DISCUSSION PAPER SERIES

IZA DP No. 7912

Can Active Labour Market Policies Combat Youth Unemployment?

Jonas Maibom
Michael Rosholm
Michael Svarer

January 2014

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Can Active Labour Market Policies Combat Youth Unemployment?

Jonas Maibom
Aarhus University

Michael Rosholm
Aarhus University, TrygFonden's Centre for Child Research and IZA

Michael Svarer
Aarhus University, TrygFonden's Centre for Child Research and IZA

Discussion Paper No. 7912
January 2014

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 7912
January 2014

ABSTRACT

Can Active Labour Market Policies Combat Youth Unemployment?

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 in-creased transitions into sickness benefits.

JEL Classification: J0, J64

Keywords: youth unemployment, activation, random controlled experiments

Corresponding author:

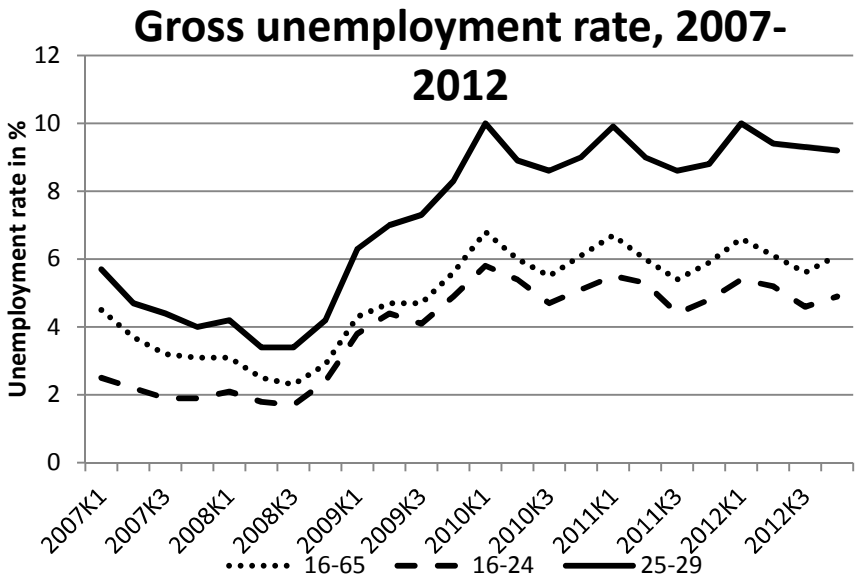
Michael Svarer
Department of Economics and Business
TrygFonden's Centre for Child Research
Aarhus University
Fuglesangs Allé 4
building 2621, room 3a
8210 Aarhus V
Denmark
E-mail: msvarer@econ.au.dk

1. Introduction

The Great Recession has had a large labour market impact, causing a steep decrease in employment rates, in many countries. For given macroeconomic 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 1970s and 1980s both show how steep increases in unemployment can translate into increases in structural unemployment and further, how difficult it may be to bring down the structural unemployment rate subsequently – 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 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 individuals. Unemployment for individuals below 25 years old is much lower than for young people aged 25-29 and is, in fact, lower than the overall unemployment rate. This might be explained in part by labour market policy, which is much stricter for individuals below 25 and in particular the benefit system, which is considerably less generous for those below 25. We will discuss these aspects in more detail later. Finally, the increase in unemployment measured in percentage points have 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 OECD average for youth below 25. The unemployment rate is close to the OECD average for the 25-29 years 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 lot 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/qualifying[2] education, ALMPs will often aim at preparing young individuals for obtaining such an education[3], whereas unemployed youth with a professional education (youth at the second barrier) receive more traditional policies aimed at improving employment outcomes directly.

[1] See e.g. http://www.bm.dk/da/Beskaeftigelsesomraadet/Flere%20i%20arbejde/Ungeindsats.aspx.
[2] This means that they have not completed education above upper secondary education or vocational oriented education and thereby they do not have formal skills beyond basic ones.
[3] This group is sometimes labeled youth at the first barrier in the literature.

The purpose of this study is to describe the active labour market tools used to help unemployed youth back into education or employment and to assess the effectiveness of the different measures. The structure of our 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 educational attainment for unemployed youth without a qualifying education (i.e. could the policy achieve dual goals).

2. 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 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] Persons not eligible for unemployment insurance benefits (UIB) are eligible for Social Assistance (means tested at the level of the household).

All unemployed are categorized into so-called match groups based on an overall assessment of the potential for the individual to be 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 past wage income cannot exceed 90%, 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%. Benefit duration has been shortened over the years and since 2011 it has been 2 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 3 months.
- Group II (ready for activity): Individual is not at present ready to start working, but is capable of participating in a programme activity aiming at employment.
- Group III (temporarily passive): 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-1990s, 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) has to 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 3rd month.

- Unemployed have a right and an obligation to participate in an activation programme after 9 months of unemployment, and thereafter every 6 months.

For youth there are some special rules:

- Below the age of 30: First interview within one month, thereafter every 3 months.

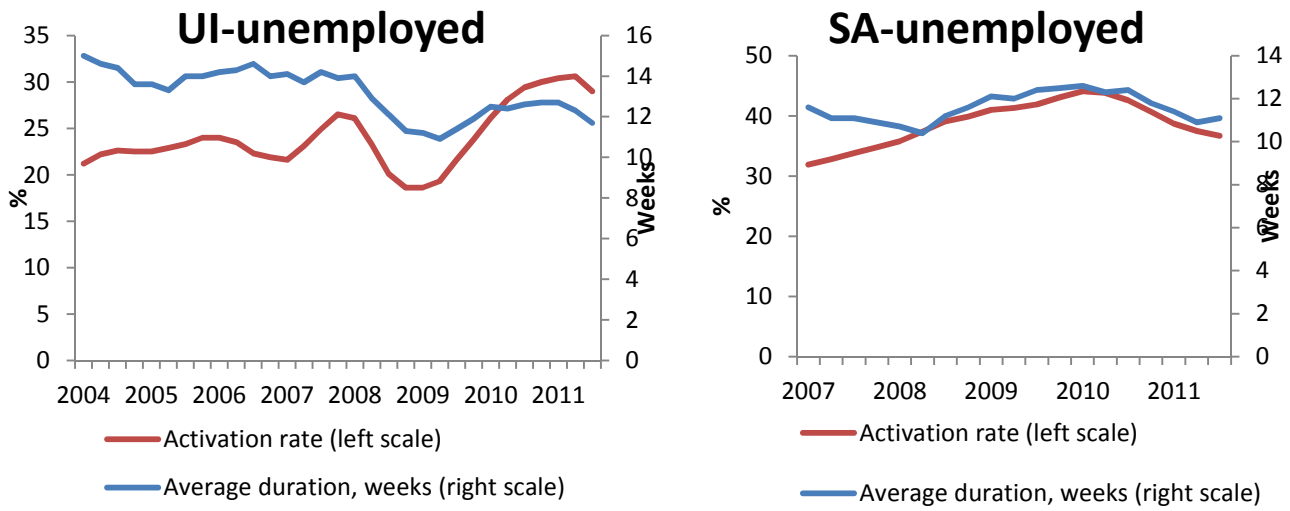
- Below the age of 30: First activation (right and duty) after 3 months. If the person does not have a qualifying education (defined above), the activation programme should be aiming at education in the ordinary educational system.

