) show that over the period 2005 to 2010, unemployed welfare recipients aged 20 to 24 are characterized by an inflow rate into One-Euro-Jobs, the main work opportunity scheme, of 5.3 to 6.5 percent. In contrast, the corresponding numbers for unemployed welfare recipients aged 25 to 29 are lower than two percent. They find somewhat less pronounced but still high differences for the inflow rates into other ALMPs.

Table 2. Average share of welfare recipients with at least one sanction (stock) in the period under review (in percent)

Region Year	Germany			West Germany			East Germany		
	2007	2008	2009	2007	2008	2009	2007	2008	2009
All	2.4	2.5	2.5	2.5	2.7	2.6	2.1	2.3	2.3
Men	3.4	3.6	3.6	3.7	3.9	3.8	3.0	3.2	3.2
Women	1.4	1.5	1.5	1.5	1.6	1.6	1.2	1.3	1.4
Aged younger than 25 years	3.8	3.9	4.0	3.7	3.8	3.8	4.0	4.1	4.4
Aged 25-49 years	2.6	2.8	2.8	2.8	3.0	2.9	2.2	2.3	2.4
Aged at least 50 years	0.8	0.9	0.8	0.9	1.0	0.9	0.6	0.7	0.7

Source: Statistics Department of the German Federal Employment Agency, own calculations.

Table 3. Average number of new sanctions per welfare recipient and month in the period under review (in percent) by sanction type

Reason for sanction Year	Missing an appointment (relatively low benefit reduction)			Other reasons (relatively high benefit reduction)		
	2007	2008	2009	2007	2008	2009
All	0.66	0.69	0.71	0.58	0.58	0.52
West	0.68	0.70	0.71	0.66	0.65	0.57
East	0.64	0.68	0.72	0.43	0.46	0.43

Source: Statistics Department of the German Federal Employment Agency, own calculations.

The procedure of imposing a benefit sanction consists of several steps. In general, the job centers must ensure that welfare recipients are aware of the potential reasons for a benefit sanction and their consequences. If the job center observes that a welfare recipient does not comply with a benefit rule, the case-manager has to document the infringement and the job center sends a written notification to the welfare recipient. It contains the details of the non-compliance and its consequences. Moreover, it contains an answer form, which the welfare recipient can use to report a

good cause for the potential non-compliance, as well as the date when the job center has to receive the response. The rules do not specify how much time has to be available for such a response. However, our administrative micro data provide the date of non-compliance and the date when the sanction begins and suggest that welfare recipients might often have about one to three weeks to return the response form. If the welfare recipient does not or cannot provide some good cause, the sanction against a welfare recipient comes into force on the first day of the calendar month that follows the month in which a welfare recipient had to hand in the answer form to the notification letter.

The benefit rules envisage an imposition of a sanction, if a welfare recipient does not comply with his obligations. Nevertheless, there might be various reasons why sanctions are not automatically imposed. First of all, not all infringements are fully observed, such that a sanction can be imposed without some doubt about the non-compliance and hence, without risking to lose a lawsuit at a Social Court. Moreover, job center staff with a huge workload might have insufficient time to monitor welfare recipients with the same intensity or provide them with job offers or ALMP placement proposals with the same intensity, even if they are similar in terms of their characteristics. Furthermore, caseworkers might have some discretion with respect to imposing a benefit sanction, as Social Code II does not entirely specify what constitutes a good cause for not following some of the obligations. They have some degree of freedom to decide whether personal reasons that a welfare recipient provides, in order to justify a suspected non-compliance, constitute a good cause. Finally, an observed refusal might not lead to a benefit sanction, if the job center staff did not inform a welfare recipient about the consequences of a refusal or if there is some doubt that the welfare recipient was properly informed about them.

3. Data and descriptive statistics

Our analysis is based on German administrative data that the Institute for Employment Research makes available for scientific use. These data offer a number of relevant variables on welfare recipients and their households that allow estimating the effects of benefit sanctions on transition rates

from welfare receipt without employment to unsubsidized employment. We combine information from two databases: the Integrated Employment Biographies (IEB) and the Unemployment Benefit II History Records.

The IEB contain spell data on contributory and minor employment as well as different types of benefit receipt, registered unemployment and job search, and ALMP participation. All spell information provides the exact day when the spell starts and ends. These data are informative about a number of personal characteristics like birth date, sex, nationality, highest schooling and occupational degree, place of residence (of the individual, employers, labor agencies and job centers) and disability. The Unemployment Benefit II History Records provide more detailed information on welfare recipients. They contain a household identifier. Therefore, with these data, we know the composition of a welfare recipient household, the role of different household members in such a household, their marital status and whether they are considered as capable of working. The Unemployment Benefit II History Records are informative about the (monthly) amount of each component of unemployment benefit II that a welfare recipient is entitled to. Additionally, there is information on their earnings and other sources of income. Finally, these data record punitive sanctions, including the day when a sanction started and ended, the day when some non-compliance took place, the type of non-compliance, and the time period within which another sanction is regarded as a multiple sanction that leads to a higher benefit reduction. There is, however, no information on warnings, since the administrative processes of the job centers only imply a systematic documentation of sanctions that came into force.

Our sample is based on an inflow sample into welfare of men aged 18 to 24 years during the period January 2007 to March 2008. We limit our analysis to West Germany, since the unemployment rate in West Germany is considerably lower than in East Germany. Compared to the East, in the West there is much more scope for job centers to place welfare recipients in jobs or in vocational training. Therefore, it is also much more likely that young welfare recipients are sanctioned because of refusing such offers or because of insufficient search effort. We focus on males who are registered as job-seekers and hence, should be available for work. For the primary caretaker of children below the age of three, the job search requirements are different. Since this more often concerns

women than men, (endogenous) fertility – if not accounted for – might bias the estimated effects of sanctions. In line with this, Wolff and Moczall (2012) show that the sanction rates for mothers of children aged less than three years are close to zero, while this is not the case for fathers of very young children; in West Germany the estimated empirical sanction hazards of fathers of very young children are quite similar to those of childless single men. Therefore, focusing on men in West Germany keeps the estimated model manageable and leads to a relatively homogenous estimation sample.

We restrict the sample to individuals who at the beginning of their spell are registered as job seekers, are not working in a contributory job or participating in selected ALMPs. The selected ALMPs comprise a start-up subsidy and longer term training programs. During their welfare spells, individuals might work in minor employment. Moreover, our spell definition does not exclude that they participate in short-term training, as such a participation only lasts for some days and up to three months, or participate in the workfare scheme, One-Euro-Jobs. Both programs imply that welfare recipients continue with their job search. In particular, One-Euro-Jobs are in nearly all cases part-time jobs, in order to leave some room for welfare recipients to continue with their job search. We do not consider individuals who were already subject to some benefit sanction at the start of their spell. A major reason for this is that at the start of their spell or just before their spell, they still received an UI benefit and have been sanctioned due to non-compliance with the UI benefit rules and the sanction period has not yet ended. In such a case, they are also automatically sanctioned under the welfare benefit rules at the time when they claim welfare. Finally, we discarded individuals who still received UI benefit at the start of their welfare spell. For them, the consequences of some non-compliance with benefit rules without good cause do not only lead to a punitive sanction under the welfare benefit system. They are also subject to a loss of their UI benefit for some weeks. However, part of our sample consists of individuals who exhausted their UI benefit.

We have to exclude from our data welfare recipients in 50 West German job centers for which micro data on sanctions are currently not available. These job centers are entirely run by municipalities and not jointly with local labor agencies. In the years 2007 to 2009, about 13 percent of the (stock of) unemployed welfare recipients were registered in these job

centers in West Germany (Statistics Department of the Federal Employment Agency).

Our final sample consists of 71 667 spells of male welfare recipients. We right-censor spells at the time when a welfare recipient reaches the age of 25 years, since the sanction rules change with reaching this age threshold. We analyze transitions into unsubsidized contributory employment. To exclude low-income jobs, we determine a minimum income of EUR 500 per month.⁷ However, individuals with an unsubsidized job might still receive additional welfare payments, if their wage is (still) below the legal minimum subsistence level.

Table 4. Share of exit into strong and mild sanctions and into unsubsidized jobs^{a)}

Men	All	Never sanctioned	With strong sanction	With mild sanction
Share of exit into				
1st strong sanction	0.132	0.000	1.000	0.388
1st mild sanction	0.122	0.000	0.360	1.000
unsubsidized contributory job	0.329	0.353	0.242	0.211
Number of spells	71 667	56 888	9 437	8 735

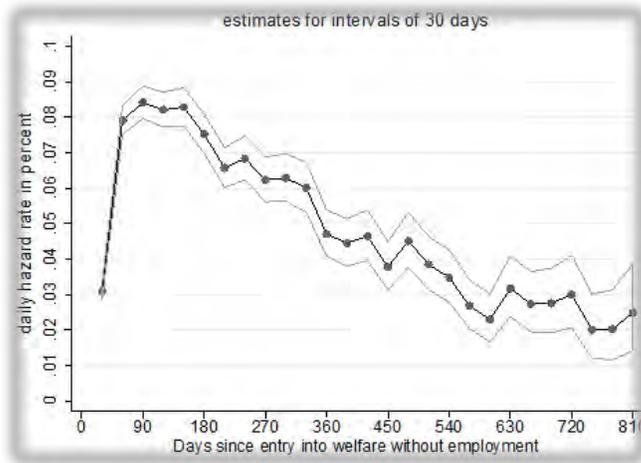
Source: Own calculations with administrative micro data.

Note: ^{a)} Contributory jobs exclude vocational training.

Table 4 displays the share of exits into the first strong and first mild sanction. Around 13 percent of our observations receive a strong sanction during their welfare spell. The share of individuals with a mild sanction is slightly lower (around 12 percent). More than one third of the individuals who receive a strong sanction also receive at least one mild sanction during their welfare spell. The same holds for mild sanctions: around 39 percent of the individuals with a mild sanction receive at least one strong sanction during their welfare spell. For around one third of the welfare spells, we observe a transition into an unsubsidized job. The table additionally indicates that among the sanctioned individuals, the share of individuals entering an unsubsidized contributory job is lower than for those people who were not sanctioned. This holds for strong and for mild sanctions, and it might be explained by the dynamic selection into the treatments: those with relatively low job finding perspectives stay longer in welfare and might be sanctioned with a higher probability.

⁷ Our results are robust if we use EUR 400 as an alternative threshold for the definition of employment.

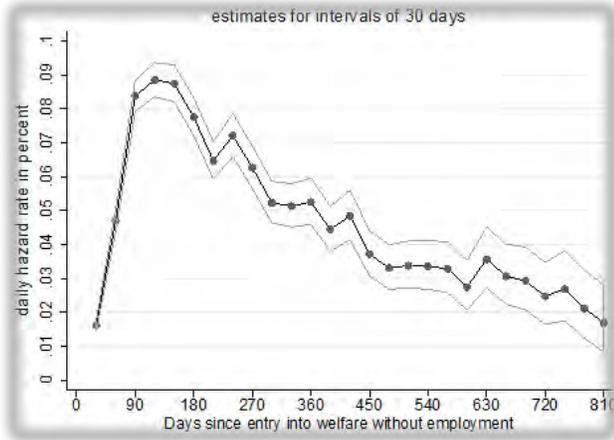
Figure 1. Empirical transition rate into the first strong punitive sanction (with a 95 percent confidence band)



Source: Own calculations with administrative micro data.

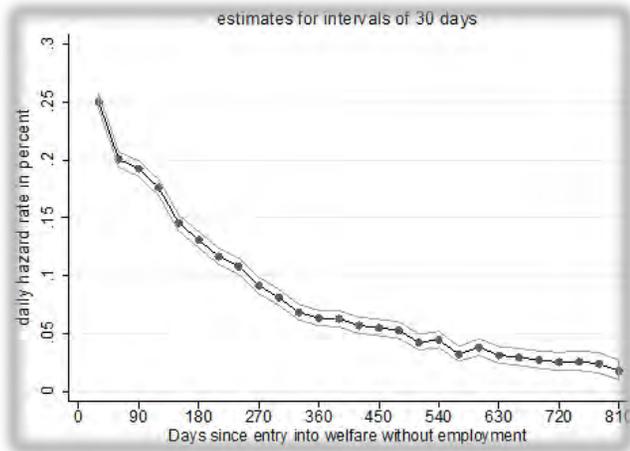
Figure 1 plots the transition rates into a first strong sanction against spell length. The transition rate is sharply increasing at the beginning of the welfare spells and afterwards it tends to fall over time spent in welfare. The same holds for the mild sanctions (see Figure 2). Figure 3 displays the empirical transition rate from welfare to work and suggests a negative duration dependence, i.e., a decreasing job finding probability with respect to elapsed time spent in social welfare. However, this might at least partly be explained by sorting over time; individuals with observed and unobserved characteristics, which go along with a low transition probability to employment, stay longer in welfare.

Figure 2. Empirical transition rate into the first weak punitive sanction (with a 95 percent confidence band)



Source: Own calculations with administrative micro data.

Figure 3. Empirical transition rate into unsubsidized contributory jobs (with a 95 percent confidence band)



Source: Own calculations with administrative micro data.

Table 5 displays sample averages of a selection of characteristics for non-sanctioned and sanctioned welfare recipients. The statistics refer to the characteristics at the start of the welfare spells in our sample. For

example, the sanctioned welfare recipients tend to be younger than those not facing a sanction. Moreover, the share of individuals who are below 20 years old is higher among those individuals who experience a strong sanction during their welfare receipt than among those who experience a mild sanction. Sanctioned individuals are also more frequently singles, of German nationality and they do, on average, have a lower skill level.

Table 5. Sample averages of selected characteristics^{a)}

	Never sanctioned	With strong sanction	With mild sanction
Number of observations	56 888	9 437	8 735
Age-distribution:			
18 years	0.101	0.149	0.133
19 years	0.117	0.159	0.146
20 years	0.131	0.150	0.160
21 years	0.137	0.148	0.154
22 years	0.147	0.156	0.158
23 years	0.170	0.151	0.148
24 years	0.198	0.087	0.101
Family status:			
single	0.790	0.847	0.853
married	0.084	0.038	0.033
not married but living with partner	0.120	0.109	0.107
separated, widow, divorced	0.007	0.005	0.007
Nationality:			
German	0.802	0.837	0.830
Turkish	0.077	0.070	0.072
other foreigner	0.121	0.093	0.098
Disability	0.014	0.006	0.006
Education:			
no occupational degree, no schooling degree	0.152	0.236	0.225
no occupational degree, low schooling degree	0.407	0.475	0.470
no occupational degree, high schooling degree	0.028	0.010	0.014
voc. training, without high schooling degree	0.201	0.101	0.109
voc. training, high schooling degree	0.017	0.004	0.004
technical university, university degree	0.007	0.004	0.003
education missing	0.188	0.170	0.175
Number of own children:			
aged less than 3 years	0.092	0.077	0.069
aged 3 to 5 years	0.027	0.020	0.021
aged 6 to 17 years	0.018	0.019	0.017
Regularly employed in last year:			
no	0.449	0.477	0.520
up to 1 quarter	0.254	0.289	0.265
> 1 quarter up to 2 quarters	0.152	0.136	0.130
> 2 quarters up to 3 quarters	0.079	0.060	0.055

Source: Own calculations with administrative micro data.

Note: ^{a)} Measured at the start of their spell.

4. Empirical Approach

We are interested in measuring the causal impact of the imposition of mild and strong sanctions on the duration of welfare receipt until taking up unsubsidized employment. Treated and untreated individuals might differ with respect to observed and unobserved characteristics. In order to measure the causal effects of strong and mild sanctions, we have to control for the selection process over time. We apply the “timing of events” approach (Abbring and van den Berg, 2003) – which is the standard approach in the literature on sanction effects – to a setting with two treatments, i.e., we estimate a mixed proportional hazard rate model with two dynamic treatments and one destination state. Transitions to alternative states like “out of the labor force” are treated as independent right-censoring. This implies the assumption that unobserved characteristics influencing the transition to work and the transition out of the labor force are uncorrelated.