- Below the age of 25 without education and dependent children: it is mandatory to take some education; if not immediately suited for enrolment into 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.

The activation rate (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% for the UI-unemployed and 35-40% 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.

Figure 2: Activation rate and average duration of activation activities – UI and SA unemployed



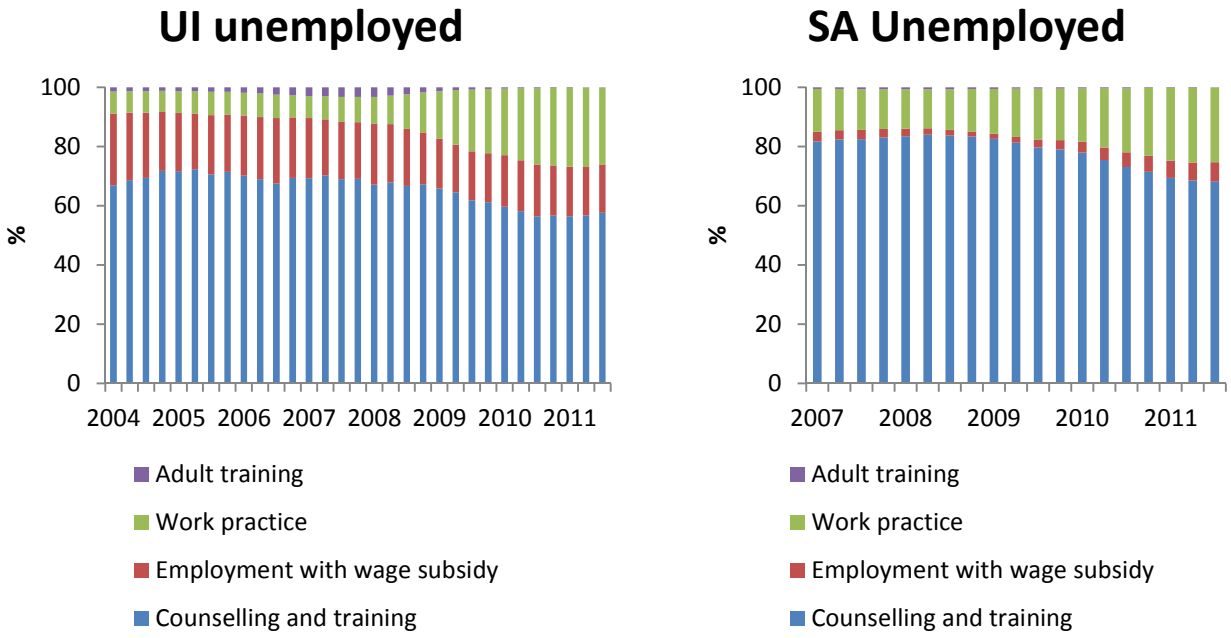
Source: www.jobindsats.dk.

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.

Figure 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



Note: Relative distribution of participants in various activation programmes, rolling annual average.
Source: www.jobindsats.dk.

3. 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 report mainly on programme effects and in some cases 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 & 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.

3.1 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 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, generally the evidence is 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) analyzed the introductory part of 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%-points 4 months after entry into the Gateway. Dorsett (2006) evaluates the subsequent programme parts of the Gateway, which are either 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 using 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. Of the two programmes, youth practice is found to be the least harmful. 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 effectful 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 German ALMPs evaluated. There are, however, some recent studies that have found positive effects for programmes aimed at youth. A recent study by Ehlers *et al.* (2012) evaluate 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. Also Flores et al. (2012) and Zhang et al. (2012) investigate educational programmes (Job Corps) for uneducated youths in the U.S. and find positive effects on future earnings.

3.2 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 2005-6 and was aimed at all newly unemployed workers eligible for UI benefits. The treatment consisted of an

[6] As mentioned above one goal for ALMPs in this setting could be to increase participation in education for this group.

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 & 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 1 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 5 years (240 weeks) we have data from since the beginning of the programme.

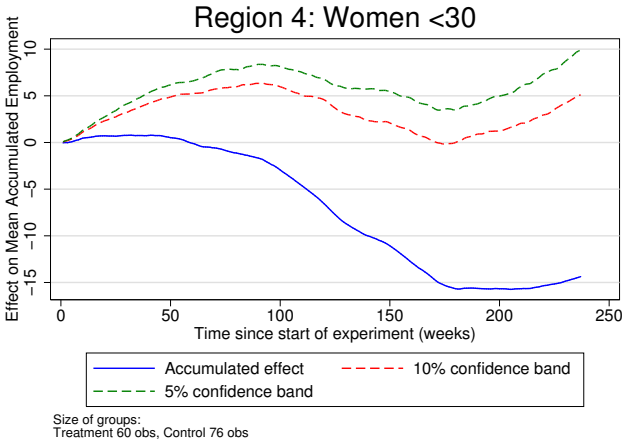
Somewhat differently from the overall results (see Maibom *et al.*, 2012), it is found that for the subpopulation of young women, meetings have negative impacts in the long run, in the sense that women in the treatment group accumulate 15 weeks less employment than women in the control group (Figure 4A & 4B). The result is, however, not statistically significant 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, and this 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 (Figure C & D), there are only positive effects, and the effects for young men are remarkable; they accumulate almost half a year of extra employment compared to the control group. This result is statistically significant, while the positive effects for women are smaller and not significant.

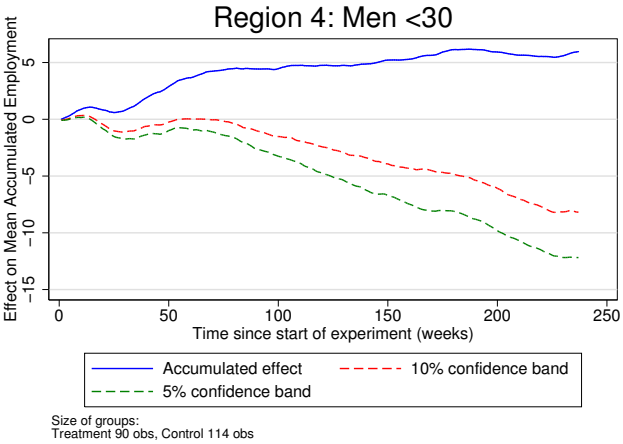
Figure 4: Effects on accumulated employment for young workers of intensified policies

A: Young women, frequent meetings



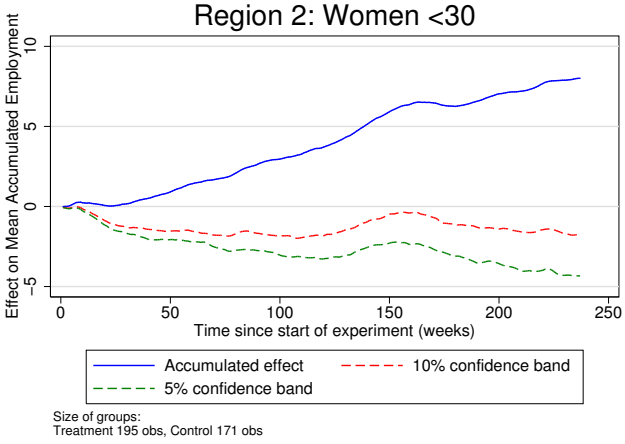
Size of groups:
Treatment 60 obs, Control 76 obs