We observe an inflow sample into welfare receipt. We assume that all individual differences in the probability of finding a job at time t can be characterized by observed characteristics x , unobserved characteristics V_e , and a sanction effect α , if a sanction has been imposed before t . Also the duration until a strong and a mild sanction depends on observed characteristics x , the elapsed time t spent in social welfare, and unobserved characteristics V_s and V_m , respectively. We specify the transition rate from welfare receipt without employment to a job $\theta_e(t)$ and the transition rates into a strong and a mild sanction $\theta_s(t)$ and $\theta_m(t)$ flexibly as piecewise constant exponential hazard rate models:

$$\begin{aligned}
 \theta_e(t) &= \exp\left(\sum_{j=2}^J I_j(t)\lambda_{je} + x'_t \beta_e + I_s(t > t_s)\alpha_s + I_m(t > t_m)\alpha_m + V_e\right) \\
 \theta_s(t) &= \exp\left(\sum_{j=2}^J I_j(t)\lambda_{js} + x'_t \beta_s + V_s\right) \\
 \theta_m(t) &= \exp\left(\sum_{j=2}^J I_j(t)\lambda_{jm} + x'_t \beta_m + V_m\right).
 \end{aligned}
 \tag{1}$$

$I_j(t)$ takes on the value of one if t is in the interval j . λ_{je} , λ_{js} and λ_{jm} describe the interval-specific baseline hazard rates for J intervals. $I_s(t > t_s)$

and $I_m(t > t_m)$ take on the value of one if $t > t_s$ and $t > t_m$, respectively. t_s is the day of the first strong sanction, while t_m is the day of the first mild sanction. α_s captures the effect of the first strong sanction on the transition rate into a job; α_m corresponds to the effect of the first mild sanction on the hazard rate to unsubsidized employment. We assume that a sanction does not affect the transition rate before the moment of the sanction. This assumption is referred to as the no-anticipation assumption. In the case of sanctions, this assumption is likely to hold, since the welfare recipient cannot anticipate the exact moment when a caseworker imposes a sanction, see Section 2. It is important to note that the individuals are allowed to know the probability distribution of future events conditional on observed and unobserved characteristics, but they are not allowed to know the exact timing of future events.⁸ Individuals might be sanctioned several times during their welfare receipt. In this paper, we focus on the impact of the first sanctions and ignore repeated sanctions. However, our empirical approach takes into account that some individuals might receive a strong and a mild sanction during their welfare spell. Moreover, we assume that the unobserved heterogeneity terms V_e , V_s and V_m are constant over time, and that V_e , V_s and V_m are uncorrelated with the observed characteristics x .

4.1 Distribution of the unobserved heterogeneity

We specify the distribution of unobserved heterogeneity G to have a discrete support with M support points. In order to force the corresponding probabilities to be between zero and one and to sum to one, we use a multinomial logit parameterization of the class probabilities:

$$\pi_m = \frac{\exp(\omega_m)}{\sum_{m=1}^M \exp(\omega_m)}, m = 1, \dots, M, \quad \omega_1 = 0. \quad (2)$$

⁸ Individuals might have additional information about the probability of being sanctioned, for example due to notification by the case worker. If this leads to a change in their job search behavior, this could lead to biased estimates. We have no information about notification dates. However, given that the period between notification and the imposition of a sanction is rather short, we do not believe that this creates a major problem for our analysis.

Each of the equation specific components of the unobserved heterogeneity V takes on a specific value at support point m . This implies that for a model with $M = 2$, G would be described by 4 parameters, for $M = 3$ we estimate 8 parameters, etc. This approach allows for a flexible covariance matrix for the unobserved components. For a similar model for unobserved heterogeneity in the context of timing of events models, see Crépon et al. (2013) and in the context of random coefficient models in the statistical literature, see e.g. Aitkin (1999). Gaure et al. (2007) provide Monte Carlo evidence that modeling selection based on unobservables by a flexible discrete distribution works well in the context of timing of events models. In the estimation, we increase the number of mass points until the model fit can no longer be improved by a further mass point, evaluated on the basis of the Akaike Criterion.

4.2 Likelihood function

Given this setup, the likelihood contribution of an individual i with an observed welfare spell duration t and a strong (mild) sanction imposed at t_s (t_m) for given unobserved and observed characteristics V and x is given by:

$$L_i(x, V) = \theta_e(t)^{\delta_e} S_e(t | x, V_e, t_s, t_m) \theta_s(t_s)^{\delta_s} S_s(t_s | x, V_s) \theta_m(t_m)^{\delta_m} S_m(t_m | x, V_m). \quad (3)$$

The indicators δ_e , δ_s and δ_m take on the value of one if a transition to regular employment, a strong sanction and a mild sanction, respectively, is observed and zero otherwise. Since we do not know the unobserved characteristics for an individual i , the “unconditional” log-likelihood contribution corresponds to the weighted sum of the contributions corresponding to the M points of support. The log-likelihood function for the sample with N individuals is given by:

$$\ln L = \sum_{i=1}^N \ln \sum_{m=1}^M \pi_m L_i(x, V(m)). \quad (4)$$

5. Results

In Table 6, we report the treatment effects for a model without (Model 1) and with (Model 2) controlling for dynamic selection based on unobserved heterogeneity. In both models, we control for observed characteristics as reported in Table 5 and allow for flexible duration dependencies for the duration in welfare receipt. Moreover, we control for the month in which the welfare spell starts and include time-varying indicators for the current quarter to capture seasonal effects. Our final specification for Model 2 includes 5 support points ($M=5$), which implies that we estimate 16 additional parameters for the distribution of unobserved characteristics as compared to a model without unobserved heterogeneity. A further increase in the number of support points does not lead to any further improvement of the model fit, evaluated on the basis of the Akaike Criterion.

Table 6. Baseline estimation results for the hazard rate to unsubsidized employment

	Model 1	Model 2
Strong sanction	0.185*** (0.022)	0.782*** (0.095)
Mild sanction	0.071*** (0.024)	0.312*** (0.094)
Unobserved het. ($M=5$)	No	Yes
Log-likelihood	-332 774.81	-331 939.05

Source: Own calculations with administrative micro data.

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation includes control variables for duration dependence, seasonal dummies, individual characteristics and local macroeconomic conditions including the local labor unemployment rate. The complete set of coefficients for Model 2 including the distribution of the unobserved heterogeneity is reported in the Appendix in Table A.1.

In a first step, we estimate a model with homogenous treatment effects. The results indicate that the imposed sanctions lead to a significant increase in the transition rate from welfare to work. This holds for strong and for mild sanctions. The corresponding coefficients of the two treatment dummies are significant in the models without and with controlling for unobserved heterogeneity. However, once we control for the dynamic selection based on unobserved characteristics, the coefficients indicate a much stronger impact of imposed sanctions. The coefficients for both types of sanctions are significantly different from each other. While the coefficient for the strong sanction indicates an increase in the transition

rate from welfare to work by around 120 percent, the imposition of a mild sanction leads to an increase in the hazard rate to unsubsidized jobs by around 37 percent.⁹

In a second step, we allow for time varying treatment effects. For this purpose, we additionally introduce two dummy variables indicating whether the sanctions are imposed after 7 to 12 months and after more than 12 months of elapsed duration of welfare receipt, respectively. The corresponding results are reported in Table 7. None of the coefficients capturing potential time-varying effects of imposed sanctions is significantly different from zero. This suggests that imposed sanctions have the same positive impact on the hazard rate from welfare to work, independent of the timing of the imposition of the sanction.

Table 7. Treatment effect heterogeneity with respect to the timing of the imposed sanction

	Model 3
Strong sanction	0.702*** (0.121)
Strong Sanction x 7-12 months	0.082 (0.080)
Strong Sanction x 13-36 months	0.136 (0.132)
Mild sanction	0.275*** (0.111)
Mild sanction x 7-12 months	-.024 (0.074)
Mild sanction x 13-36 months	0.010 (0.124)
Unobserved het. (M=5)	Yes
Log-likelihood	-331 937.92

Source: Own calculations with administrative micro data.

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation includes control variables for duration dependence, seasonal dummies, month of entry into social welfare, individual characteristics and local macroeconomic conditions including the local labor unemployment rate. The complete set of coefficients including the distribution of the unobserved heterogeneity is available upon request.

Since strong sanctions involve a much larger reduction in benefit payments than mild sanctions, it is not surprising that we see a larger

⁹ The size of the impact of mild sanctions is within the range of previously reported estimates. Van den Berg et al. (2004) and van der Klaauw and van Ours (2013) estimate the effects of sanctions implying a benefit reduction between 5 percent and 20 percent of the benefit level and usually lasting between 1 and 2 months. Their findings suggest an increase in the transition rate to work by more than 100 percent (van den Berg et al., 2004) and by 21 percent (van der Klaauw and van Ours, 2013), respectively.

effect of this type of sanctions on the hazard rate to work. After all, we expect a more pronounced decrease in the reservation wage and a stronger increase in the search effort by the job seeker. However, the effect of a mild sanction is much larger than 10 percent of the effect of a strong sanction, while the benefits reduction in the latter case is about ten times the reduction in the former case. This suggests that part of the effect of the mild sanctions is due to the fear of intensified monitoring after the first punishment. In reality, monitoring may be more strongly intensified after a strong sanction than after a weak sanction, but this would merely reinforce our conclusion. As a caveat, notice that job search theory does not provide a justification for the assumption that the elasticity of the transition rate to work with respect to the benefits level is a constant (see e.g. van den Berg, 2001).

Overall, our results suggest that the imposition of sanctions pushes the welfare recipients to work. This suggests that sanctions help reduce welfare dependency and increase the employment prospects of sanctioned individuals. However, it is important to note that the quality of the jobs taken up by sanctioned and not sanctioned individuals might differ, see e.g. van den Berg and Vikström (2013) for corresponding evidence for sanctions in the unemployment insurance system in Sweden. The analysis of the impact of sanctions on job quality and long-term employment prospects goes beyond the scope of this paper, but it is an important dimension of the policy evaluation of sanctions in social welfare systems.

6. Conclusions

In this paper, we give an overview of the literature on sanctions in social welfare systems and analyze the impact of strong and mild sanctions for young welfare recipients in Germany on the hazard rate to unsubsidized employment. We use detailed administrative data and estimate the timing of events models to control for selection into treatments based on unobserved characteristics.

Our results suggest that both types of sanctions lead to a higher transition rate to work, and that this effect is larger for strong sanctions. The findings indicate that part of the sanction effects is due to the fear of intensified monitoring after the punishment. This suggests that in the case

of a first punishment during a welfare spell, it is not necessary to give the maximum possible sanction, in the sense that a less strong sanction also has a strong effect on the transition rate to work, while having a smaller disutility cost for the individual. This has the additional advantage that case workers with a personal bond to their clients may be less reluctant to issue a sanction if the benefits reduction is modest. The determination of the optimal size of a sanction is beyond the scope of our paper, but is an interesting topic for further research.

We find that the effects of sanctions do not depend on the moment in the welfare spell that they are imposed. We did not investigate effects on post-welfare outcomes. Strong sanctions imply a complete cut of the basic cash transfer payments, and there exists evidence that severe sanctions may go along with adverse post-unemployment effects for sanctioned individuals. Therefore, future research should investigate to what extent strong sanctions for young welfare recipients lead to jobs with lower wages and a lower future wage growth. Moreover, for the evaluation of strong sanctions for young job seekers, it is important to know whether sanctioned welfare recipients have a higher probability of leaving the labor force for some time, which might lead to less stable employment paths and a higher probability of welfare dependency in the future.

References

- Abbring, J.H. and van den Berg, G.J. (2003), The non-parametric identification of treatment effects in duration models, *Econometrica* 71, 1491-1517.
- Abbring, J.H., van den Berg, G.J. and van Ours, J.C. (2005), The effect of unemployment insurance sanctions on the transition rate from unemployment to employment, *Economic Journal* 115, 602-630.
- Aitkin, M. (1999), A general maximum likelihood analysis of variance components in generalized linear models, *Biometrics* 55, 117-128.
- Arni, P., Lalive, R. and van Ours, J.C. (2013), How effective are unemployment benefit sanctions? Looking beyond unemployment exit, forthcoming in *Journal of Applied Econometrics*.
- van den Berg, G.J. (2001), Duration models: Specification, identification, and multiple durations, in J.J. Heckman and E. Leamer (eds.), *Handbook of Econometrics V*, North Holland, Amsterdam.
- van den Berg, G.J., Hofmann, B. and Uhlendorff, A. (2013), The role of sickness in the evaluation of sanctions and job search assistance, working paper, University of Mannheim.

- van den Berg, G.J., van der Klaauw, B. and van Ours, J.C. (2004), Punitive sanctions and the transition rate from welfare to work, *Journal of Labor Economics* 22, 211-241.
- van den Berg, G.J. and Vikström, J. (2013), Monitoring job offer decisions, punishments, exit to work, and job quality, forthcoming in *Scandinavian Journal of Economics*.
- Boockmann, B., Thomsen, S.L. and Walter, T. (2009), Intensifying the use of benefit sanctions – An effective tool to shorten welfare receipt and speed up transitions to employment, ZEW Discussion Paper 09-072, Mannheim.
- Cockx, B., Dejemeppe, M., Launov, A. and van der Linden, B. (2011), Monitoring, sanctions and front-loading of job search in a non-stationary model, IZA Discussion Paper 6181, Bonn.
- Crépon, B., Ferracci, M., Jolivet, G. and van den Berg, G.J. (2013), Dynamic treatment evaluation using data on information shocks, working paper, CREST-INSEE.
- Gaure, S., Roed, K. and Zhang, T. (2007), Time and causality: A Monte Carlo assessment of the timing-of-events approach, *Journal of Econometrics* 141, 1159-1195.
- Götz, S., Ludwig-Mayerhofer, W. and Schreyer, F. (2010), Sanktionen im SGB II: Unter dem Existenzminimum, IAB-Kurzbericht 10/2010, Nuremberg.
- Hillmann, K., and Hohenleitner, I. (2012), Impact of benefit sanctions on unemployment outflow - Evidence from German survey data, HWWI Research Paper 129.
- Hohmeyer, K. and Wolff, J. (2012), A fistful of euros: Is the German one-euro job workfare scheme effective for participants?, *International Journal of Social Welfare* 21, 174-185.
- Immervoll, H. (2009), Minimum-income benefits in OECD countries: Policy design, effectiveness and challenges, IZA Discussion Paper 4627, Bonn.
- van der Klaauw, B. and van Ours, J. (2013), Carrot and stick: How re-employment bonuses and benefit sanctions affect exit rates from welfare, *Journal of Applied Econometrics* 28, 275-296.
- Lalive, R., van Ours, J. and Zweimüller, J. (2005), The effect of benefit sanctions on the duration of unemployment, *Journal of the European Economic Association* 3, 1386-1417.
- McVicar, D. and Podivinsky, J.M. (2009), How well has the new deal for young people worked in the UK regions?, *Scottish Journal of Political Economy* 56, 167-195.
- Nivorozhkin, A. and Wolff, J. (2012), Give them a break! Did activation of young welfare recipients overshoot in Germany? (A regression discontinuity analysis), working paper, Institute for Employment Research, Nuremberg.
- Roed, K. and Weslie, L. (2012), Unemployment insurance in welfare states: The impacts of soft duration constraints, *Journal of the European Economic Association* 10, 518-554.
- Schneider, J. (2008), The effect of unemployment benefit II sanctions on reservation wages, IAB Discussion Paper 19/2008, Nuremberg.
- Schneider, J. (2009), Activation of Welfare Recipients: Impacts of Selected Policies on Reservation Wages, Search Effort, Re-employment, and Health, Ph.D. thesis, Department of Economics, Free University, Berlin.
- Schreyer, F., Zahradnik, F. and Götz, S. (2012), Lebensbedingungen und Teilhabe von jungen sanktionierten Arbeitslosen im SGB II, *Sozialer Fortschritt* 61, 213-220.

Svarer, M. (2011), The effect of sanctions on exit from unemployment: Evidence from Denmark, *Economica* 78, 751-778.

Wolff, J. and Moczall, A. (2012), Übergänge von ALG-II-Beziehern in die erste Sanktion – Frauen werden nur selten sanktioniert, IAB Research Report 11/2012, Nuremberg.

Appendix

Table A.1. Estimation results of the baseline model with unobserved heterogeneity

	Hazard to employment		Hazard rate to first strong sanction		Hazard rate to first mild sanction	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Constant	-5.130515	0.158080	-6.880703	0.203792	-7.216254	0.184075
Months (4-6)	-0.083919	0.023334	0.246107	0.028023	0.594404	0.030092
Months (7-9)	-0.279043	0.034329	0.157233	0.039584	0.443427	0.041883
Months (10-12)	-0.598340	0.045046	0.077548	0.052776	0.271400	0.055682
Months (13-15)	-0.696361	0.054301	-0.098624	0.069530	0.160535	0.069454
Months (16-18)	-0.878425	0.064484	-0.106717	0.084422	-0.030784	0.087062
Months (19-21)	-1.052770	0.079293	-0.401210	0.111219	0.027233	0.101659
Months (22+)	-1.230289	0.084511	-0.385069	0.117476	-0.117241	0.111257
Strong sanction	0.782172	0.094551	-	-	-	-
Mild sanction	0.311632	0.094106	-	-	-	-
Unemployment rate	-0.028918	0.011435	0.068598	0.016713	0.097659	0.016696
Long-term unemployment rate	-0.049925	0.018770	-0.175495	0.027678	-0.184822	0.027831
Vacancy-unemployment ratio	1.300257	0.122082	-0.121874	0.190038	0.501887	0.193653
19 years old	0.126459	0.036641	-0.174940	0.045003	-0.069329	0.045452
20 years old	0.271460	0.036041	-0.371208	0.047387	-0.035765	0.046270
21 years old	0.343946	0.035852	-0.372906	0.047889	-0.050034	0.047156
22 years old	0.369640	0.035984	-0.337617	0.048691	-0.044487	0.047711
23 years old	0.361069	0.035945	-0.394205	0.048816	-0.142131	0.048469
24 years old	0.315363	0.036865	-0.456664	0.052593	-0.123432	0.052907
Married	0.569860	0.044840	-0.701120	0.084487	-1.004373	0.090916
Not married but living with a partner	0.410107	0.036748	-0.180760	0.062590	-0.269523	0.064204
Separated, widowed, divorced	0.316058	0.093308	-0.039931	0.151940	0.142763	0.143225
Turkish nationality	0.351525	0.031019	-0.069834	0.047443	-0.010066	0.046231
Other foreigner	0.275137	0.025684	-0.241262	0.041053	-0.167059	0.040111
Disabled	-0.598388	0.081799	-1.043968	0.140925	-1.070423	0.149979

Table A.1. Conitnued....

	Hazard to employment		Hazard rate to first strong sanction		Hazard rate to first mild sanction	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
No occ. degree, no schooling	-0.974984	0.030245	0.612680	0.049243	0.419112	0.047844
No occ. degree, low schooling	-0.836541	0.024563	0.512844	0.043328	0.351232	0.042219
No occ. degree, high schooling	-0.836541	0.024563	0.512844	0.043328	0.351232	0.042219
Voc. training, no high schooling	-0.034326	0.065461	-0.655787	0.179194	-0.704207	0.179111
University degree	-0.888921	0.109175	0.083861	0.189785	-0.231612	0.206156
Education missing	-1.066053	0.032631	0.044950	0.052843	0.066901	0.053210
No. of children < 3 years	0.052594	0.026167	0.128825	0.045499	0.141447	0.047252
No. of children between 3-5 years	-0.002592	0.041586	0.083750	0.069756	0.132720	0.075947
No. of children between 6-17 years	-0.091246	0.050606	0.076187	0.077220	0.084351	0.083071
Partner younger than 20	-0.156885	0.046080	0.100165	0.076811	0.135341	0.077171
Partner between 25-29 years	0.036150	0.048532	-0.094228	0.094973	-0.106401	0.099005
Partner between 30-34 years	-0.232888	0.114819	-0.009697	0.188350	-0.521382	0.227006
Partner older than 34 years	0.037653	0.141078	0.234258	0.269624	0.060685	0.286221
Partner foreigner	-0.146464	0.045723	0.002087	0.084725	0.013801	0.089221
Partner no occ. degree, no schooling	-0.080288	0.058435	0.222546	0.096258	0.272048	0.097745
Partner no occ. degree, low schooling	0.123997	0.041032	0.046355	0.074059	0.060349	0.075810
Partner no occ. degree, high schooling	0.087996	0.113328	-0.351585	0.238024	-0.344974	0.271434
Partner education missing value	-0.079421	0.049024	0.037398	0.089641	0.135388	0.090826
Unobserved Heterogeneity						
V2	1.272058	0.213185	-3.517990	4.065140	-3.267671	3.420475
V3	-2.295400	0.145296	0.691409	0.236896	0.234630	0.217350
V4	-0.153988	0.577736	-2.633537	1.947935	-1.783316	0.667200
V5	-1.397236	0.516297	-1.146243	0.568552	-1.562899	0.478757
ω_2	-0.777491	0.416161				
ω_3	-0.456368	0.336561				
ω_4	-0.131856	0.662975				
ω_5	-0.158201	0.653638				

Source: Own calculations with administrative micro data.

Note: The estimation additionally includes month of entry into social welfare and dummies for the different counties (Bundesländer).

Comment on van den Berg, Uhlendorff and Wolff: Effects of sanctions for young welfare recipients

Johan Vikström*

The paper by van den Berg, Uhlendorff and Wolff provides new empirical evidence on the effects of sanctions for welfare recipients and presents an insightful overview of the current literature. The authors find that actually imposed sanctions increase the exit rate to work (the job finding rate), reduce job quality and increase the exit rate out of the labor force. These are the three key results of the paper. All three findings constitute important contributions to the literature and all three are of great importance for both economists and policymakers. The paper is also very well executed. In this comment, I initially discuss the validity, interpretation and policy implications of these three key findings. Then, I discuss two broader questions: could we expect differential effects from strong and mild sanctions and are threat effects due to the mere threat of a sanction more important than the effects of actually imposed sanctions?

The first key result of the paper is that a sanction in the form of a three-month benefit loss increases the exit rate to work by about 104 percent. During the three-month period, benefits are limited to the cost of rent and heating for first time offenders and fully withdrawn for repeated violations within one year, i.e. rather strong sanctions. Even though the paper considers strong sanctions, the effect on the exit rate is arguably very large. As an illustration, take an individual with 20 months expected time on welfare without a sanction and, for simplicity, assume that s(he)

* IFAU and UCLS, johan.vikstrom@ifau.uu.se.

has a constant exit rate to work. If this individual were given a sanction after nine months, the expected time on welfare time would drop from 20 months to 14.6 months. Is this an unreasonably large effect? Let us consider this question by comparing the size of the effect with estimates for older welfare recipients and unemployed individuals. We use the same simplified example as above and present calculations using the main estimates from studies from different countries in Table 1. Most of these studies report large effects of sanctions, and the effects in this paper are of the same magnitude as those found in several other papers. However, there are also striking differences across countries. One explanation could, of course, be differences in benefit rules, but these differences also suggest that the effects for young welfare recipients could vary substantially across countries. More evidence from other countries therefore seems to be crucial before launching extended sanctions in the Nordic countries.

The large effect also warrants a discussion about the empirical strategy. The authors base their analysis on the Timing-of-Events (ToE) approach developed by Abbring and van den Berg (2003). The same method has been used in most recent papers on the effects of benefit sanctions and it could therefore be considered to be the standard method for the evaluation of the effects of benefit sanctions. One reason is that a credible analysis of the effects of sanctions is difficult and no other method besides the ToE approach is applicable. In that sense, the paper applies state-of-the-art techniques. However, the approach still relies on a number of assumptions, including parametric restrictions. The implication is that the size of the effect on exit to work is estimated under some reservations and associated with some uncertainty.

The result that a sanction leads to more exits from the labor force is the second main result of the paper. The authors find that the exit rate into the partly unknown “Out of Labor Force State” increases by 89 percent. This is an entirely expected adverse effect of sanctions. From a theoretical perspective, sanctions might lead to increased search efforts and reduced reservation wages, which in turn lead to an increased exit rate to work. A sanction might also discourage some welfare recipients, leading to exits from the labor force, black market work, criminality and so on. Examining the effects on exits out of the labor force is therefore of great policy relevance. It is also crucial to attempt to uncover whether exits out

of the labor force imply black market work, crime or just general inactivity. In this paper, out of the labor force implies at least four months without employment, program participation, UI benefits or minor employment. The authors also examine the status of these individuals 6-12 months after their exits out of the labor force and conclude that six months after the exit the status is unknown for 86 percent. This is a large share and any additional information about these individuals would be informative for understanding the full effects of sanctions on young welfare recipients. At the same time, note that the monthly exit rate out of the labor force on average is about 0.02, while the exit rate to work is about 0.1. This means that even if the effect on the exit rate out of the labor force is large in relative terms, it is quantitatively less important compared to the effect on the exit rate to work, since the latter increases from a much higher initial level.

The third key result is that actually imposed sanctions decrease the wage rate in the first job after unemployment with about 4.3 percent. The authors interpret this finding as an effect on job quality and report it as a negative effect of sanctions. I believe this interpretation to be overly simplified for several reasons. First, while an initially lower paid job could serve as a stepping stone to a better paid job, those sanctioned could also get stuck in less qualified occupations, implying that the lower initial wage has long-lasting impacts. This means that the policy implications of an initially lower wage almost completely depend on the long-run effects, which are not studied in the paper. Second, interpreting a wage effect purely as a job quality effect is problematic, especially if job quality is referred to as the quality of the employer–employee match. The reason is that a lower wage could entirely be caused by decreased bargaining power, meaning that sanctioned individuals obtain similar jobs as individuals without sanctions, but at a slightly lower wage rate. However, could the wage effect be a result of desperation following the benefit loss, suggesting that sanctioned individuals quickly accept any job, even a job with much lower qualification requirements? In the latter case, one could talk about a job quality effect; however, it is not necessarily so in the former case. Moreover, since the paper does not investigate the effects on, say, the occupational level, the conclusion that there are negative job quality effects is not necessarily supported by the empirical evidence. However, note that van den Berg and Vikström (2013) document effects on the

occupational level as well as the long-run wage effects following sanctions for unemployed individuals, which lends some support to the interpretation in the paper.

Another important policy aspect is the choice between strict and mild sanctions. This paper only considers strong sanctions (benefits limited to the cost of rent or full withdrawal). The German system also includes milder sanctions. The authors describe that a missed appointment leads to a 10 percent reduction of the benefits. From a policy perspective, this might be the most important area for future analyses, since any knowledge about the relative effect of strong and mild sanctions is crucial when designing sanction systems in different countries. It might, for instance, be the case that strong and mild sanctions have similar effects on the exit rate to work, while strong sanctions have a larger impact on the exit rate out of the labor force and wage rates.

The paper focuses entirely on the effects of actually imposed sanctions, often referred to as *ex-post* effects in the literature. *Ex-post* effects are very interesting, but probably more important are threat effects due to the mere threat of a sanction (so-called *ex-ante* effects). Any such threat effects would affect the search behavior of all welfare recipients. I completely understand that studying threat effects is difficult, but any discussion of such effects would be enlightening, especially since *ex-post* effects are most likely more policy-relevant. In addition, it may be possible to study *ex-ante* effects for German welfare recipients using, say, some regional variation in monitoring strictness.

Table 1. Illustration of employment effects of a benefit sanction after 9 months

	Time in unemployment
Baseline case no sanction	20 months
Welfare recipients	
Germany (this paper)	14.6 months
Netherlands (van den Berg et al., 2004)	13.7 months
Unemployment insurance	
Sweden (van den Berg et al., 2013)	18.0 months
Denmark (Svarer, 2011)	14.1 months
Netherlands (Abbring et al., 2005)	17.3 months
Switzerland (Arni et al., 2013)	15.9-17.1 months

Source: Own simulations of the effect of a sanction given after 9 months in unemployment on expected time in unemployment.

Notes: Calculations based on an individual with a constant monthly exit rate to work without a sanction equal to 0.05 and the estimated effects on this exit rate from different studies.

References

- Abbring, J.H. and van den Berg, G.J. (2003), The non-parametric identification of treatment effects in duration models, *Econometrica* 71, 1491-1517.
- Abbring, J.H., van den Berg, G.J. and van Ours, J.C. (2005), The effect of unemployment insurance sanctions on the transition rate from unemployment to employment, *Economic Journal* 115, 602-630.
- Arni, A., Lalive, R. and van Ours, J.C. (2013), How effective are unemployment benefit sanctions? Looking beyond unemployment exit, forthcoming in *Journal of Applied Econometrics*.
- van den Berg, G.J., van der Klaauw, B. and van Ours, J.C. (2004), Punitive sanctions and the transition rate from welfare to work, *Journal of Labor Economics* 22, 211-241.
- van den Berg, G.J. and Vikström, J. (2013), Monitoring job offer decisions, punishments, exit to work, and job quality, forthcoming in *Scandinavian Journal of Economics*.
- Svarer, M. (2011), The effect of sanctions on exit from unemployment: Evidence from Denmark, *Economica* 78, 751-778.

Can active labour market policies combat youth unemployment?

Jonas Maibom^{*}, Michael Rosholm^{**} and Michael Svarer^{***}

Summary

Active labour market policies (ALMPs) may play an important role in preventing an increase in long-term unemployment following the Great Recession. We consider this issue for Denmark, a country relying extensively on this instrument. We present evidence on the effectiveness of ALMPs as a way of fighting youth unemployment using results from a randomised controlled trial (RCT) that intensified the use of ALMPs. The intervention was conducted after the onset of the financial crisis, and the findings are relatively unfavourable in the sense that further intensification of an already quite intensive effort for youth did not increase employment. In addition, the intensification of ALMPs seems to have increased transitions into sickness benefits.

Keywords: youth unemployment, active labour market programmes, random controlled experiments.

JEL classification numbers: J0, J64.

^{*} Department of Economics and Business, Aarhus University, maibom@econ.au.dk.

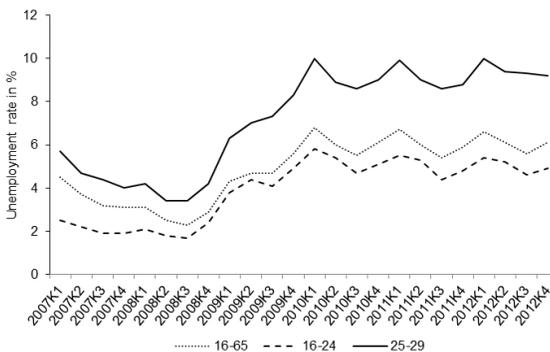
^{**} TrygFondens Centre for Child Research and Department of Economics and Business, Aarhus University, rom@asb.dk.

^{***} TrygFondens Centre for Child Research and Department of Economics and Business, Aarhus University, msvarer@econ.au.dk.

The Great Recession has had a large labour market impact, causing a steep decrease in employment rates, in many countries. For given macro-economic and institutional settings, the overriding task for labour market policies in this case is to reduce the extent to which the resulting rise in unemployment translates into an increase in long-term unemployment and the structural unemployment rate. Experience from the high unemployment period in the 1970's and 1980's shows how steep increases in unemployment can translate into increases in structural unemployment and, further, how difficult it may be to subsequently bring down the structural unemployment rate – in Denmark this process lasted almost 15 years.

Prior to the Great Recession, the Danish labour market was booming, to the point of overheating. Employment was historically high (in fact, much higher than what was justified by production), and unemployment was extremely low, more than 2 percentage points below the estimated structural rate. The financial crisis therefore initiated a process of adjustment towards lower employment and higher unemployment. Part of which was bound to happen anyway. But, clearly, the magnitude of this realignment was fortified by the crisis.

Figure 1. Gross unemployment rates for different age groups



Source: Statistics Denmark. Gross unemployment includes all unemployed recipients of UI benefits or social assistance, irrespective of whether they were openly unemployed or participating in active programmes.

Figure 1 reveals several relevant observations. First, the rise in youth unemployment has been dramatic, but from very low levels. Second, the level of youth unemployment strongly depends on the age of the individ-

uals. Unemployment for individuals aged below 25 is much lower than for young people aged 25-29 and is, in fact, lower than the overall unemployment rate. This might in part be explained by labour market policy, which is much stricter for individuals aged below 25 and in particular the benefit system, which is considerably less generous for those aged below 25. We will discuss these aspects in more detail later. Finally, the increase in unemployment measured in percentage points has been higher for youth than for older workers – reflecting the classical phenomenon that youth are more sensitive to business cycles than older workers (see e.g. Andersen et al., 2013).

In an international comparison (based on labour force surveys), youth unemployment is slightly below the OECD average for youth aged below 25. The unemployment rate is close to the OECD average for the 25-29 year old. The economic crisis has implied that the Danish unemployment rate is now close to the OECD average and this is also true for youth. In the current situation, it is therefore not the case that Denmark is doing particular well in terms of keeping unemployment rates low (see e.g. OECD, 2013).

The increase in unemployment – in particular the increase for youth – has received a great deal of policy attention, and a number of youth packages have been introduced to combat youth unemployment.¹ The case of youth unemployment is particularly interesting as active labour market policies (ALMP) for youth in Denmark serve dual objectives: education or employment, depending on which barriers an individual face. For unemployed individuals who have failed to obtain a professional/qualifying² education, ALMPs will often aim at preparing young individuals for obtaining such an education³ whereas unemployed youth with a professional education (youth at the second barrier) receive more traditional policies aimed at directly improving employment outcomes.

The purpose of this study is to describe the active labour market tools used to help unemployed youth return to education or employment and to assess the effectiveness of the different measures. The structure of our

¹ See e.g.

<http://www.bm.dk/da/Beskaeftigelsesomraadet/Flere%20i%20arbejde/Ungeindsats.aspx>.

² This means that they have not completed an education above upper sec-ondary education or vocational-oriented education and thereby, they do not have any formal skills beyond the basic ones.

³ This group is sometimes labelled youth at the first barrier in the literature.

study is as follows. First, we provide a short introduction to the Danish labour market with special focus on the use of active labour market policies and the rules that apply to youth. Second, we briefly review the literature on the effects of ALMPs for youth. Finally, we present the results from a randomised controlled trial (RCT) that was conducted in Denmark in 2009. The RCT was designed to test if further intensification of ALMPs towards youth would be successful in increasing employment for unemployed youth with a qualifying education, and whether similar programmes could also help increase the educational attainment for unemployed youth without a qualifying education (i.e. could the policy achieve dual goals?).

1. A short introduction to the Danish labour market

The Danish labour market is characterised by three distinct elements; low levels of job protection, relatively generous unemployment benefits, and an extensive use of ALMPs. Our focus in this paper is on the later part, but clearly the two other components are also important for unemployment, and the use of ALMPs must be seen in this context.

To explain ALMPs in Denmark, a few institutional details are in order. Unemployment insurance is a voluntary scheme based on membership fees and tax-financed subsidies.⁴ Individuals not eligible for unemployment insurance benefits (UIB) are eligible for Social Assistance (means tested at the household level).