B: young men, frequent meetings



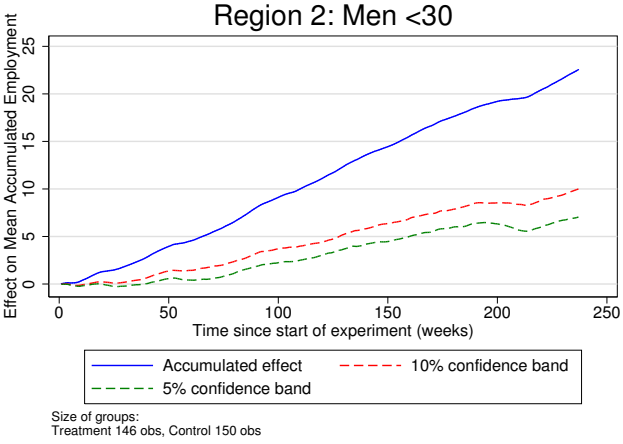
Size of groups:
Treatment 90 obs, Control 114 obs

C: Young women, early activation



Size of groups:
Treatment 195 obs, Control 171 obs

D: Young men, early activation



Size of groups:
Treatment 146 obs, Control 150 obs

The conclusion so far from these experiments 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 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 4 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.

Lastly, 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 cyclicality 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 affect not only unemployed workers and participants in programmes, but also employed workers facing an unemployment risk.

4. The Danish youth experiment

As in most countries, youth in Denmark were disproportionally affected by the financial crisis, and youth unemployment rose more sharply 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 uneducated[7] youth (youth at the first barrier), the long-term strategy was to encourage enrolment into a relevant education was promoted.

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, then we contrast it to the actual implementation, and subsequently we analyse the effects of the experimental intervention.

4.1 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. At 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 education above upper secondary education or vocational oriented education and thereby they do not have formal skills beyond basic ones.

[8] Qualifying education was earlier defined as youth who holds a completed education above upper secondary education or vocational oriented education. But note that here the caseworker 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 either activation programme, educational job or business centre	
Week 13		Work practice or subsidized employment initiated
Week 32	Status meeting	Status meeting
For very disadvantaged individuals in group 1, special programmes are designed on top of the already mentioned treatment scheme.		

At 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 into a skill clarification course with 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 either: (i) an activation programme, (ii) a job with an educational purpose, (iii) work practice in a local business centres

(consisting of special employers cooperating 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 into a business oriented activation programme (public/private wage subsidy or work practice). Treatment ends around week 32, where a meeting is held that puts the individual back into the standard regime and plan future activities. Comparing this treatment regime to that which applies to uneducated individuals, it is obvious that this treatment intends much more focus on getting individuals into regular jobs instead of education.

From 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 is very hard to identify. A design like this is also very likely to experience implementation problems due to the ambitious treatment design. This will make it even harder identifying 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 issues like this, 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 to whether the extra funds were sufficient to provide the intended treatment and whether the job centres could find the required resources.

4.2 Data

In this section we describe the sample of unemployed that are enrolled into 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 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 3380 individuals where 1697 are assigned to the control group and 1683 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.

Table 2: Number of individuals in the experiment

	Total	Uneducated	Educated
Treatment	1683	1115	568
Control	1697	1153	544
Total	3380	2268	1112

To assess the validity of the randomization scheme Table A.2 and A.3 in the appendix provide

tabulated averages of selected individual characteristics for each of the sub-samples in the table above (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 uneducated 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 randomized assignment our results for the subsamples could be biased, however we have not found any other indications suggesting such problems.[11]

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 into 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. 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 is 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).

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

[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 the appendix figure A.3 we present the distribution of elapsed duration by treatment status to investigate this point.

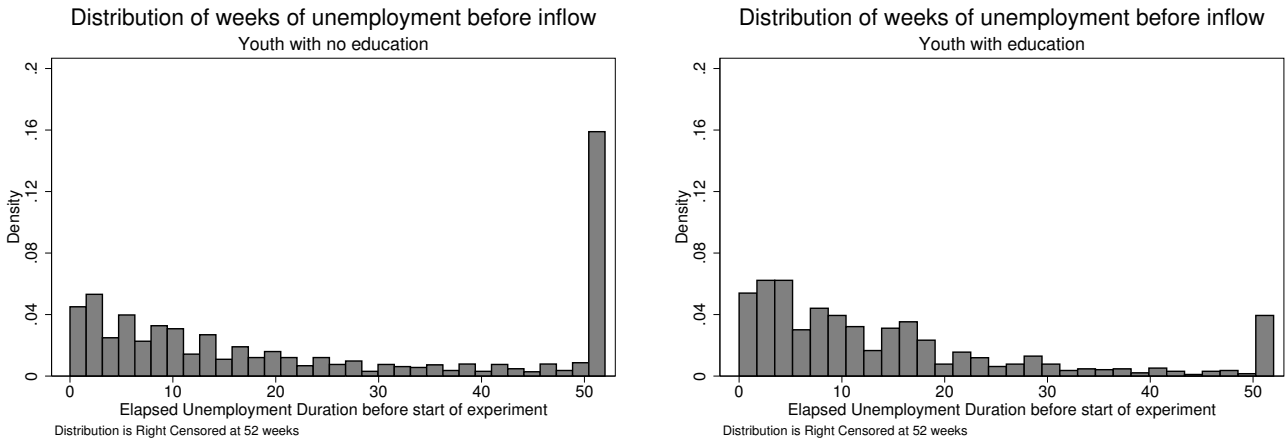
also follow individuals back in time, although the employment information (from the e-Income register) is available only from 2008 and onwards.

4.2.1 Describing the sample before inflow into the experiment

As mentioned in the presentation of the treatment scheme, the samples are created 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. It is therefore important to describe the sample in this dimension, in order to understand the population and to 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 (16 %) of individuals who have been unemployed for more than a year. In the educated sample the concentration is a lot smaller (4 %). Furthermore around 19% of the uneducated youth in the experiment are newly unemployed (less than 5 weeks on unemployment) whereas the number is around 24% for educated youth. In previously conducted experiments (e.g. Quickly Back To Work 1 and 2) these fractions were considerably higher (around 90%).

Figure 5: Sampling inflow or stock



In addition, on average 9% of the last year, before inflow into the experiment, is spent in education and approximately 23% of the last year is spent in employment. 58% of the last year is spent on some kind of public transfers (e.g. unemployment benefits, SA, sickness benefits)[12].

Summing up, the study population consist 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.

4.2.2 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 education. Below we contrast this assessment with the educational level of the individual using Danish register data (IDA).[13]

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 ideally 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%). 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. 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%).