All unemployed are categorized into so-called match groups based on an overall assessment of the potential for the individual of being employed (qualifications, experience, social situation etc.). The assessment is made by case workers at the job centre. This classification has three match-groups.⁵ The system applies to all recipients of temporary income transfers, i.e. unemployment benefits, social assistance, sickness-payment, flex-job etc.

⁴ The maximal benefit level is regulated by i) the fact that the replacement rate relative to the past wage income cannot exceed 90 per cent, and ii) a nominal cap (indexed to wage development). As a consequence, the effective replacement rate is declining in the wage and is on average about 65 per cent. The benefit duration has been shortened over the years and since 2011 it has been two years (2 ½ years). To remain eligible for unemployment benefits, there is a work requirement equal to 12 months within the last 36 months.

⁵ <https://www.retsinformation.dk/Forms/R0710.aspx?id=139870>.

- Group I (job-ready): Individuals with no problems beyond unemployment. A person who is available for a job and who can become self-supportive within three months.
- Group II (ready for activity): The individual is not ready to start working at present, but is capable of participating in a programme activity aiming at employment.
- Group III (temporarily passive): The individual is neither ready for a job nor for participation in a programme activity aiming at employment.

Individuals on UIB are automatically in group I. The rules for ALMP reviewed below apply to all individuals (whether on UIB or SA) in match group I. In the following, we distinguish between unemployed entitled to UIB – UI unemployed – and unemployed entitled to social assistance and in match group I – SA unemployed. The sum of UI- and SA-unemployed is denoted gross unemployment.

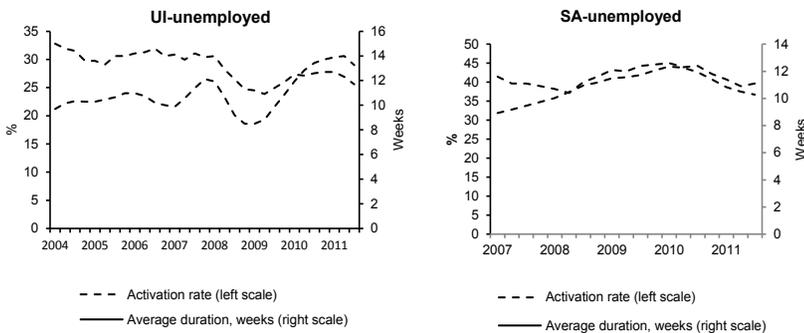
While the overall focus on ALMP has been strong since the mid-1990's, the specific design has been under more or less continuous change. Recently, part of this change can be subscribed to favourable results obtained from Danish RCTs, more on this issue later. The current system has two key elements (contact and activation):

- Contact: An unemployed (whether on UIB or SA) must have a CV available on the job centre website within the first four weeks of unemployment, and he/she must participate in interviews on job search and labour market availability at the job centre at least every third month.
- Unemployed have a right and an obligation to participate in an activation programme after nine months of unemployment, and thereafter every six months.
- For youth there are some special rules:
- Below the age of 30: First interview within one month, thereafter every three months.
- Below the age of 30: First activation (right and duty) after three months. If the person does not have a qualifying education (defined

above), the activation programme should be aiming at education in the ordinary educational system.

- Below the age of 25 without education and dependent children: it is mandatory to take some education; if not immediately suited for enrolment in the ordinary education system, activation should focus on improving the pre-conditions making this possible at a later stage. For individuals with a labour market relevant education, the activation should focus on enhancing the scope for ordinary work.
- Immediate activation of the very young (age group 18-19). Some municipalities have introduced immediate and full-time activation for very young individuals claiming SA.

Figure 2. Activation rate and average duration of activation activities – UI and SA unemployed



Source: www.jobindsats.dk.

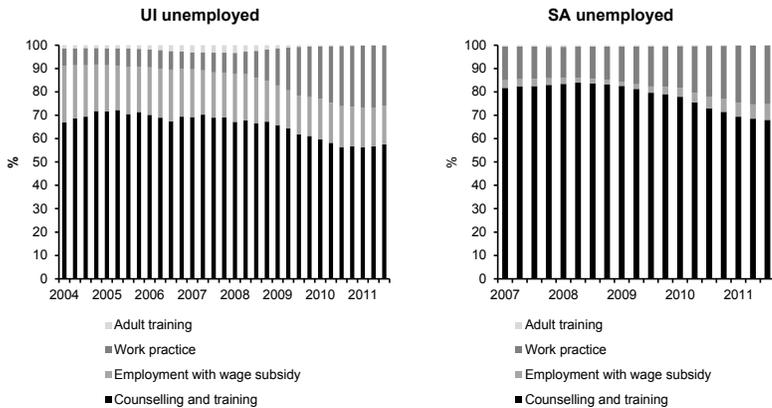
The activation rate (the number of people in programmes relative to the relevant target group) and the average programme duration are shown in Figure 2 for both UIB- and SA-unemployed. It is slightly above 20 per cent for the UI-unemployed and 35-40 per cent for the SA-unemployed. The average duration of programme activities is slightly longer (14-15 weeks) for UIB-unemployed than for SA-unemployed (10-11 weeks). It is noteworthy that the activation rate has remained fairly constant despite the dramatic increase in unemployment. This documents a very flexible system in terms of scaling up (or down) activities for the unemployed.

The programme types are illustrated in Figure 3. Counselling and training activities as well as employment with wage subsidies (for UI-

unemployed) have decreased in relative importance, while work practice, which consists of short-term affiliations (typically four weeks) with private or public-sector firms, has grown. It is noteworthy that the activation rate has remained fairly high despite the crisis and the increase in unemployment. This is obviously reflected in a clear pro-cyclical pattern for the expenditures on ALMPs.

Figures 2 and 3 reveal nothing about whether the current intensity of the programmes is indeed the optimal one, or whether the optimal mix of programmes is achieved. In order to provide insights into these questions, we proceed by reviewing existing evidence to discuss which types of programmes are more likely to be favourable and, finally, we analyse an increase in the intensity of some programmes using experimental variation.

Figure 3. Activation types for UIB- and SA-unemployed



Source: www.jobindsats.dk.

Note: Relative distribution of participants in various activation programmes, rolling annual average.

2. Effects of ALMPs for youth

There is a fairly large literature on the impacts of ALMPs and specifically for youths in Europe. The literature distinguishes effects from ALMPs in different dimensions such as: effects before the programme actually takes place (ex-ante/threat effects), effects during programme participation

(locking-in effects) and effects after the programme has actually taken place (programme effects). Below, we mainly report on programme effects and, in some cases, we also comment on shorter term effects (a mixture of locking-in and programme effects). This is done since most studies report this subset of effects, although the ex-ante effects have earlier been shown to be quite important in an overall assessment of the effectiveness of ALMPs (see e.g. Rosholm and Svarer, 2008).

Initially, we focus on the effects of ALMPs on youth in general, and then we proceed to the effects on disadvantaged youth. Lastly, we present some Danish experimental evidence and make some general comments.

2.1 European evidence

Overall, the evidence regarding the effectiveness of ALMPs for youth is fairly mixed, as shown in e.g. meta-studies by Kluve (2010) and Card et al. (2010). The meta-analysis of Kluve (2010), which focuses on European studies, reports that ALMPs targeting youth are commonly less likely to be effective as compared to non-targeted programmes. Looking at a restricted sample including 35 studies with a particular focus on youth programmes, Kluve (2010) reports 17 studies with significantly positive impacts for youth, 13 studies with insignificant effects, and the remaining 5 studies report negative effects. This evidence suggests that wage subsidy programmes and programmes aimed at enhancing job search efficiency are mostly positive, although there are also studies pointing to the opposite. The evidence for training programmes is more mixed but mostly positive, while for public sector job creation, the evidence is less favourable. In more recent studies, the evidence is generally slightly less positive, showing a majority of studies reporting negative or insignificant impacts.

One youth programme that has been shown to be quite successful is the UK New Deal for Young People (NDYP). Blundell et al. (2004) analysed the introductory part of the New Deal for Young People, called the Gateway. It consisted of frequent meetings with a mentor with the aim of encouraging and improving job search. They find an increase in the employment rate of 5 percentage points four months after entry into the Gateway. Dorsett (2006) evaluates the subsequent programme parts of the Gateway, which are more job search assistance, subsidized employment,

training/education programmes or job creation schemes. The first two elements are shown to be much more effective than alternative programmes in terms of causing entry into employment, and thereby the effects follow the ranking indicated above.

Looking at a few recent studies from Northern Europe, van den Berg et al. (2012) show how attending meetings with a caseworker affects young UI benefit recipients using a duration model framework with Danish data. They show that young workers attending a meeting with a case worker experience a sharp increase in job finding rates, which then tends to wear off rather quickly for young men, while the increase is more lasting for young female workers. For Sweden, Larsson (2009) finds that 'youth' practice and labour market training programmes both tend to have negative short-run impacts and zero to small positive effects in the longer run in the Swedish labour market. Youth practice is found to be the least harmful of the two programmes. Lastly, a more recent study by Caliendo et al. (2011) finds positive long-term effects on employment for nearly all measures aimed at labour market integration on the German labour market. The most effective programmes are (again) found to be wage subsidy programmes, whereas public sector job creation is found to be harmful or ineffective.

If we look particularly at programmes towards disadvantaged youth (often at the education barrier), the evidence is extremely scarce and even less positive (Kluve, 2010).⁶ For instance, Caliendo et al. (2011) find no effects on education participation for low educated youth from any of the evaluated German ALMPs. There are, however, some recent studies that have found positive effects for programmes aimed at youth. A recent study by Ehlers et al. (2012) evaluates a German programme targeting disadvantaged youth with a combination of coaching, training and temporary work. They use data from a quasi-randomized-out control group to investigate programme effects and find positive effects on post-programme employment rates. Flores et al. (2012) and Zhang et al. (2012) also investigate educational programmes (Job Corps) for uneducated youths in the US and find positive effects on future earnings.

⁶ As mentioned above, one goal for ALMPs in this setting could be to increase participation in education for this group.

2.2 Experimental Danish evidence

In Denmark, there is some evidence obtained from RCTs regarding policies aimed at (young) workers receiving UI benefits (i.e. the most ‘employable’ workers). Below we briefly present two of these experiments and their primary findings.

The experiment Quickly Back to Work (QBW) was conducted in the winter of 2005-2006 and was aimed at all newly unemployed workers eligible for UI benefits. The treatment consisted of an intensification of the active measures aimed at unemployed workers, i.e. earlier and more frequent meetings, job search assistance, and early mandatory participation in activation programmes. Graversen and van Ours (2008) show that the treatment group found employment much faster than the controls and focusing on the youth, they find even larger effects than for the overall population. Rosholm (2008) shows that, in general, the positive effects from QBW tend to derive in particular from early meetings and threat effects from having to participate in programmes. Subsequently, these results have influenced the labour market policy in Denmark and moved activation forward, especially for young unemployed and have moved the first meeting with a case worker to one month for unemployed youth (the rules were presented in the last section).

In a subsequent set of randomized experiments, Quickly Back to Work 2 (QBW2), conducted in 2008, two interventions were investigated, namely: early and frequent meetings with a case worker and early mandatory participation in activation programmes (see Maibom et al., 2012 for details). Figures 4A-D below show how the experiment affected young workers. The figures report the difference in accumulated weeks of employment between the treatment and control group in the almost five years (240 weeks) from which we have data since the beginning of the programme.

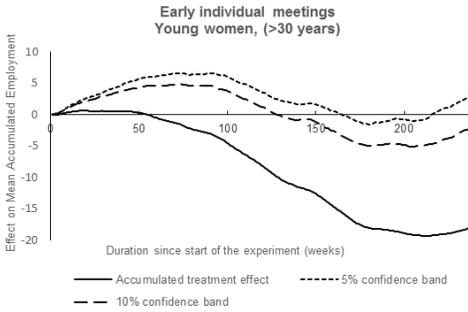
Somewhat differently from the overall results (see Maibom et al., 2012), it is found that for the subpopulation of young women, meetings have negative impacts in the long run, in the sense that women in the treatment group accumulate 15 weeks less employment than women in the control group (Figures 4A and 4B). The result is, however, not statistically significant until after three years which could be due to the very small sample. For young men, there are positive effects from meetings with case workers, but again, these are not statistically significant.

If we look at early activation programmes, we see a different picture emerging. The intervention consisted of mandatory participation in activation programmes after 13 weeks of unemployment, which allows us to analyse the overall effects (including threat effects) from the programme. In the full population, Maibom et al. (2012) report that men were more employed as a result of this intervention and the interpretation is one of threat effects for men anticipating early activation and therefore intensifying job search and hence finding jobs. For women in general, small negative effects are reported (i.e. women in the treatment group were less employed) and interpreted to be locking-in effects. In the sub-population of young workers (Figures C and D), there are only positive effects, and the effects for young men are remarkable; they accumulate almost half a year of extra employment as compared to the control group. This result is statistically significant, while the positive effects for women are smaller and not significant.

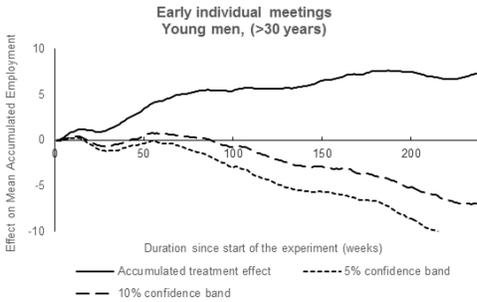
The conclusion from these experiments so far is that counselling (in the form of meetings with case workers) may be effective for newly unemployed young workers, but that the impacts tend to be larger for the adult population. In addition, early activation does appear to be effective, since it leads to increased search activities *before* actual participation, particularly for young men. It is, however, also clear from the literature that activation has a locking-in effect and therefore tends to prolong the unemployment spells for those who participate. On top of this are the costs of running activation programmes. To decrease both the locking-in effects and the costs of programmes, there has been a shift from more expensive programmes like class-room training to less expensive programmes like work practice, where the unemployed has an internship at a private or public firm for an average of four weeks, receiving unemployment benefits. Although it has been the intention to implement a more work-related activation policy, it has not been implemented in the experiments in a sufficiently clean way to allow a direct test of the effectiveness of the two training schemes against each other.

Figure 4. Effects on accumulated employment for young workers of intensified policies

a. Young women, frequent meetings



b. Young men, frequent meetings



c. Young women, early activation

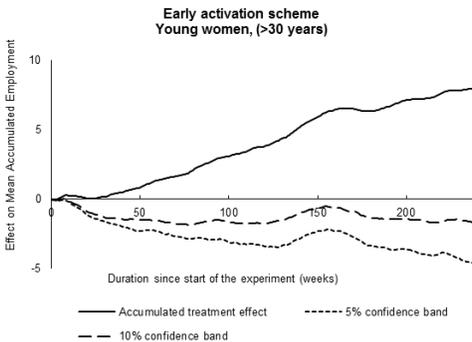
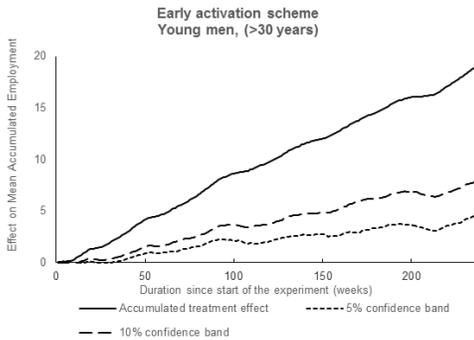


Figure 4. Continued....

d. Young men, early activation



Source: Own calculations.

Finally, a few general comments that are related to the literature evaluating ALMPs in general and thereby also to the studies presented above. First, note that locking-in effects, threat effects and post programme effects are also likely to vary with the economic cycle; for instance, QBW1 was conducted in years when the Danish economy was booming, whereas QBW2 was conducted just before the financial crisis. The empirical evidence on the cyclicity of programme effects is rather scarce, but Forslund et al. (2011) provide some insights. They argue that ALMPs affecting the returns to search (such as e.g. meetings) are probably less effective in recessions, when there are generally fewer jobs and competition is fiercer. Second, most assessments of the effects of ALMPs do not consider general equilibrium effects, arising via job creation, induced by changes in job creation rates due to changed search behaviour or wage effects, cf. Andersen and Svarer (2012). The wage effect is potentially important since ALMPs do not only affect unemployed workers and participants in programmes, but also employed workers facing an unemployment risk.

3. The Danish youth experiment

As in most countries, youth in Denmark were disproportionately affected by the financial crisis, and youth unemployment rose more sharply as

compared to older workers. This led to a demand for more active policies aimed at reducing youth unemployment. Since the unemployed youth face different barriers, a dual strategy seemed appropriate. For young people with a professional education (youth at the second barrier), the goal was to bring them back into employment relatively fast, whereas for uneducated⁷ youth (youth at the first barrier), the long-term strategy was to encourage enrolment in a relevant education.

The policies aimed at youth are already quite intensive, as discussed earlier. To test if it would be helpful with even more intensive counselling and a stronger focus on education for uneducated youth, a randomized experiment was implemented in Denmark, starting in November 2009. In the following subsections, we study this experiment in detail. We discuss the experimental design and then we contrast it to the actual implementation and, subsequently, we analyse the effects of the experimental intervention.

3.1 Treatment design

Table 1 illustrates the treatment protocol as it was prescribed to the participating job centres. The intended treatment scheme applies to unemployed youth below 30 who became or were already unemployed in the period from November 2009 and the next 14 weeks in 14 (not randomly) selected job centres. This means that the treatment targets both UI and SA unemployed, and that the samples are obtained from both the inflow and the stock of unemployed. Upon inflow into the experiment, the caseworker makes an assessment of whether the unemployed has a “qualifying”⁸ education and the job centre receives information from an external agent (that performs the randomization) about whether to assign individuals to the treatment or the control group. The treatment will differ depending on the skill assessment made by the caseworker.

⁷ Uneducated youth are defined as youth without a professional education. This means that they have not completed an education above upper secondary education or a vocational-oriented education and thereby, they do not have any formal skills beyond the basic ones.

⁸ Qualifying education was earlier defined as youth who hold a completed education above upper secondary education or vocational-oriented education. But note that here, it is the caseworker that makes the assessment. We will comment on this below.

Table 1. Design of the randomised experiment

Deadline:	Uneducated Youth (group 1)	Educated Youth (group 2)
Week 1	Information letter, meeting, and skill clarification course	Information letter and meeting
Week 1 or 2	Individual meetings every week until week 32 after unemployment entry	Fortnightly meetings until week 14 after unemployment entry
Week 3	If needed preparatory adult education is initiated	
Week 6	Mentor is assigned, enrolment into activation programme, educational job or business centre	
Week 13		Work practice or subsidized employment initiated
Week 32	Status meeting	Status meeting

Source: Own calculations.

Note: For very disadvantaged individuals in group 1, special programmes are designed on top of the already mentioned treatment scheme.

In the week of inflow into the experiment, unemployed individuals assigned to the treatment group received an information letter which informed the individual about participating in a pilot study and contained a description of the new rules and deadlines that would apply. In the same week, they participated in an introductory meeting at the job centre where the labour market outlook was discussed and the skills of the unemployed were assessed (past employment, future job possibilities, CV etc.).

For individuals that do not have a qualifying education (Uneducated Youth), the job centre has the option of enrolling the individual in a skill clarification course with a duration of 1-2 days to assess the academic skills. If needed, preparatory adult education is initiated from around week 3. Furthermore, uneducated individuals attend weekly meetings from week 1 or 2 and for the next 32 weeks. Meetings can be held either at the job centre or by phone if the unemployed is participating in e.g. an activation programme. After 6 weeks of unemployment, a mentor is assigned to the uneducated unemployed, and she is enrolled into: (i) an activation programme, (ii) a job with an educational purpose, or (iii) work practice in a local business centre (consisting of special employers coop-

erating with the job centre)⁹. A mentor is either an externally hired person or a caseworker, either from the job centre or from the activation programme. Treatment of individuals ends around week 39 where a meeting is held and the individual is put back into the standard regime and plans future activities.

Summarizing the treatment for uneducated individuals, we see a very intense and broad range of treatments that all aim at improving the skills of the unemployed and motivate/prepare them for undertaking ordinary education. The overall aim is to bring the unemployed closer toward the educational system or, alternatively, if their skills are deemed insufficient for undertaking further education, employment.

The treatment regime for youth with a qualifying education is as follows; after an initial meeting and information letter, the unemployed participates in meetings every other week for 14 weeks. Thereafter, the unemployed is enrolled in a business oriented activation programme (public/private wage subsidy or work practice). The treatment ends around week 32, where a meeting is held that puts the individual back into the standard regime and plans future activities. Comparing this treatment regime to that which applies to uneducated individuals, it is obvious that this treatment has much more focus on getting individuals into regular jobs instead of education.

From the above, it is clear that the treatment scheme employs a broad selection of tools in order to try to bring individuals closer towards either the educational system or actual employment. This implies that an evaluation of this design will focus on the impact of the treatment as a whole and not on the effects from sub-elements as the effects arising from different sub packages are very hard to identify. Such a design is also very likely to experience implementation problems due to the ambitious treatment design. This will make it even harder to identify the successful sub-elements, due to the wide variety of treatments actually administered. Given these concerns, we will focus on the intention-to-treat effects that the experiment generates; one could also argue that these are the policy relevant effects in this setting.

⁹ For more information on this cooperation see:
<http://ams.dk/da/Viden/Udvikling%20og%20forsoeg/Virksomhedscentre/Virksomhedsindsats/Virksomhedscentre.aspx>.

Another concern with the design is that the control group might be given lower priority in the pursuit of giving the treatment group all the intended treatments. In order to avoid such issues, the job centres were allocated extra funds from the labour market authorities in order to deal with the increased requirements. We have no further insight into whether the extra funds were sufficient to provide the intended treatment and whether the job centres could find the required resources.

3.2 Data

In this section, we describe the sample of unemployed that are enrolled in the experiment. The data are extracted from administrative registers, merged by the National Labor Market Authority into an event history data set, which records and governs the payments of public income transfers, records participation in ALMPs, and has information on periods of employment and unemployment. The administrative data are used for determining eligibility for UI and SA benefit receipt and for determining whether the job centres meet their requirements in terms of meetings and activation intensities. The information is therefore considered to be very reliable. The event history data set includes detailed information on: weekly labour market status and history (employment, unemployment, in education, on leave, etc.), ethnicity, gender, residence, marital status and UI fund membership. Our final sample consists of 3 380 individuals where 1 697 are assigned to the control group and 1 683 are assigned to the treatment group. Table 2 illustrates the distribution into treatment and control groups for both uneducated and educated youth. Table A.1 in the Appendix illustrates how the sample is constructed from individuals flowing into the experiment starting from November 2009 and onwards. The inflow numbers are very similar except around week 5 where the inflow is particularly large (just before Christmas); this is counteracted in the subsequent weeks (6-9) due to the holiday season.

To assess the validity of the randomization scheme, Tables A.2 and A.3 in the Appendix provide tabulated averages of selected individual characteristics for each of the sub-samples in the above table (i.e. educated controls, treatments etc.) and for a sample where there is no distinction in terms of education (pooled sample). There are no obvious deviations from random assignment in the pooled sample or in the sample with un-

educated individuals. In the sample with educated individuals, the fraction of newly unemployed individuals is somewhat larger in the treatment group.¹⁰ The average time spent in unemployment before inflow into the experiment is significantly larger in the control group (p-value 0.079). To the extent that these differences will represent some deviation from a randomized assignment, our results for the subsamples could be biased. However, we have not found any other indications suggesting such problems.¹¹

Table 2. Number of individuals in the experiment

	Total	Uneducated	Educated
Treatment	1,683	1,115	568
Control	1,697	1,153	544
Total	3,380	2,268	1,112

Source: Own calculations.

Comparing the two sub-samples (educated vs. uneducated), we find interesting differences between educated and uneducated individuals. Uneducated individuals are younger and they have longer elapsed unemployment spells before they are enrolled in the experiment. They also have larger transfer degrees (the fraction-of-the-last-year spent on some kind of public income support), which reinforces the perception that these individuals are indeed a “weaker” group of unemployed.

To evaluate the actual implementation and subsequent effects from the experiment, we have weekly information on labour market status, meeting attendance, and programme participation for each person in the experiment. Each person is followed until the end of January, 2013. The labour market status is inferred based on information from the register on payments of public income transfers, which is used to construct the labour market states ‘unemployment’ and ‘other public income transfers’. Data from the e-income register are used to identify weeks in employment. Finally, there is a residual labour market category, called ‘self-sufficient’, consisting of the self-employed and individuals that are neither working nor receiving any income transfers (e.g. housewives).

¹⁰ P-value in a two-sided t-test of equality of means is 0.2241.

¹¹ The distribution of elapsed unemployment duration is heavily right skewed such that noise at the tail could cause the difference in the means. In Appendix Figure A.2, we present the distribution of elapsed duration by treatment status to investigate this point.

Given the sampling window (week 45 in 2009 to week 6 in 2010), all individuals can be followed for at least 151 weeks and for 165 weeks at most after their entry into the experiment. We can also follow individuals back in time, although the employment information (from the e-Income register) is available only from 2008 and onwards.

Describing the sample before inflow into the experiment

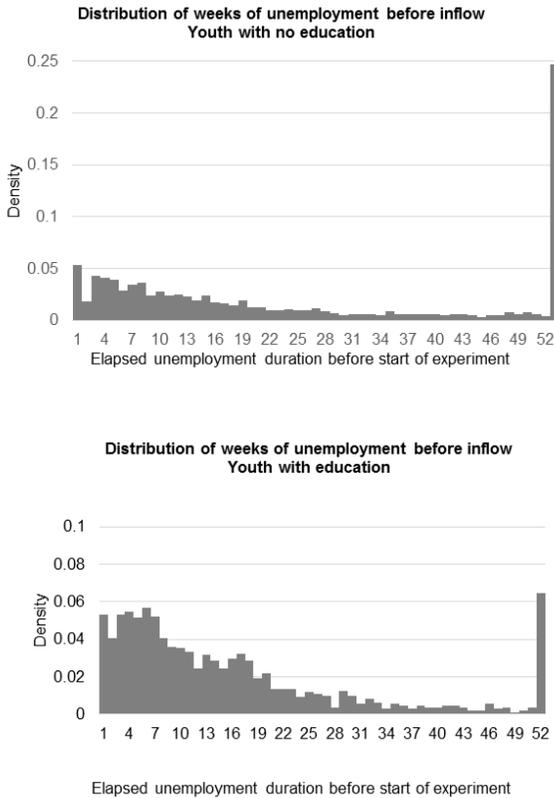
As mentioned in the presentation of the treatment scheme, the samples are created by sampling both from the inflow into unemployment and the stock of unemployed. Given the effects from both state dependencies and dynamic selection in the stock of unemployed, one can argue that the latter group probably constitutes a much weaker target group who will be harder to help back into employment. Therefore, it is important to describe the sample in this dimension, in order to understand the population and compare the results to findings from previous experiments. Figure 5 plots the distribution of elapsed unemployment duration before inflow into the experiment. Notice that the distribution is “top coded” at 52 weeks.

Figure 5 illustrates that especially among uneducated individuals, there is a high concentration (24 per cent) of individuals who have been unemployed for more than a year. In the educated sample, the concentration is a lot smaller (6 per cent). Furthermore around 19 per cent of the uneducated youth in the experiment are newly unemployed (less than five weeks on unemployment) whereas the number is around 24 per cent for educated youth. In previously conducted experiments (e.g. Quickly Back To Work 1 and 2) these fractions were considerably higher (around 90 per cent).

In addition, on average 9 per cent of the last year, before inflow into the experiment, are spent in education and approximately 23 per cent of the last year are spent in employment. 58 per cent of the last year are spent on some kind of public transfers (e.g. unemployment benefits, SA, sickness benefits).¹²

¹² The residual group is, for instance, in the self-support state.

Figure 5. Sampling inflow or stock



Source: Own calculations.

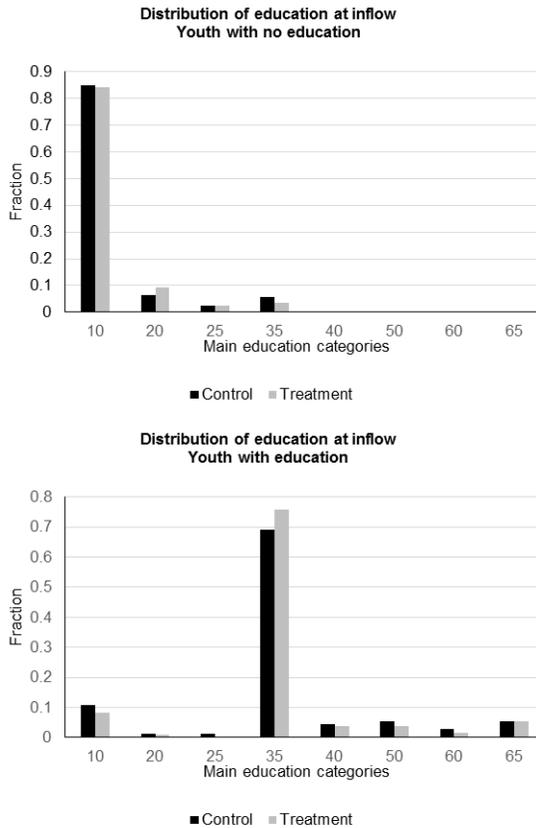
Summing up, the study population consists of much weaker unemployed in this experiment when compared to previously conducted experiments, in the sense that they have been unemployed for a longer period of time before they enter the experiment.

The division into two sub-samples

As mentioned in the section on the treatment design, the caseworker had to make the assessment of whether the unemployed have sufficient educa-

tion. Below, we contrast this assessment with the educational level of the individual using Danish register data (IDA).¹³

Figure 6. Education



Source: Own calculations.

Notes: 0=missing, 10=primary school, 20/25=High School, 30/35=Short/Medium vocational, 40=Short higher education, 50=Medium Higher Education, 60=Bachelor, 65=Master and above.

Figure 6 plots the distribution of educational levels within the two subsamples. It is evident that there are huge educational differences between the two samples, but there is also some overlap in the distributions (values from 35 (Medium Vocational Education) and above should ideal-

¹³ IDA is a register based annual matched-employer-employee panel covering 1981-2012. This panel contains the entire Danish labor force. The unit of observation is a given individual in a given year with educational measures generally referring to the last week of November.

ly define the educated sample). We have tried to investigate whether there are any systematic misplacement patterns by a simple linear probability model, where we regress an indicator variable of over/under misplacement (according to the IDA registers) on available explanatory variables.¹⁴ Generally, the R-squared from such regressions is very low (around 3 per cent). The main statistical significant findings are that individuals are less likely to be “over placed” (i.e. caseworker assessment is higher than registers) if they belong to match group 2 compared to 1 (they are weaker unemployed),¹⁵ and individuals are more likely to be “under placed” (registers predict higher education than caseworker assessment) the older they are. The treatment status is (marginally!) significant in the latter regression, and the effect is negative such that, if anything, controls have higher education than their treated counterparts, but the effect is very small (less than 2 per cent).

Summarising, it appears that misplacement according to the registers is mainly due to unobservable influences such as e.g. measurement error or caseworker idiosyncrasies. Some misplacements could be due to older individuals who have maybe left school so many years ago that their skills are deemed insufficient. We will proceed by treating the sample as two subsamples. As a robustness check, we have evaluated the experiment using just one pooled sample. As expected, the results are weighted averages of the subgroup analysis that we will report below. The results are available in Appendix Figures A.4 and A.5.

3.3 Implementation

In this subsection, we present evidence on the actual implementation of the experiment. To illustrate the degree of compliance with the experimental protocol, we present a set of figures on the weekly treatment intensities for unemployed individuals (the fraction of unemployed individuals that is given a specific treatment in a given week). We focus on the first 50 weeks – this includes the treatment period and, in addition, allows for implementation lags such as delayed meetings due to sickness, planning in the job centre etc. The figures should be regarded as lower bounds

¹⁴ These are the same as those used in table A.2 in the Appendix. In addition, we also included job centre dummies and treatment status. The results are available upon request.

¹⁵ Match group 3 is excluded from the experiment.

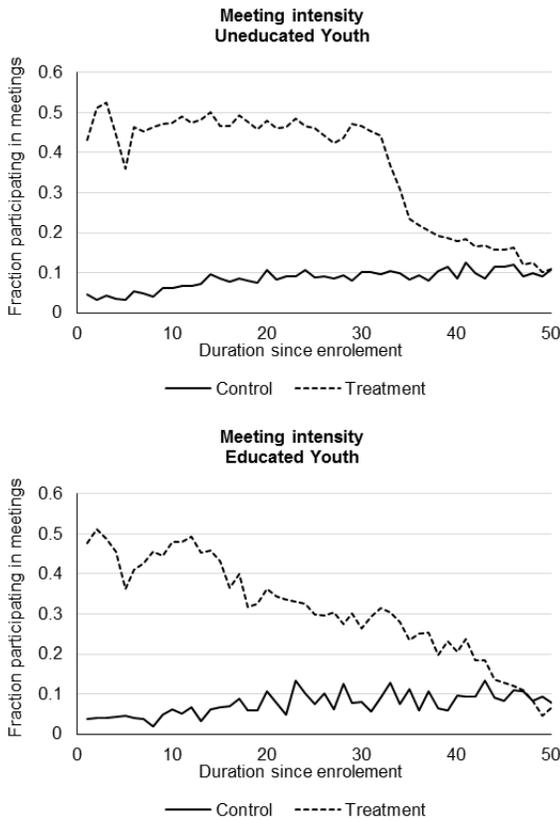
on the actual implementation in the job centres, as unemployed individuals participating in, for instance, two meetings in a given week will only be counted once and, furthermore, individuals who have employment at the end of the week might not be treated.