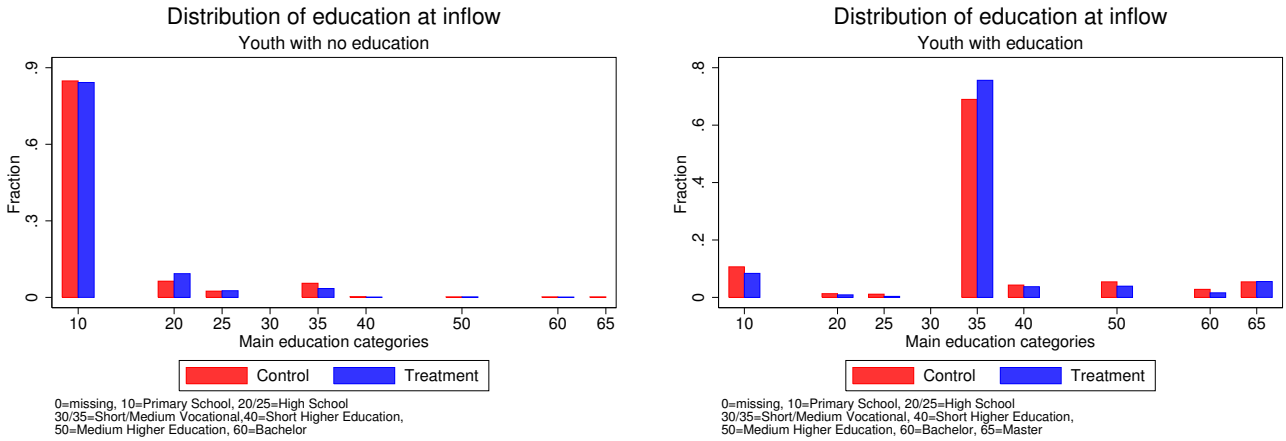
[12] The residual group are for instance in the self-support state

[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.

[14] These are the same as those used in table A.2 in the appendix. In addition, we included also job center dummies and treatment status. Results are available upon request.

[15] Match group 3 is excluded from the experiment.

Figure 6: Education



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 aged individuals who have maybe left school so many years ago that their skills are deemed insufficient. We will proceed treating the sample as two subsamples. As a robustness check, we have evaluated the experiment using just one pooled sample. The results are as expected weighted averages of the subgroup analysis that we will report below. The results are available in the appendix Figures A.4 and A.5.

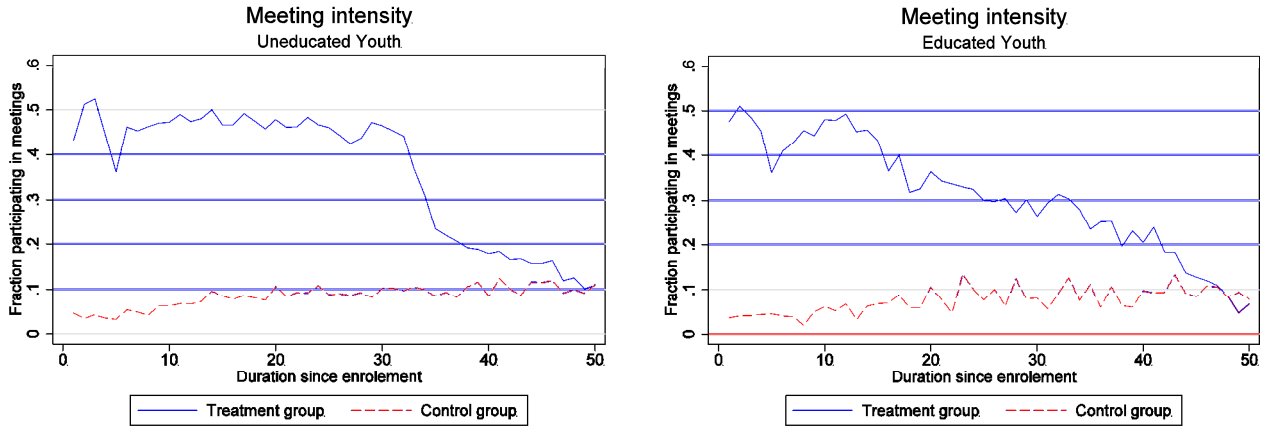
4.3 Implementation

In this subsection we present evidence on the actual implementation of the experiment. To illustrate the degree of compliance to 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 allows in addition for implementation lags such as delayed meetings due to sickness, planning in the job centre etc. The figures should be regarded as lower bounds 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.

4.3.1 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" behavior. For uneducated youth, 50 % 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% of the individuals attend meetings but this fraction declines almost linearly over time.

Figure 7: Meeting intensity



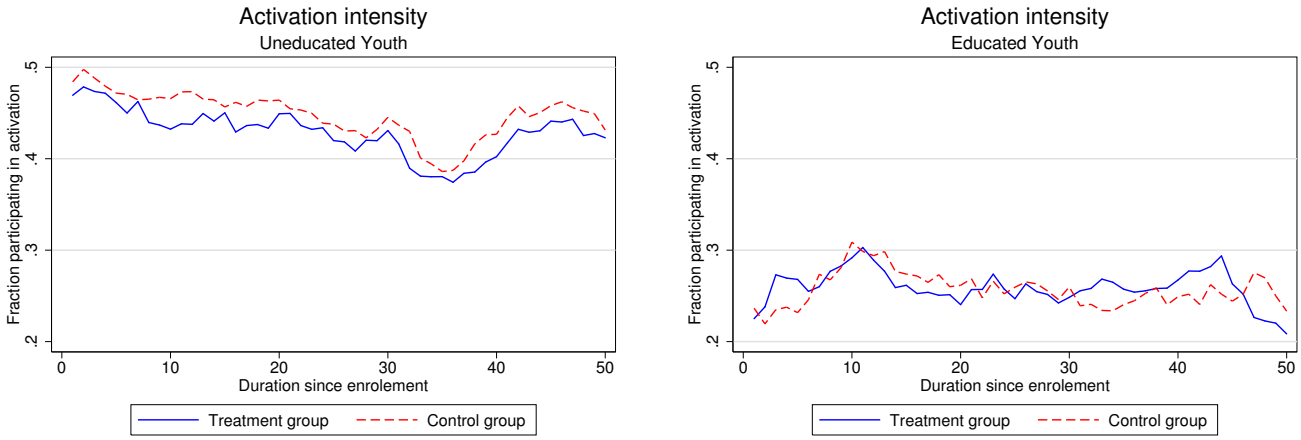
The observed treatment intensities imply that an uneducated (educated) individual who is unemployed over the whole treatment horizon will have participated in an average of 18 meetings (15 meetings), compared to around 4 meetings in the control group. From the observed treatment intensity we can therefore 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% of all meetings held during the treatment period are telephone meetings and only around 55% take place at the job centre. The typical content of a meeting consists of both a monitoring (are benefit requirements fulfilled etc.) and a counseling part. *Ex post*, some caseworkers have mentioned concerns that they had difficulties securing a proper amount of content/progression in the meetings due to the high meeting frequency.

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 control group amount is roughly similar to the numbers presented above.

4.3.2 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



First of all, it is clear from the figures that the "normal" use of activation programmes is much larger for uneducated individuals compared to those who hold an education. For uneducated individuals, if anything, it appears that the control group participates in activation slightly more that the treatment group, in spite of the treatment protocol which prescribes that these individuals should be enrolled into some kind of activation programme already after 6 weeks. Individuals with education are also not activated more than their control counterparts.