Meetings

Figure 7 shows the fraction of unemployed individuals in the treatment and control groups that participate in meetings. The figure illustrates that in both groups, there is an increased meeting activity in the treatment group compared to the control group that marks the “normal” behaviour. For uneducated youth, 50 per cent of the unemployed in a given week participate in meetings for the first 30 weeks. After week 30, the intensity of meetings falls for this group, but it remains higher than that of the control group until around week 50. For educated youth, the meeting intensity is less stable over the treatment period, initially around 50 per cent of the individuals attend meetings but this fraction declines almost linearly over time.

The observed treatment intensities imply that an uneducated (educated) individual who is unemployed over the whole treatment horizon will have participated in an average of 18 meetings (15 meetings), as compared to around four meetings in the control group. From the observed treatment intensity, we can therefore conclude that educated individuals receive much more than the intended treatment, whereas uneducated individuals receive fewer meetings than intended (which was one per week). When we look further into the type of meetings, we see that roughly 35 per cent of all meetings held during the treatment period are telephone meetings and only around 55 per cent take place at the job centre. The typical content of a meeting consists of both a monitoring (are benefit requirements fulfilled etc.) and a counselling part. *Ex post*, some caseworkers have mentioned concerns that they had difficulties in securing a proper amount of content/progression in the meetings due to the high meeting frequency.

Figure 7. Meeting intensity



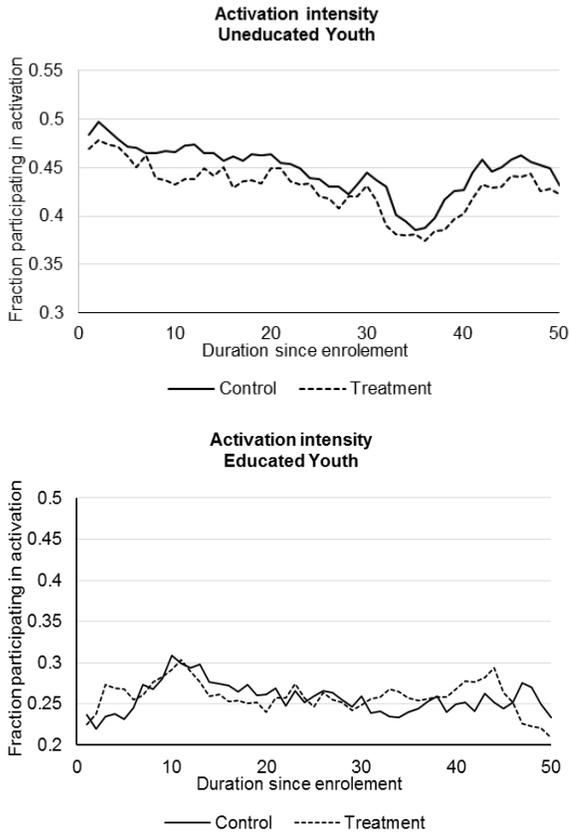
Source: Own calculations.

Comparing the observed meeting intensities with earlier experiments, we can conclude that the meeting frequency is higher in the current experiment. In e.g. QBW2, the treatment group participates in around 10 meetings on average during the first 50 weeks, whereas the amount for the control group is roughly similar to the numbers presented above.

Activation programmes

Figure 8 shows the fraction of unemployed individuals who participate in an activation programme in a given week after their enrolment into the experiment.

Figure 8. Activation intensity



Source: Own calculations.

First of all, it is clear from the figures that the “normal” use of activation programmes is much larger for uneducated individuals as compared to those who hold an education. For uneducated individuals, if anything, it appears that the control group participates in activation slightly more than the treatment group, in spite of the treatment protocol which prescribes that these individuals should be enrolled in some kind of activation programme already after six weeks. However, individuals with education are not activated more than their control counterparts.

Looking at the types of activation programmes in which individuals participate, we generally see that individuals without education participate in activation programmes with educational purposes and only around

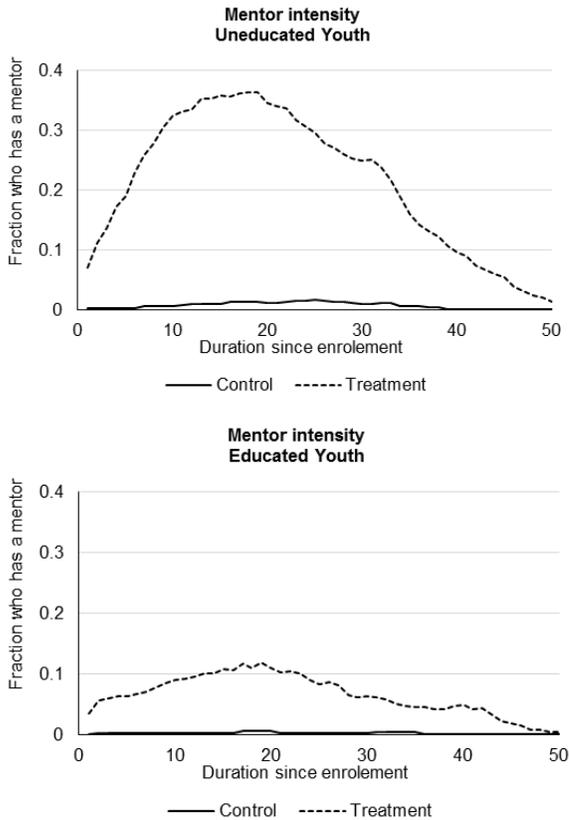
10 per cent of the individuals activated in a given week are participating in work-related activation programmes. On average, unemployed individuals will have experienced about 20 weeks of activation after 50 weeks of unemployment. This clearly illustrates the intensive regime that exists for both the treatment and the control group. For individuals with education, the activation intensity is smaller but still around 13 weeks of participation in activation programmes after 50 weeks of unemployment. As could be expected, since the educated group is more “employable”, a larger share of activation programmes are work-related, around 60 per cent in the control group and around 70 per cent in the treatment group.

Summarizing the implementation analysis of activation programmes, we note that job centres hardly comply with the treatment protocol at all. A larger share of treated individuals with education go into work-related programmes than in the control group, but the overall amount of activation is more or less the same in the two groups.

Other aspects of treatment

An important part of the intended treatment scheme described above is the assignment of a mentor to each unemployed individual in the uneducated group. Figure 9 illustrates the use of mentors.

Clearly, there is not 100 per cent compliance with the treatment requirement either. At its peak, less than 40 per cent of the individuals without education are assigned a mentor and on average around 20 per cent have one during the first 40 weeks. A number of individuals (20 per cent of those who have a mentor) experience several mentors during their unemployment spell, and in 50 per cent of the assignment cases, the mentor is simply a caseworker at the job centre. Only 20 per cent of the individuals, who were assigned to a mentor, meet the mentor for more than a total of four hours during the treatment period. These observations lead us to the conclusion that the mentor part of the treatment is also far from its initial intention.

Figure 9. Mentor intensity

Source: Own calculations.

Ex post, some job centres argued that they found it less useful to assign mentors since the unemployed were already participating in meetings every week. Surprisingly, individuals with education were also assigned mentors to some extent (see Figure 9), although this was not the intention. This indicates that there is a smoothing of treatments between groups which implies that we have to take this into account in the policy conclusions that can be obtained from this experiment. As a side comment, this also underlines one of many potential complications in very ambitiously designed RCTs, which challenges the conclusions that can be drawn.

We have made similar “treatment intensity” calculations for the preparatory adult education scheme, which is designed to start around week

3 for individuals without education. Less than 3 per cent of the treatment group participate in such programmes at any point in time during the first 40 weeks. The average duration of these programmes is 13 weeks. A final programme element, which we have looked into, is the use of other programmes than those mentioned at the bottom of Table 1. This gives the job centre the possibility of letting very vulnerable youth participate in a broader range of programmes such as rehabilitation, meetings with a psychologist, physical exercise, addiction treatment etc. The fraction of the treated individuals without education who participate is shown in Figure A.1 in the Appendix. For those participating, the average duration of these programmes is 17 weeks and the treatments cover all the above mentioned programme types. As above, we also observe a minor fraction of the treatment group from individuals with education participating in these programmes. Once more this suggests that educational achievement is not the only relevant characteristic when the caseworker defines the optimal treatment, and that some smoothing between treatment groups has been going on.

In conclusion, this subsection reveals that the main intervention in the programme was the intensified use of meetings between the unemployed and a case worker. For uneducated youth, there was also an increase in the use of preparatory programmes. There was almost no difference in the use of activation programmes, and the use of mentors was below the intended use, and most case workers assigned themselves as mentors.

4. Effects of the youth experiment

We mainly study two outcomes and we consider both static and dynamic short-term and long-term effects. Our outcomes are the fraction of individuals employed/in education, and the difference in the accumulated number of weeks in employment/education. Being employed is defined by the receipt of a wage income, and being in education is defined by the receipt of an educational grant. We do, however, also investigate whether the transition to sickness benefits is affected by the experiment.

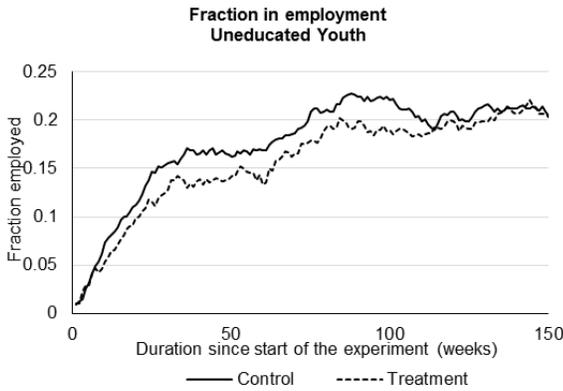
4.1 Employment and education

Figure 10 illustrates the evolution in the employment level and how it accumulates over time for uneducated youth.¹⁶ The figure clearly illustrates that this group of individuals has a relatively low attachment to the labour market. Three years after enrolment into the experiment, only around 20 per cent of the individuals are employed. Furthermore, it is within the first year that the unemployed find employment, hereafter the employment level stabilizes. For almost the entire period, a larger fraction of individuals in the control group have employment as compared to the treatment group. The difference increases over the first 50 weeks and persists until after 100 weeks, where the treatment group catches up.

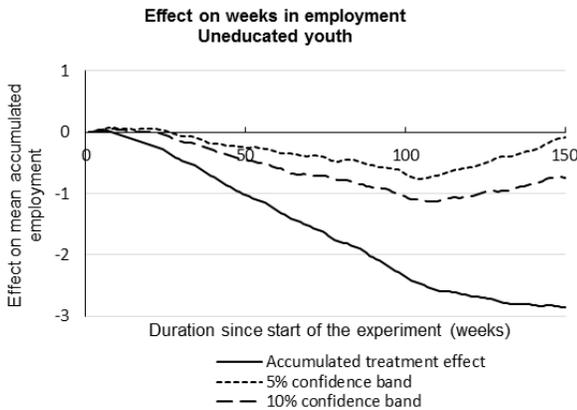
Table 3 reports results from a simple linear probability model with employment status at various points in time as the independent variable and where treatment status and various other explanatory variables are included (to decrease the residual variation and hence, the estimated standard errors). From here, it is clear that there are significant differences in the employment probability between the treatment and the control group. Treatment group members are around 3 per cent less likely to be employed in a given week. Since this difference persists over time, we also see that the control group on average accumulates more weeks of employment over time such that control group members have spent approximately three weeks more in employment after 150 weeks. This difference is significant at the 5 per cent level.

¹⁶ The graphs showing accumulated outcomes also contain confidence bands, which are calculated using a bootstrap procedure presented in Maibom et al. (2012). Explanatory variables are included in our regressions to remove residual variation and decrease minimum detectable effects. This also accounts for the fact that our samples are not entirely balanced in elapsed unemployment duration before inflow into the experiment.

Figure 10. Effect on employment, uneducated youth



Note: Size of groups; Treatment 1115 obs, Control 1153 obs.



Source: Own calculations.

Table 3. Effect of treatment for uneducated youths at a specific time since enrolment

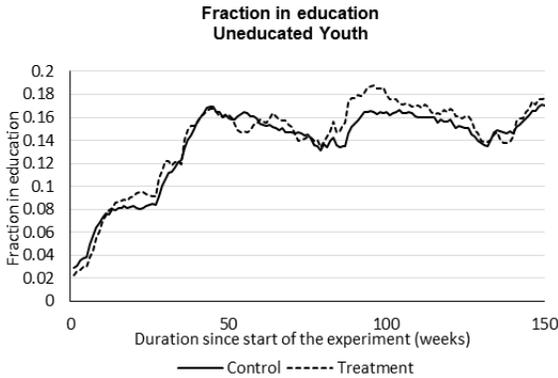
Treatment group indicator	After 20 weeks	After 40 weeks	After 60 weeks	After 100 weeks	After 150 weeks
Employment	-0.165 (-1.28)	-0.292* (-1.94)	-0.374** (-2.49)	-0.356** (-2.13)	-0.55 (-0.33)
Education	0.009 (0.79)	0.001 (0.07)	0.006 (0.41)	0.017 (1.10)	0.010 (0.65)

Source: Own calculations.

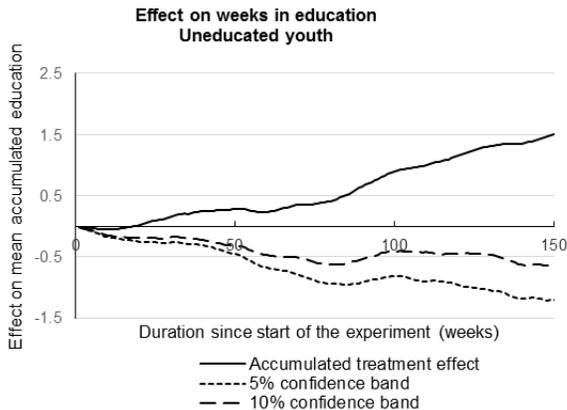
Note: * implies significance at the 10% level and ** at the 5% level, t-statistics in parenthesis.

Looking at the same figures for the education outcome variable (Figure 11), we see that the differences between control and treatment groups are much smaller, and the accumulated difference amounts to less than 1.5 weeks by the end of the observation period and is not statistically significant.

Figure 11. Effect on education, uneducated youth



Note: Size of groups: Treatment 1115 observations, Control 1153 observations.

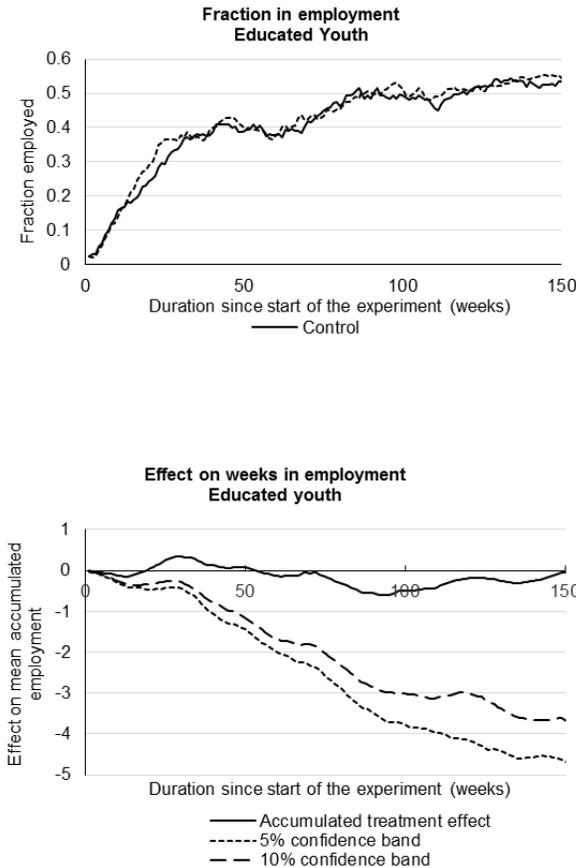


Source: Own calculations.

Therefore, we can conclude that for uneducated youth, there is a negative effect on accumulated employment and no significant effect on weeks spent in education. This suggests that individuals in the treatment group suffer from a lock-in effect in the sense that their treatments pro-

long their unemployment spells. When we decompose the 2-3 weeks of “missing” employment in the treatment group, we observe that around 1 week is spent on sickness benefits (more on this below) and the remaining 1-2 weeks are spent in regular unemployment.

Figure 12. Effect on employment, educated youth



Source: Own calculations.

Turning to the group of educated youth, Figure 12 shows the evolution of the employment levels in the treatment and control groups. Compared to their uneducated counterparts, the employment levels are generally larger (2-3 times as large), although they are not impressive. As in the uneducated group, the employment levels increase in the first year and

afterwards, the growth rate declines and the levels stabilize. This is consistent with a standard dynamic selection process where the most employable initially leave unemployment, and the remaining stock of unemployed consists of the less employable or finds less stable employment relations.

Contrary to the uneducated group, the employment levels in the treatment and the control group are more or less equal along the study horizon, except maybe a small initial effect on employment levels in the treatment group.¹⁷ There are no significant findings in the linear probability model (Table 4), except for weeks 24-26 (not reported). After 150 weeks, the treatment group does, on average, have no extra weeks in employment as compared to the control group.

Table 4. Effect of treatment for educated youth at specific times since enrolment

Treatment group indicator	After 20 weeks	After 40 weeks	After 60 weeks	After 100 weeks	After 150 weeks
Employment	0.0423 (1.59)	0.0097 (0.33)	0.001 (0.03)	0.026 (0.88)	0.012 (0.39)
Education	-0.0139 (-1.23)	-0.008 (-0.58)	0.005 (0.30)	-0.006 (-0.35)	-0.0007 (-0.04)

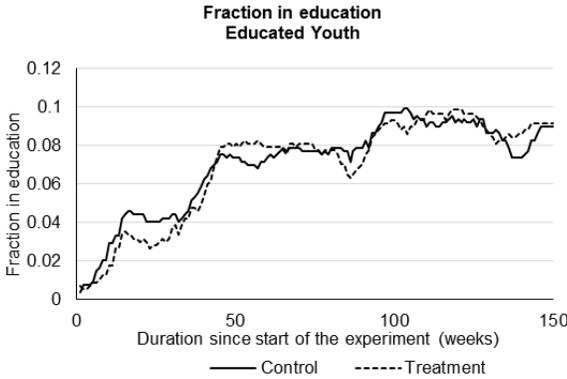
Source: Own calculations.

Note: * implies significance at the 10% level and ** at the 5% level, t-statistics in parenthesis.

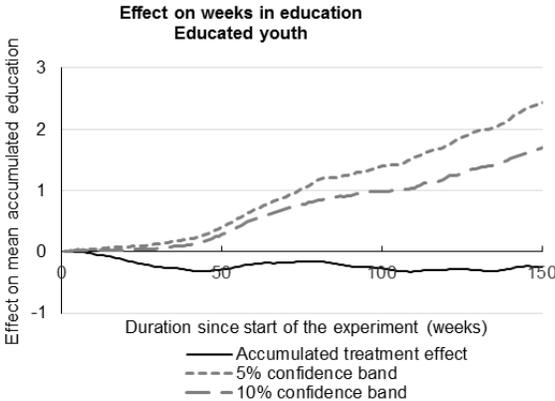
When we look at the education outcome (Figure 13), we see that a very small fraction of educated individuals go into further education (less than 10 per cent). There are differences in the fractions but when accumulated, we get a very small negative effect (insignificant).

¹⁷ Not surprisingly, the effects are somewhat larger (still clearly insignificant) when we exclude elapsed unemployment duration as an explanatory variable, but the results are still very similar. For comparison, we report the figure where we do not include elapsed unemployment duration in Appendix Figure A.3.

Figure 13. Effect on education, educated youth



Note: Size of groups; Treatment 568 observations, Control 544 observations.



Source: Own calculations.

Interestingly, the treatment group also spends an extra 1.5 weeks on sickness benefits; we will focus on this in the next section. Decomposing these extra 1.5 weeks in the control group, we see that some of it is spent in regular unemployment and part of it as self-sufficient (i.e. the individual is not a wage earner and does not receive any transfer incomes from the public authorities).

We have tried to look deeper into the more “qualitative” aspects of the education outcome. One point could be that although the differences between the fraction of treatments and controls enrolled in education do not

seem to be (that) large, there might be important differences in their graduation rates, such that individuals in the treatment group actually obtain education, whereas controls never manage to graduate, or graduate at a slower rate. Therefore, we have looked into the educational data provided by Statistics Denmark within the timeframe of our study.

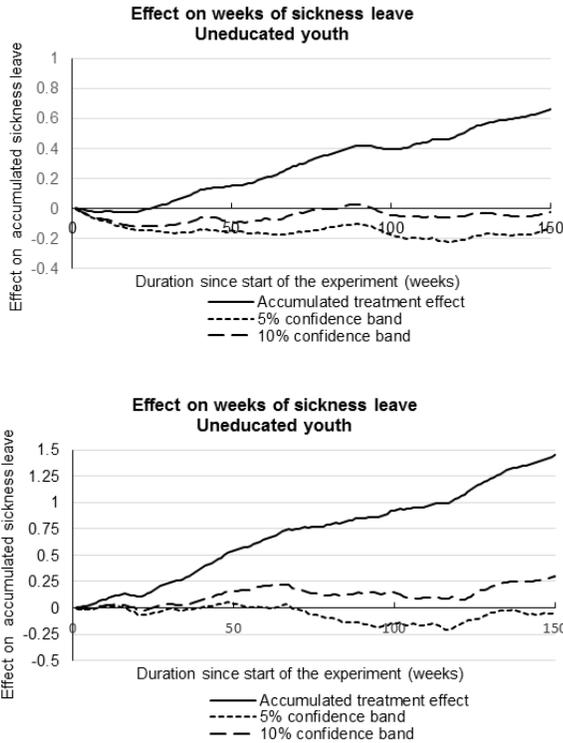
Less than 2 per cent of the individuals without education have a changing educational status within our timeframe, and there are no differences between treatment and control groups. For individuals with education, these numbers are even smaller. These small numbers also illustrate the very low chances of actually completing qualifying education for the target group of unemployed uneducated youth (at least in terms of completing formal education recorded in our educational registers).

4.2 Sickness benefits

We have also analysed exits into sickness benefits. Although exits into sickness benefits occur less frequently, Figure 14 shows that there are marginally significant positive accumulated effects at the 10 per cent level for both groups of unemployed youth.

Figure 14 shows that uneducated youth accumulate around 0.6 weeks more sickness benefits, whereas educated youth accumulate close to 1.5 weeks more. It therefore suggests that a possible downside of intensifying ALMPs is that it can push unemployed workers into sickness benefits, perhaps as an attempt to escape the intensified treatment or as a result of additional pressure and stress, which may lead to an even longer way back into employment.

Figure 14. Effect on sickness benefits



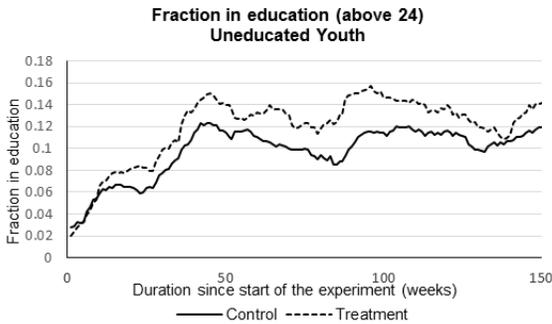
Source: Own calculations.

4.3 Heterogeneous treatment effects

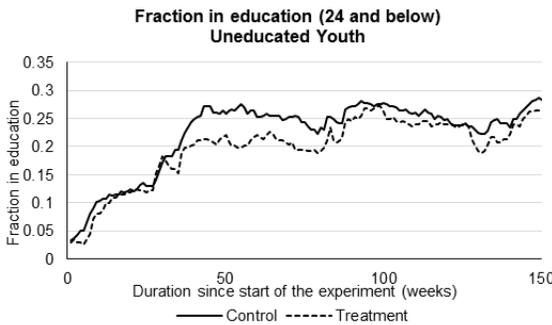
The above section has identified average (ITT) effects. In this subsection, we look further into whether effects are particularly (un)favourable for specific subgroups and thus, whether there are any observable heterogeneous treatment effects. This is done in a regression framework, where accumulated weeks of employment (education) after week 150 are regressed on treatment status, other explanatory variables and their interactions with treatment status. We look for heterogeneous responses in the following dimensions: age groups (relatively young or old), gender, marital status, ethnicity, week of inflow, whether the individual is on SA or on UI benefits, and finally job centres.

With respect to accumulated weeks of employment, our only significant findings (10 per cent level) are that one job centre (Vejele) seems to be doing particularly bad for uneducated treated individuals as compared to their controls, and one job centre (Aalborg) seems to be doing particularly well for educated treated individuals. We have tried to look at the local implementation of the treatment design for these job centres but no clear conclusions regarding implementation prevail.

Figure 15. Effect on education, uneducated youth by age group



Note: Size of groups; Treatment: 802 observations, Control: 313 observations.



Source: Own calculations.

Note: Size of groups; Treatment: 313 observations, Control: 356 observations.

With respect to accumulated weeks of education, we find some differences in the age dimension. Uneducated treated individuals below the age of 24 accumulate three weeks less in education over the 150 weeks studied whereas the older counterparts spend more time in education (four

weeks) than their corresponding controls (the pattern is illustrated in Figure 15). Both effects are significant at the 5 per cent level in our regressions (the results are available upon request). As these effects counteract each other, we see no effects at the aggregate level.

We also find significant interactions with respect to ethnicity, which implies that non-western immigrants in the treatment group perform particularly bad with respect to education enrolment as compared to their control counterparts (11 weeks less). We have not found any similar pattern in the sample with educated individuals, but it should also be noted that the sample size here is very limited.

Figure 16. Effect on employment, UI and SA unemployed

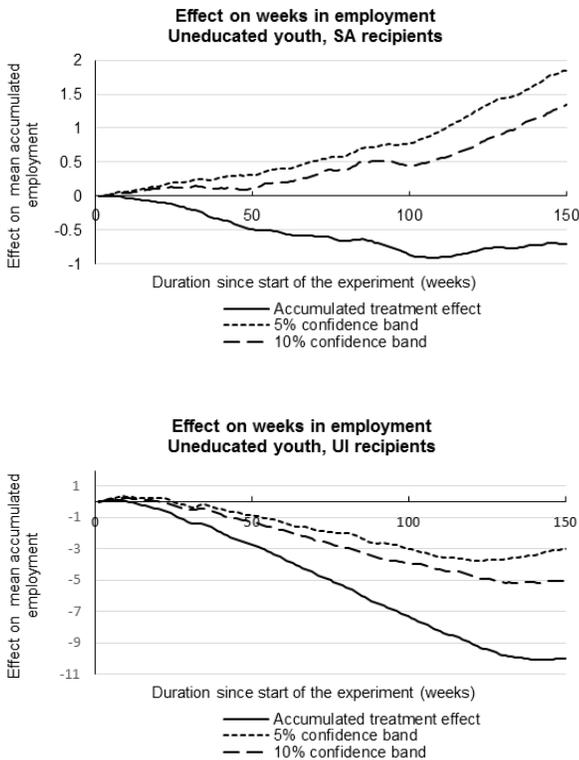
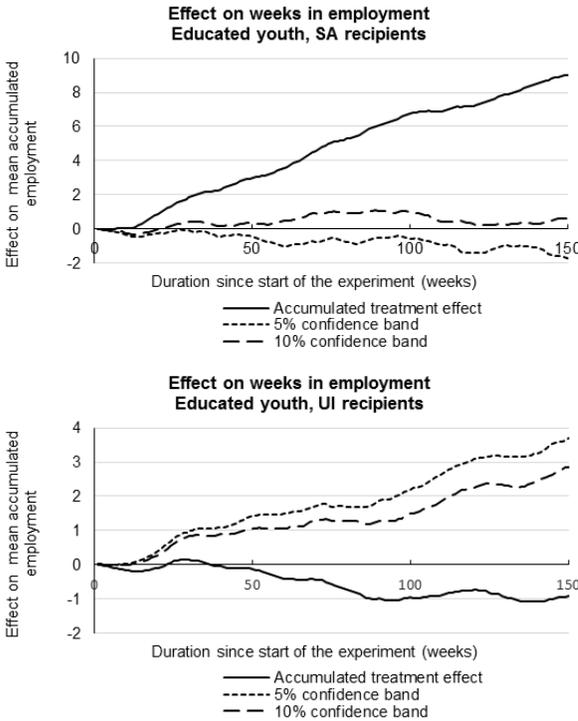


Figure 16. Continued....



Source: Own calculations.

As for the two types of transfer incomes, SA versus UI, Figure 16 presents the accumulated differences over time (notice the difference in scales). From Figure 16, it appears that the negative effects on employment for uneducated youth primarily stem from those on UI benefits who experience a dramatic reduction in accumulated employment, while the effect is considerably smaller (and insignificant) for youth on SA. As for the educated youth, we find marginally significant (but large) positive effects for those on SA and nothing for youth on UI benefits. Concerning entry into education, there were no significant differences between UI and SA recipients. Overall, we would then say that youth on SA respond more positively to this intervention than youth on UI benefits.

5. Concluding remarks

We investigated the effectiveness of active labour market policies for young unemployed Danes. The policy response to youth unemployment in Denmark has relied heavily on active measures, such as frequent meetings with case workers and an intensive use of activation programmes. Empirical findings from the period prior to the financial crises suggest that both meetings and activation had a positive impact on the job finding rate of unemployed youth in Denmark. Partly based on these earlier findings, there has been an intensification of ALMPs in general and for youth in particular. Our main empirical contribution is to evaluate an RCT that was conducted in Denmark in the winter of 2009, that is, at a time when unemployment was rising sharply following the financial and economic crises. The main feature of the experiment was to further intensify the classical tools of the ALMP toolbox and shift the focus from classroom training to work practice and more firm-based job training. Several components of the programme were not implemented according to protocol, which underlines the importance of contrasting the intended treatment with the actual one to improve our understanding of the effects (or the lack of effects) from the experiments. Our analysis documents that the main difference between the treatment and the control group was the number of encounters with a case worker and, potentially, a minor increase in other programmes such as rehabilitation for uneducated youth.

The findings are that for uneducated youth, there was a negative effect on employment. This was in some sense the intention of the programme, since those with no further education should be guided towards education if the option was feasible for the individual. The group of uneducated unemployed did, in fact, accumulate slightly more education, but the magnitude was very small, and the effect on education was far from statistically significant, and we observe no long-term effects in terms of improving educational levels.

For the group of unemployed with some type of further education, the differences between the treatment and the control group are rather small both in the employment and the education dimension. There is some indication that exit to employment was positively affected in the period when the meetings took place, but the size and robustness of the effect are small. At the end of the observation period, the treatment group had ac-

cumulated close to one week more employment than the control group. The effect is not statistically significant.

In some sense, the findings are perhaps not too surprising. The use of meetings is already quite intensive in Denmark towards youth, and the labour market at the time of the experiment was characterised by low job finding rates and rapidly increasing unemployment. In addition, the treatment population consisted of individuals with quite long elapsed unemployment spells and earlier evidence on the effectiveness of e.g. meetings should have much stronger effects for newly unemployed. The analysis found visible effects on the exit to sickness benefits for both groups of unemployed. This side effect should, of course, be taken into account when deciding on the intensity of ALMPs.

References

- Andersen, T.M. and Svarer, M. (2012), The role of workfare in striking a balance between incentives and insurance in the labour market, forthcoming in *Economica*.
- Andersen, T.M., Maibom, J., Svarer, M. and Sørensen, A. (2013), Are the costs of recessions cohort specific?, manuscript, Aarhus University.
- van den Berg, G.J., Kjærsgaard, L. and Rosholm, M. (2012), To meet or not to meet (your case worker) – That is the question, IZA Discussion Paper 6476, Bonn.
- Blundell, R., Costa-Dias, M., Meghir, C. and van Reenen, J. (2004), Evaluating the employment impact of a mandatory job search program, *Journal of the European Economic Association* 2, 569-610.
- Caliendo, M., Künn, S. and Schmidl, R. (2011), Fighting youth unemployment: The effects of active labour market policies, IZA Discussion Paper 6222, Bonn.
- Card, D., Kluve, J. and Weber, A. (2010), Active labor market policy evaluations – A metaanalysis, *Economic Journal* 120, F452-F477.
- Dorsett, R. (2006), The new deal for young people: Effect on the labour market status of young men, *Labour Economics* 13, 405-422.
- Ehlers, C., Kluve, J. and Schaffner, S. (2012), Temporary work as an active labor market policy: Evaluating an innovate program for disadvantaged youth, *Economics Bulletin* 32, 765-773.
- Flores, C., Flores-Lagunes, A., Gonzalez, A. and Neumann, T.C. (2012), Estimating the effects of length of exposure to instruction in a training program: The case of job corps, *Review of Economics and Statistics* 94, 153-171.
- Forslund A., Fredriksson, P. and Vikström, J. (2011), What active labor market policy works best in a recession?, *Nordic Economic Policy Review* 2011/1, 171-202.
- Graversen, B.K. and van Ours, J.C. (2008), How to help unemployed find jobs quickly: Experimental evidence from a mandatory activation program, *Journal of Public Economics* 92, 2020-2035.

- Kluve, J. (2010), The effectiveness of European active labor market programs, *Labour Economics* 17, 904-918.
- Larsson, L. (2009), Evaluation of Swedish youth labor market programs, *Journal of Human Resources* 38, 891-927.
- Maibom, J., Rosholm, M. and Svarer, M. (2012), Experimental evidence on the effects of early meetings and activation, IZA Discussion Paper 6970, Bonn.
- OECD (2013), *Employment Outlook*, OECD, Paris.
- Rosholm, M. (2008), Experimental evidence on the nature of the Danish employment miracle, IZA Discussion Paper 3620, Bonn.
- Rosholm, M. and Svarer, M. (2008), The threat effect of active labour market programmes, *Scandinavian Journal of Economics* 110, 385-401.
- Zhang, J., Rubin, D. and Mealli, F. (2012), Likelihood-based analysis of causal effects of job-training programs using principal stratification, *Journal of the American Statistical Association* 104, 166-176.