Looking at the types of activation programmes that individuals participate in, we generally see that individuals without education participate in activation programmess with educational purposes and only around 10% 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

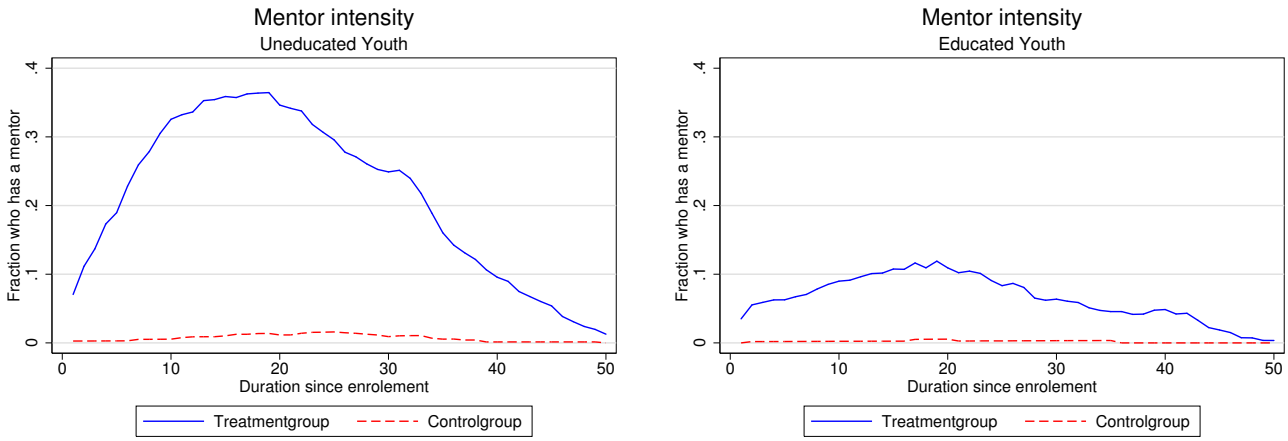
weeks of unemployment. As could be expected, since the educated group is more "employable", a larger share of activation programmes are work-related, around 60% in the control group and around 70% in the treatment group.

Summarizing the implementation analysis of activation programmes, we note that job centres hardly comply to 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.

4.3.3 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.

Figure 9: Mentor intensity



Clearly there is not 100% compliance to the treatment requirement either. At its peak, less than 40% of the individuals without education are assigned a mentor and on average around 20% have one during the first 40 weeks. A number of individuals (20% of those who have a mentor) experience several mentors during their unemployment spell, and in 50% of the assignment cases, the mentor is simply a caseworker at the job centre. Only 20% of the individuals, who were assigned to a mentor, meet with the mentor for more than a total of 4 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.

Ex post, some job centres argued that they found it less useful to assign mentors as 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, and this 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% of the treatment group participates 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 programme than those mentioned in the bottom of Table 1. This gives the job centre the possibility to let 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 of 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, again 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.

5. 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 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.

5.1 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. 3 years after enrolment into the experiment only around 20% 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 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 residual variation and hence estimated standard errors). From here it is clear that there are significant differences in the employment probability between the treatment and control group. Treatment group members are around 3% 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 3 weeks more in employment after 150 weeks, this difference is significant at the 5 % level.

[16] The graphs showing accumulated outcomes also contain confidence bands, which are calculated using a bootstrap procedure presented in Maibom *et al.* (2013). 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

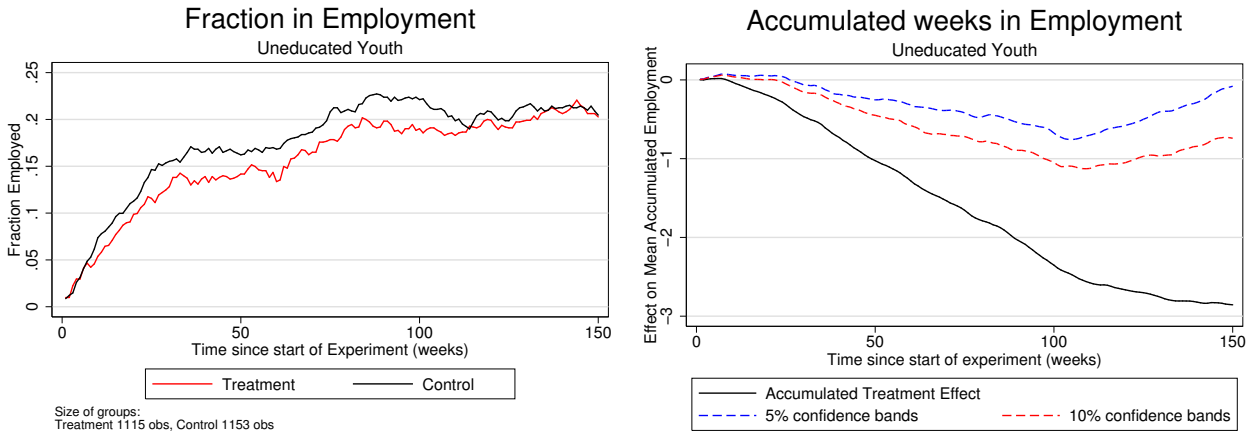


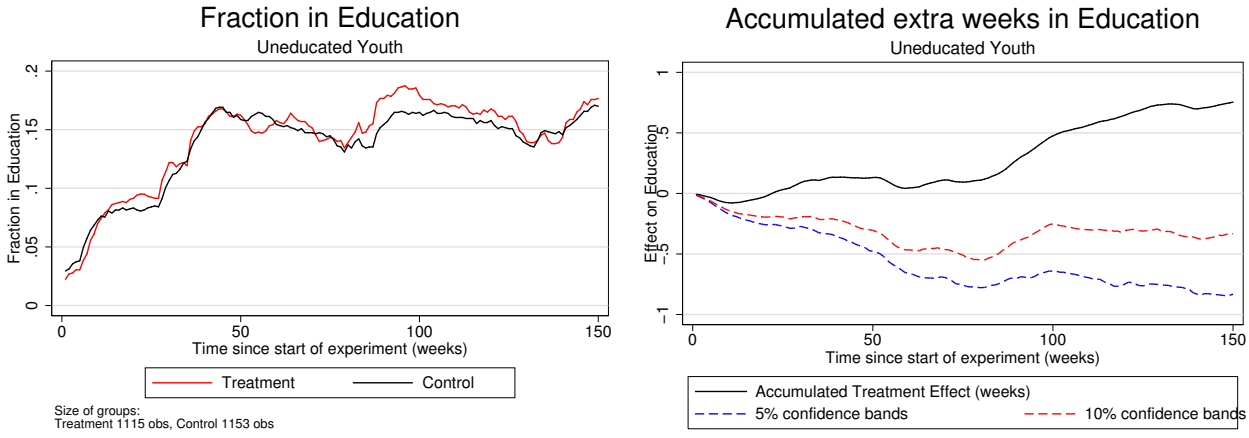
Table 3: Effect of treatment for uneducated youths at a specific time since enrolment

Treatment group indicator	After 20 weeks	After 40 weeks	After 60 weeks	After 100 weeks	After 150 weeks
Employment	-0.0165	-0.0292*	-0.0374**	-0.0356**	-0.055
	(-1.28)	(-1.94)	(-2.49)	(-2.13)	(-0.33)
Education	0.009	0.001	0.006	0.017	0.010
	(0.79)	(0.07)	(0.41)	(1.10)	(0.65)

* 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 week by the end of the observation period and is not statistically significant.