Appendix

Table A.1 Inflow into the experiment

Week no	Control	Treatment	Total
1	151	154	305
2	137	131	268
3	169	161	330
4	170	173	343
5	339	345	684
10	176	170	346
11	168	157	325
12	188	182	370
13	191	203	394
14+15	8	7	15
Total	1 697	1 683	3 380

Source: Own calculations.

Note: Week 1 is week 45 in 2009.

Table A.2 Descriptive statistics

Characteristics	Uneducated youth		Educated youth	
	Control	Treatment	Control	Treatment
Age (years)	26.38	26.57	28.35	28.26
Males	0.514	0.526	0.625	0.616
Under 25	0.417	0.381	0.138	0.150
Married	0.125	0.148	0.215	0.210
Danish origin	0.846	0.834	0.877	0.896
Western origin. not Danish	0.020	0.029	0.037	0.021
Non-Western	0.134	0.137	0.086	0.083
Average transfer degree last year	0.615	0.631	0.507	0.461
Transfer degree < 0.1 last 3 years	0.200	0.212	0.298	0.338
Transfer degree ϵ (0.1;0.5) last 3 years	0.474	0.464	0.594	0.586
Transfer degree > 0.5 last 3 years	0.326	0.325	0.108	0.076
Share in "Manufacturing" industry UI fund	0.149	0.143	0.165	0.180
Share in "Metal" industry UI fund	0.004	0.004	0.121	0.114
Share in "Construc" industry UI fund	0.014	0.015	0.162	0.165
Share in Other UI fund	0.060	0.080	0.061	0.062
Share of Newly UE (less than 5 weeks of UE before inflow)	0.191	0.197	0.239	0.266
Average time in UE before treatment	41.43	42.23	18.48	15.87
Number of observations	1 153	1 115	544	568

Source: Own calculations.

Table A.3 Descriptive statistics (pooled sample)

Characteristics	Pooled sample	
	Control	Treatment
Age (years)	27.01	27.13
Males	0.550	0.557
Under 25	0.328	0.303
Married	0.154	0.169
Danish origin	0.856	0.855
Western origin. not Danish	0.025	0.026
Non-Western	0.119	0.119
Average transfer degree last year	0.581	0.573
Transfer degree < 0.1 last 3 years	0.200	0.224
Transfer degree \in (0.1;0.5) last 3 years	0.485	0.476
Transfer degree > 0.5 last 3 years	0.315	0.300
Share in "Manufacturing" industry UI fund	0.154	0.155
Share in "Metal" industry UI fund	0.042	0.042
Share in "Construc" industry UI fund	0.060	0.066
Share in Other UI fund	0.060	0.074
Share of Newly UE (less than 5 weeks of UE before inflow)	0.318	0.332
Average time in UE before treatment	30.28	31.10
Number of observations	1 697	1 683

Source: Own calculations.

Figure A.1. Precautionary programmes

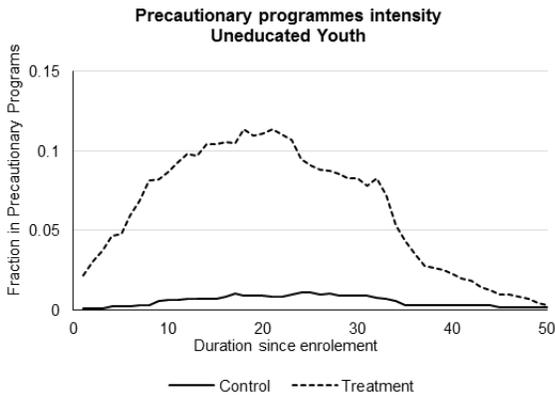
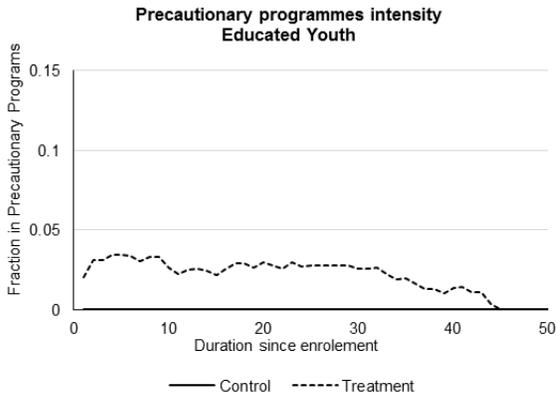
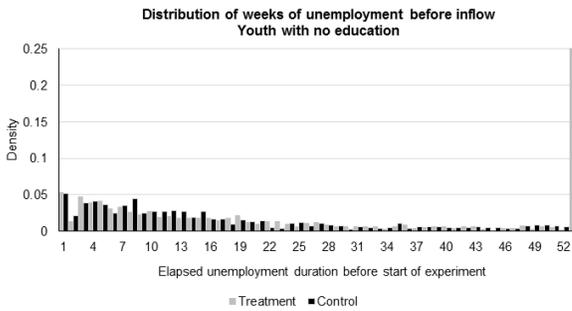


Figure A.1. Continued....



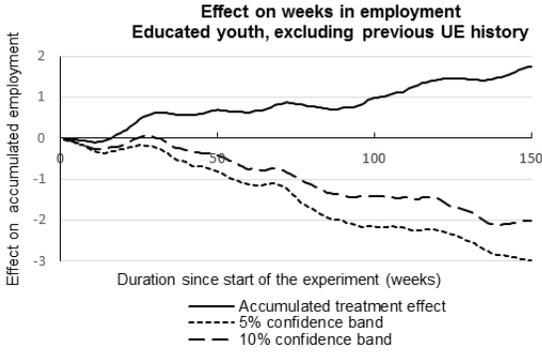
Source: Own calculations.

Figure A.2 Unemployment history in treatment and control groups



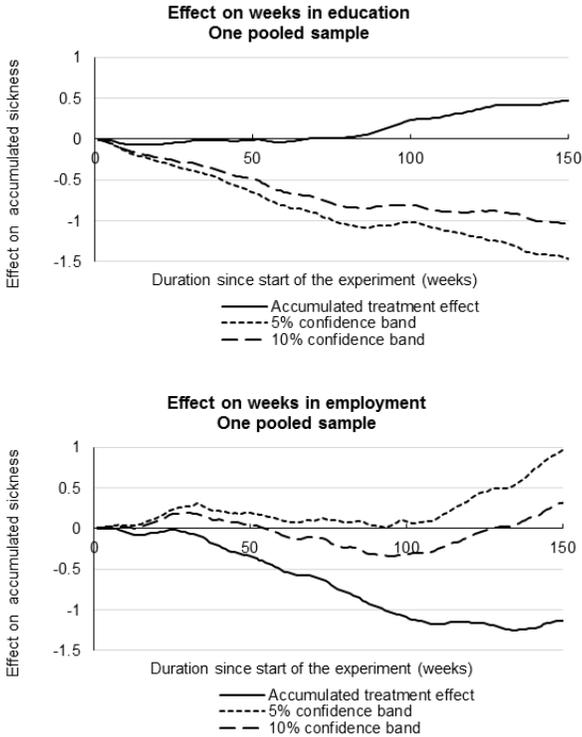
Source: Own calculations.

Figure A.3 Excluding elapsed unemployment duration as an explanatory variable



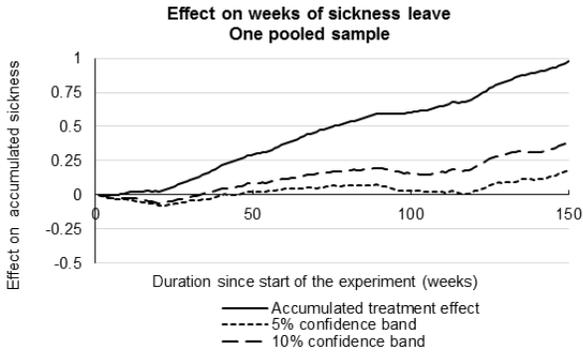
Source: Own calculations.

Figure A.4 Pooled sample



Source: Own calculations.

Figure A.5 Pooled sample



Source: Own calculations.

Comment on Maibom, Rosholm and Svarer: Can active labour market policies combat youth unemployment?

Caroline Hall*

The paper by Maibom, Rosholm and Svarer discusses different active labor market tools used to help unemployed youth either find a job or return to education, and it tries to assess the effectiveness of different types of measures. The assessment partly builds on previous studies, but is also based on an evaluation of a randomized controlled experiment targeted at unemployed youth in Denmark during the Great Recession. The paper thus studies questions of great interest for both economists and policy makers.

I think that this paper makes a valuable contribution to our so far rather limited understanding of which types of policies work best, and which do not work, when it comes to helping unemployed youth. As pointed out in the paper, although there are several studies of the impacts of active labor market policies (ALMPs) for youth, the evidence is rather mixed: Studies of similar types of policies have many times shown results pointing in different directions. Hence, more research in this area is needed.

Two aspects of this paper make it particularly interesting:

First, the paper partly focuses on a group of young unemployed individuals that is relatively disadvantaged on the labor market (the group referred to as the “uneducated youth”). When it comes to impacts of

* Institute for Evaluation of Labour Market and Education Policy (IFAU) and Uppsala Center for Labour Studies (UCLS), caroline.hall@ifau.uu.se.

ALMPs for disadvantaged youth, the existing evidence is very scarce. At the same time, knowledge on how to best help these individuals is highly important from a policy perspective. For example, in Sweden, where youth unemployment generally stands out as high in comparison with the other Nordic countries (e.g. OECD, 2013), we know that most unemployed youth have short unemployment spells (e.g. SCB, 2009). But some groups of unemployed youth, e.g. those who have not completed upper secondary school, face rather large difficulties on the labor market and have a relatively high risk of long-term unemployment (e.g. OECD, 2008). To date, we know very little about how to best help this group.

The second reason why I find the paper particularly interesting is that the results presented are based on a randomized controlled experiment – which is rather rare when it comes to ALMPs (at least outside Denmark). The evaluation can thereby avoid the main methodological problems we generally face in evaluations of ALMPs using non-experimental data and, thus, the findings are likely to be more reliable as compared to many other studies.

Below, I list my comments on the paper section by section. I will highlight some things I found unclear upon reading the paper and raise some questions regarding how we should understand the results presented in the paper and what policy conclusions we can draw from them.

Short introduction to the Danish labor market (with a focus on the use of ALMPs)

Overall, I think that this section does a good job at introducing the reader to the use of active labor market policies in Denmark in a short and clear way. However, a few things were still unclear to me after reading this section. I list these below:

The section describes how the unemployed are categorized into three different groups based on an assessment of their potential of getting employed: (1) “job-ready” (those who can become self-supportive in three months); (2) “ready for activity”; and (3) “temporarily passive”. The rest of this section is then devoted to reviewing the rules for group (1), the “job-ready”. This gave me the impression that the “job-ready” will be the group at focus also in the experiment. However, from the description of the youth experiment in Section 3.1, it seems that the experiment was

aimed at all unemployed youth; that is, unemployed individuals from all three groups. If that is the case, why not describe the rules for all three groups?

In this section, as in the rest of the paper, the unemployed are categorized into those “with education” (or with “qualifying education”) and those “without education” (or “without qualifying education”). The treatment protocol in the experiment also differs for these two groups. However, what it means to be “without education” was not explained in the version of the paper that I received. I assume that all youth have some education. Are youth “without education” those who lack an upper secondary degree (or the equivalent), or are they perhaps those who lack post-secondary education? This will be important to clarify.

One last thing that would be helpful to add to this section is an explanation of which economic incentives the unemployed face regarding participation in meetings and activation activities. Are there financial penalties, e.g. reduced or withdrawn unemployment insurance (UI) benefits, if they do not show up at meetings or refuse to participate in activation activities? Do these rules differ for individuals receiving UI benefits and for those receiving social assistance (SA)?

Brief review of the literature on the effects of ALMP for youth

One perspective that I think would be interesting to add to the literature review is whether the effects of different types of ALMPs (for youth) could be expected to vary by the economic cycle. In particular, since the paper focuses on ALMP during the Great Recession, this would be an interesting aspect to discuss.

There is not much empirical evidence on this question, but previous research has, e.g., questioned the effectiveness of relying heavily on job-search assistance (which seems to have been part of the experiment evaluated in the paper) during recessions when there are few jobs to apply for (see e.g. Forslund et al., 2011). Different economic conditions could offer one potential explanation for why the effects of the current experiment with more intensified ALMPs (mainly consisting of more intensified meetings) differ from the effects of previous similar experiments which took place before the financial crisis.

The Danish youth experiment: Data and implementation

As the paper deals with experimental data, the evaluation is rather straightforward. The authors assess the validity of the randomization by presenting descriptive statistics for the treated and controls, and the groups look fairly similar in terms of the included characteristics.

The implementation of the experiment and the degree of compliance with the treatment protocol are carefully described and illustrated. The authors show that the treatment mainly involved a large increase in meeting intensity (involving both monitoring and counseling), for both the “uneducated” and the “educated” group of unemployed youth. On the other hand, there was not much difference between treated and controls regarding the use of activation activities, mentors or preparatory adult education.

One additional program element that was used more frequently among the treated than the controls was what is referred to as precautionary programs (e.g. rehabilitation, meetings with a psychologist, addiction treatment, etc). At the peak, slightly above 10 percent participate in these programs among the non-educated (compared to 1 or 2 percent among the controls). Those who participate seem to do so for quite a long time (17 weeks on average). This part of the treatment is not mentioned when discussing the results. To me it is not obvious that the increased use of these programs was so minor that it should be ignored when we interpret the results.

Effects of the youth experiment

The results in the paper indicate that more intensified meetings with caseworkers (on average) did not help bring the unemployed youth into employment or education. For the uneducated group, there are even negative effects on employment. However, a previous randomized experiment in Denmark (“Quick Back to Work”), where the treatment also involved earlier and more frequent meetings, showed positive and statistically significant effects on employment – also among youth (Graversen and van Ours, 2008). The positive effects seemed to partly derive from the earlier meetings (Rosholm, 2008). In order to understand what policy conclusion we can draw from the results in this paper, it would be helpful

to have some more discussion of why the results of these two experiments may differ:

- One potential explanation put forward in the paper is that the use of meetings is already quite intensive in Denmark when it comes to unemployed youth. Hence, the current experiment seems to have raised the meeting intensity from a higher level as compared to the previous experiment discussed above. I think it would be helpful with some more details regarding how the meeting intensity differed between these two experiments. Such a comparison could perhaps tell us something about whether there is a level beyond which a further increase in meeting intensity is no longer beneficial.
- Related to this is the fact that the unemployed youth, both treated and controls, already participate quite intensively in activation activities. This is especially true for the non-educated: 40-50 percent of this group participate in such measures each given week. Adding more meetings in a situation where participants are already “locked-in” to other activities might not be helpful for increasing employment.
- Another potential explanation as to why the effects of more intensified meetings differed between the two experiments could be the different labor market conditions. The previous experiment took place before the financial crisis, and the discouraging results of the current experiment might be attributed to the weak labor market. Frequent meetings involving job search assistance and monitoring may not be an efficient method when there are few jobs to apply for.
- Another potentially relevant difference between the two experiments is that the target group seems to have differed: The current experiment targets both SA and UI benefit recipients, whereas the previous experiment seems to have only targeted UI benefit recipients. Since there is a work requirement in the unemployment insurance, UI benefit recipients most likely have a closer attachment to the labor market. I think it would be interesting to see if the effects of the treatment differ between UI and SA benefit recipients. The paper investigates whether there are any heterogeneous treatment effects along several dimensions, but this dimension has not been explored so far.

Among both the educated and uneducated youth, more intensified meetings increased the use of sickness benefits. The authors state that this highlights a possible downside of intensifying ALMP, as sickness benefits may imply a longer way back to employment. From the description of ALMPs in Section 1, it seems like the same rules for ALMPs apply to all recipients of temporary income transfers, including sickness benefits. Hence, to the reader, it is not obvious why an increased use of sickness benefits would lead to a longer way back to employment. It is also not clear why unemployed individuals would have incentives to transfer to sickness benefits as the intensity of ALMP increases.

References

- Forslund, A., Fredriksson, P. and Vikström, J. (2011), What active labor market policy works best in a recession?, *Nordic Economic Policy Review* 2011/1, 171-202.
- Graversen, B.K. and van Ours, J.C (2008), How to help unemployed find jobs quickly: Experimental evidence from a mandatory activation program, *Journal of Public Economics* 92, 2020-2035.
- OECD (2008), *Economic Survey Sweden*, OECD, Paris.
- OECD (2013), *Employment and Labour Markets: Key Tables from OECD/Youth Unemployment Rate*, OECD, Paris, www.oecd-ilibrary.org.
- Rosholm, M. (2008), *Experimental evidence on the nature of the Danish employment miracle*, IZA Discussion Paper 3620, Bonn.
- SCB (2009), *AKU 1:a kvartalet 2009 (tema ungdomsarbetslöshet)*, *Statistiska meddelanden AM 11 SM 0902*, Statistics Sweden, Stockholm.



norden

Nordic Council of Ministers

Ved Stranden 18
DK-1061 Copenhagen K
www.norden.org



9 789289 327602

US2014:416
ISBN 978-92-893-2760-2
<http://dx.doi.org/10.6027/US2014-416>
ISSN 1904-4526