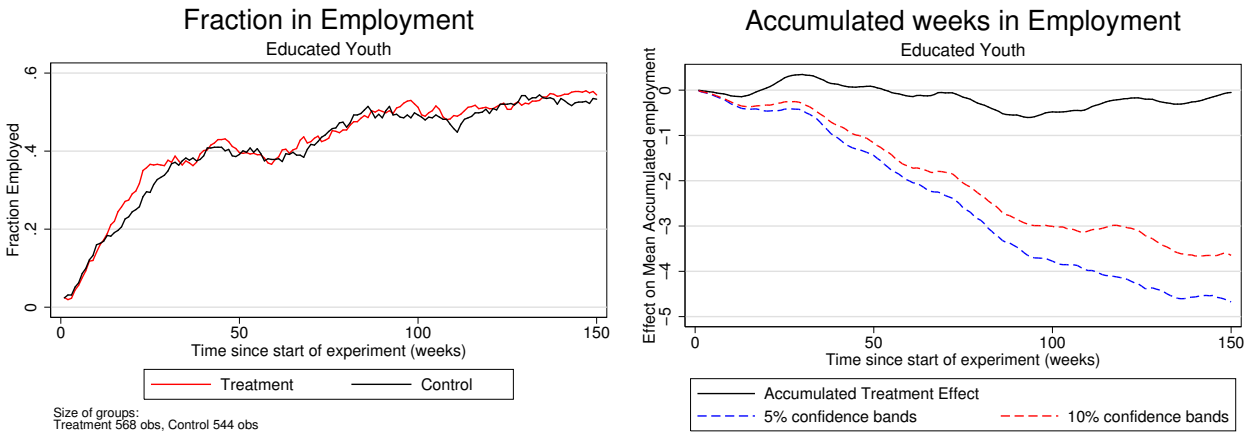
Figure 11: Effect on education, uneducated youth



We can therefore 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 prolong 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.

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 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 leave unemployment initially, and the remaining stock of unemployed are the less employable or find less stable employment relations.

Figure 12: Effect on employment, educated youth



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 the weeks 24-26 (not reported). After 150 weeks the treatment group has on average no extra weeks in employment than the control group.

[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 the appendix Figure A.3

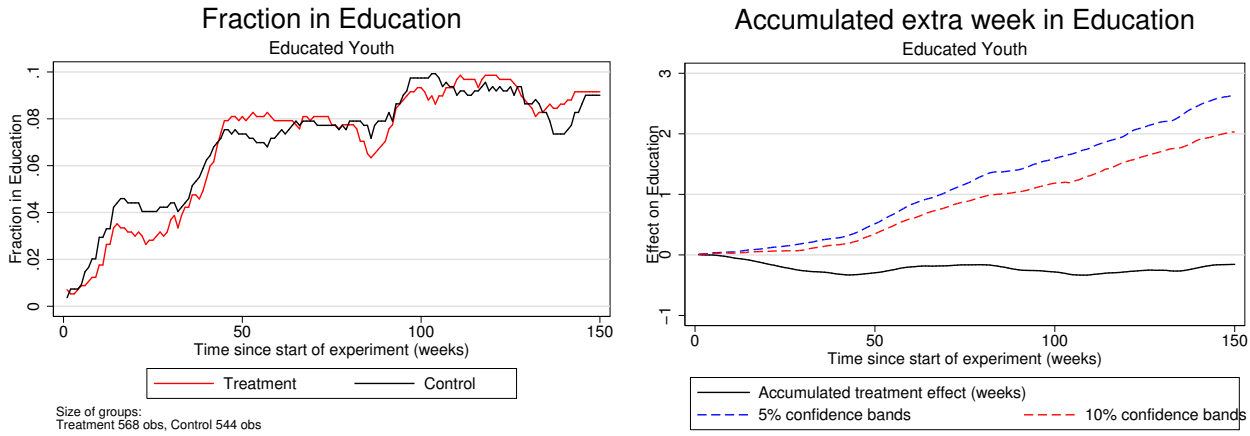
Table 4: Effect of treatment for educated youth at specific times since enrolment

Treatment group indicator	After 20 weeks	After 40 weeks	After 60 weeks	After 100 Weeks	After 150 weeks
Employment	0.0423	0.0097	0.001	0.026	0.012
	(1.59)	(0.33)	(0.03)	(0.88)	(0.39)
Education	-0.0139	-0.008	0.005	-0.006	-0.0007
	(-1.23)	(-0.58)	(0.30)	(-0-35)	(-0.04)

* 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%). There are differences in the fractions but when accumulated we get a very small negative effect (insignificant).

Figure 13: Effect on education, educated youth



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 a 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 into education does 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. We have therefore looked into the educational data provided by Statistics Denmark within the timeframe of our study.

Less than 2% of individuals without education have 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).

5.2 Sickness benefits

We have also analyzed exits into sickness benefits. Although exits to sickness benefits occur less frequently, Figure 14 shows that there are marginally significant positive accumulated effects at the 10% level for both groups of unemployed youth.

Figure 14: Effect on sickness benefits

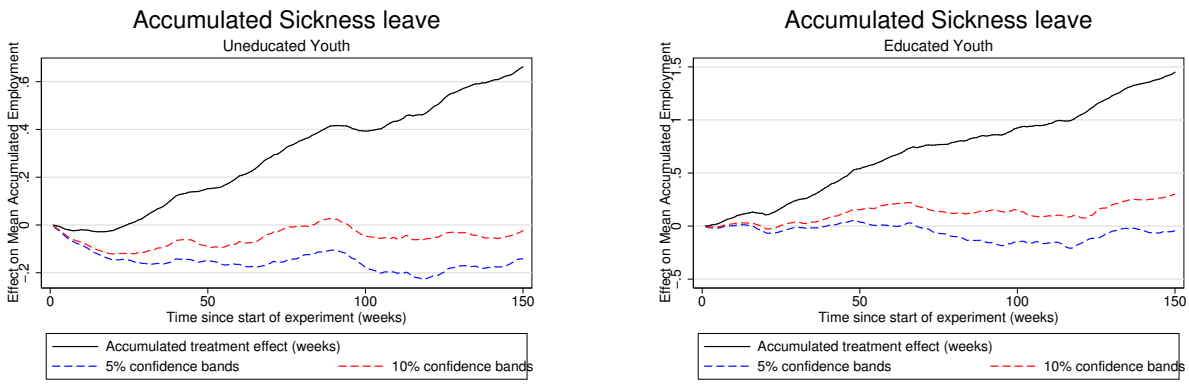


Figure 14 shows that uneducated youth accumulate around 0.6 weeks more sickness benefits, whereas the educated youth accumulate close to 1.5 weeks more. It therefore suggests that a possible downside of intensifying ALMPs is that it can push unemployed workers into sickness

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.

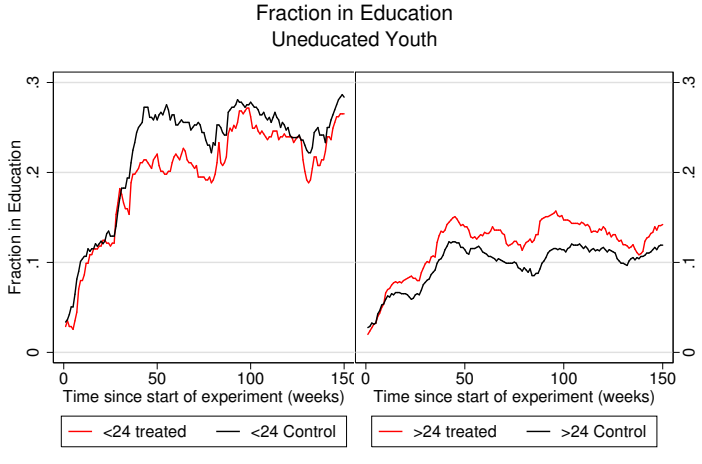
5.3 Heterogeneous treatment effects

The section above has identified average (ITT) effects, in this subsection we look further into whether effects are particularly (un)favorable for specific subgroups and thus whether there are 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% level) are that one job centre (Vejle) seems to be doing particularly bad for uneducated treated individuals 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 prevails.

With respect to accumulated weeks of education we find some differences in the age dimension. Uneducated treated individuals below the age of 24 accumulate 3 weeks less in education over the 150 weeks studied whereas the older counterparts spend more time in education (4 weeks) than their corresponding controls (the pattern is illustrated in Figure 15). Both effects are significant at the 5% level in our regressions (results are available on request). As these effects counteract eachother we see no effects at the aggregate level.

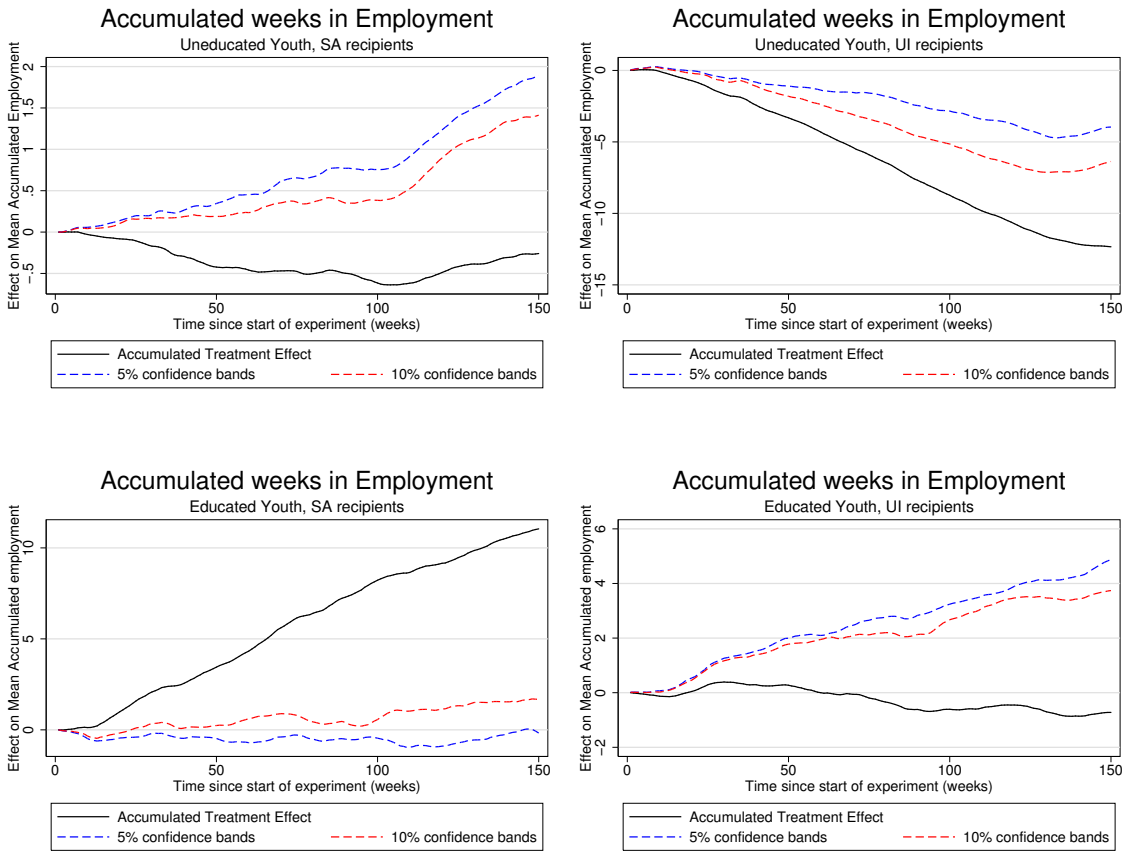
Figure 15: Effect on education, uneducated youth by age group



Size of treatment groups:
Young 313 obs, Old 802 obs

We also find significant interactions with respect to ethnicity, which implies that non-western immigrants in the treatment group perform particular bad with respect to education enrollment compared to their control counterparts (11 weeks less). We have not found a 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



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 stems primarily 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.

6. 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 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 a 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 to shift focus from classroom training to work practice and more firm-based job training. Several components of the programme were not implemented according to protocol, and this underlines the importance of contrasting intended treatment with the actual one for improving our understanding of the effects (or lack of) from the experiments. Our analysis documents that the main difference between the treatment and 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 control groups are rather small both in the employment and the education dimension. There is some indication that the exit to employment was positively affected in the period in which the meetings took place, but the size and robustness of the effect is small. At the

end of the observation period the treatment group had accumulated close to 1 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 meeting 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 M. Svarer, 2012, The role of workfare in striking a balance between incentives and insurance in the labour market, *Economica*, to appear

Andersen, T.M., J. Maibom, M. Svarer, and A. Sørensen, 2013, Are the costs of recessions cohort specific?, manuscript*,* Aarhus University

Berg, G. J. van den, L. Kjærsgaard & M. Rosholm (2012) To Meet or Not to Meet (Your Case Worker) – That is the Question. IZA Discussion Paper 6476, IZA Bonn.

Blundell, R., Costa-Dias, M., Meghir, C., & van Reenen, J., 2004. "Evaluating the employment impact of a mandatory job search program", Journal of the European Economic Association, 2(4), 569-610.

Caliendo, M., Künn, S. and Schmidl, R., 2011: "Fighting Youth Unemployment: The effects of Active Labour Market Policies", *IZA DP 6222.*

Card, D., Kluve, J. and A. Weber (2010) "Active Labor Market Policy Evaluations – A Metaanalysis" *The 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., J. Kluve & S. Schaffner (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. & Neumann T. C. (2012), Estimating The Effects of Length of Exposure to Instruction in a Training Program: The Case of Job Corps. *The Review of Economics and Statistics,* 94, 153-171.

Forslund A., P. Fredriksson & J. Vikström, 2011, What active labor market policy works best in a recession?, *Nordic Economic Policy Review*, 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(10-11):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. , M. Rosholm, and M. Svarer, 2012, Experimental evidence on the effects of early meetings and activation, IZA Discussion Paper 6970, IZA Bonn.

OECD, 2013, Employment Outlook.

Rosholm, M., 2008, Experimental evidence on the nature of the Danish employment miracle, IZA *Discussion Paper, No. 3620.*

Rosholm, M. & M. Svarer, 2008, The Threat Effect of Active Labour Market Programmes, *Scandinavian Journal of Economics*, 110(2), 385-401.

Zhang, J., Rubin, D. & 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	1697	1683	3380

Note: Week 1 is week 45 in 2009

Table A.2: Descriptive statistics

	Uneducated youth		Educated youth	
Characteristics	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	1153	1115	544	568

Table A.3: Descriptive statistics (pooled sample)

	Pooled sample	
Characteristics	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 ε (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	1697	1683

Figure A.1: Precautionary programmes

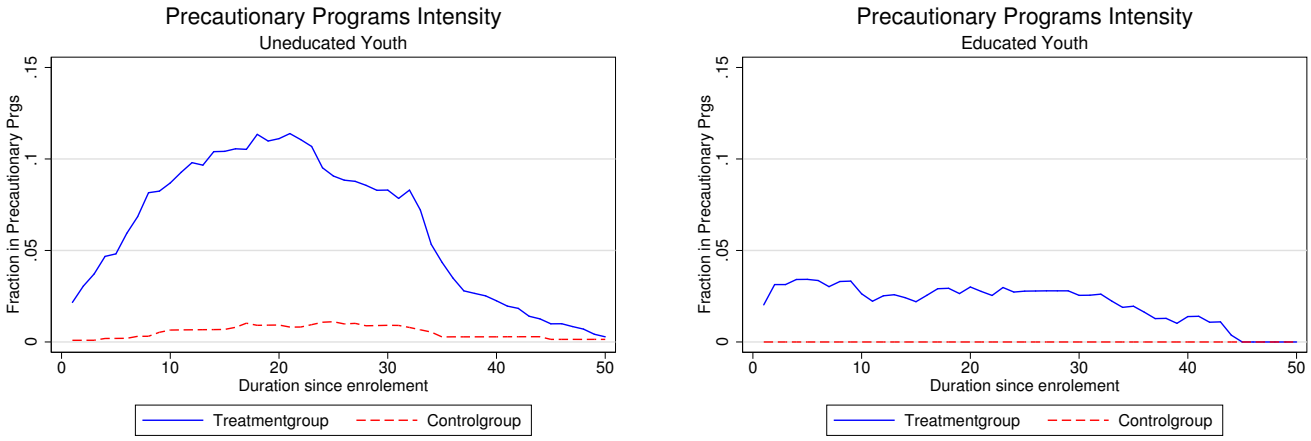


Figure A.2:

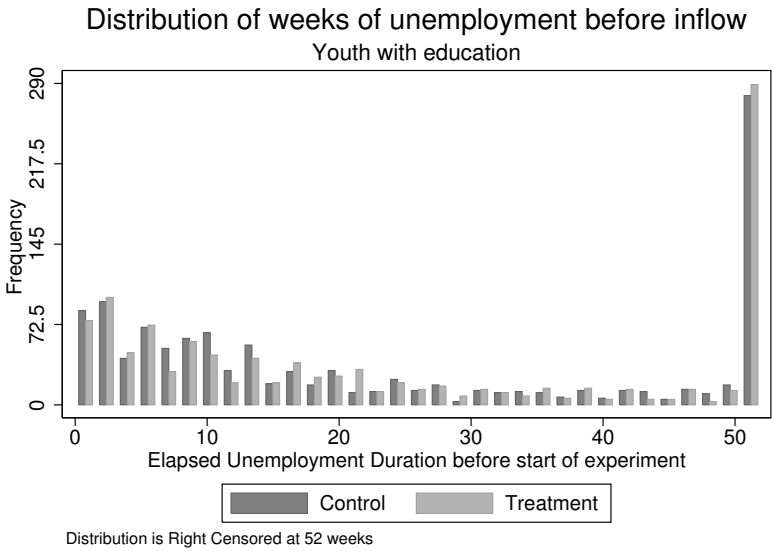


Figure A.3: Excluding elapsed unemployment duration as an explanatory variable

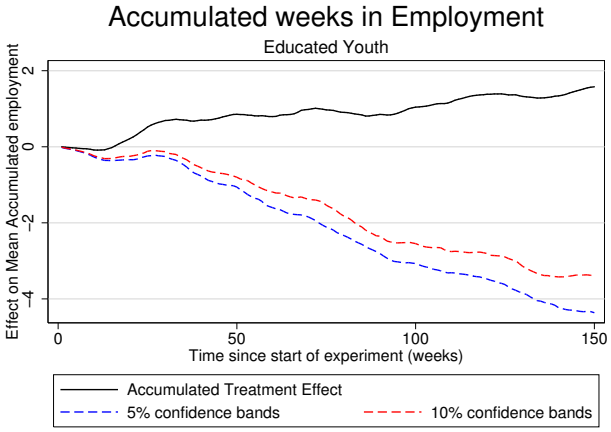
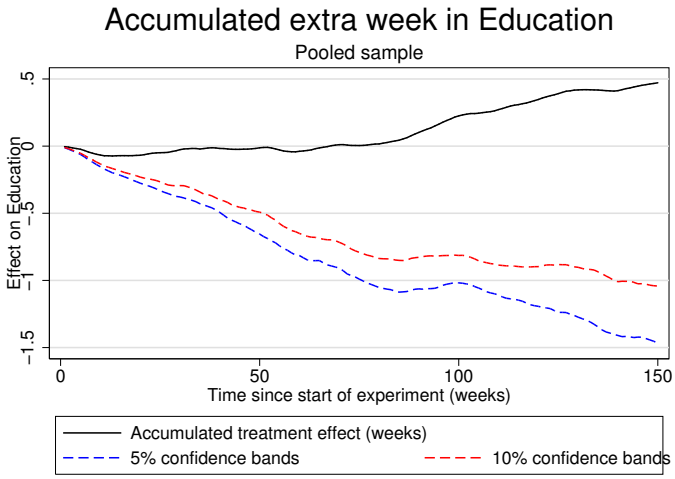


Figure A.4: Pooled sample



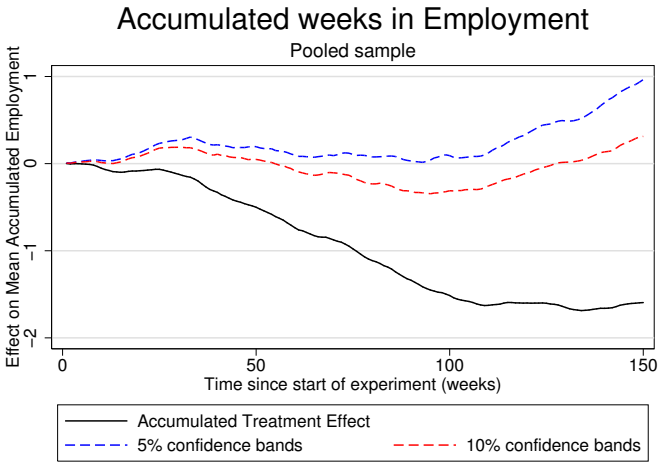


Figure A.5: Pooled sample