Experimental Evidence on the Effects of Early Meetings and Activation *

Jonas Maibom Pedersen
Aarhus University, CAFE

Michael Rosholm†
Aarhus University, CAFE and IZA

Michael Svarer
Aarhus University, CAFE and IZA

May 22, 2013

Abstract

We analyze the effects of four randomized social experiments, involving early and intensive active labour market policy, conducted in Denmark in 2008. The experiments entailed different combinations of early and intensive treatment in terms of meetings and active labour market programmes. We find that frequent meetings between newly unemployed workers and case workers can increase employment rates over the next two years by up to 10 weeks. The effects continue to grow over the whole 4 year horizon studied. For men, we find evidence of a threat effect of having to participate in early active labour market programmes, while no such effect is found for women. We conduct a cost-benefit analysis of each of the four experiments and find that meetings yield the largest net benefits.

JEL-Codes: J64, J68

Keywords: Randomized social experiment, treatment effect, active labour market policy, cost-benefit analysis

*Acknowledgement: We are grateful to the Danish Labour Market Board for making data available and for the CAFE grant enabling part of this research, to seminar participants at IAB in Nürnberg, the University of Bergen and SOFI at Stockholm University for valuable comments.

†Corresponding author: Michael Rosholm, Department of Economics and Business, Aarhus University, Fuglesangs Allé 4, 8210 Aarhus V, Denmark. Email: rom@asb.dk, phone: +45 87164832.

1 Introduction

In this paper we present results from four randomized social experiments involving early and intensive active labour market policies. The participants in the experiments were newly unemployed unemployment insurance (UI) benefit recipients in Denmark in 2008. The experimental treatments consisted of dramatic increases in the frequency of early counseling and monitoring, and of early mandatory active labour market programmes (ALMPs). The experiments shed light on the nature of active labour market policy impacts in both the long and short run, and in particular on their differential effects on men and women, but we also investigate differential effects between young workers and older workers, across different cyclical conditions, and impacts on unemployment and job durations.

In 2005, the first Danish randomized policy experiment was conducted in the labour market,[1] Quickly Back to Work (QBW1, hereafter), see Graversen & van Ours (2008a & 2008b), Rosholm (2008), and Vikström *et al.* (2011). It involved a dramatic intensification of active labour market policies in the sense of providing newly unemployed workers with a sequence of treatments at a very early stage of unemployment. This experiment involved a number of active labour market policy instruments, i.e. job search training courses, frequent meetings with caseworkers, and early mandatory activation. The results were strong: those who were randomized into the treatment group experienced a 3 week reduction in unemployment duration compared to those in the control group, and a cost-benefit analysis conducted by the Danish Economic Council (2007) demonstrated large net gains. However, there was uncertainty concerning the actual source of the success; was it the combined package of treatments, or were certain elements of the package crucial? Could even better results be obtained by focusing on single elements of the package, or could similar effects be obtained at lower costs? To shed light on these questions, a new set of four randomized experiments were designed, Quickly Back to Work 2 (QBW2, henceforth). These experiments were designed in such a way that they would yield estimates of the effects of single elements of the QBW1 package. The treatments consist of weekly group meetings between a group of unemployed workers and 1-2 caseworkers (A), bi-weekly individual meetings between one unemployed worker and one caseworker (B), early programme participation (C), and a combination of group meetings and early

[1] Strictly speaking another randomized experiment was conducted already in 1994 although the target group was different, for more on this experiment see Rosholm and Skipper (2009).

programme participation (D).

Active labour market policies are a pivotal element in the so-called Flexicurity model for the labour market, which the EU commission recommends to its member states, referring to Denmark as a model case (European Commission, 2007). The Flexicurity model consists of three components; 1) flexible hiring and firing rules and regulations (that is, low levels of employment protection legislation), similar in spirit to those in Anglo-Saxon countries, 2) a generous and universal unemployment insurance and social assistance system, similar to that in other Scandinavian welfare economies, and 3) *a very active labour market policy* ensuring the availability and the qualification level of the workforce. In the 1980s, when unemployment rates were persistently high, the first two features of the Flexicurity model - flexibility in the labour market and a tight social safety net - were already present in the Danish labour market, but active labour market policies were only in their infant stages and not nearly as intensive as they have become today. In the 1990s the equilibrium unemployment rate fell simultaneously with a gradual intensification of the use of active labour market policy measures, and therefore, many observers have seen intensive active labour market policies as *the* crucial component in the Flexicurity model (see e.g. Andersen & Svarer, 2007). Our paper sheds further light on the validity of such an assessment.

We find large positive effects of some of the policy components investigated, and there are enlightening differences, especially with respect to the type of policy, the gender of the unemployed worker, the age of the participants, and the cyclical conditions. Counseling in the form of individual meetings between caseworkers and unemployed workers has very large effects, arising later for men than for women. Mandatory ALMPs have a large (threat-) effect for men, while there is a small temporary negative (lock-in) effect for women. Moreover, the threat effect appear to be present only when labour market conditions are good - we show this point by exploiting that the experiment was conducted right during the tipping point of the business cycle in 2008. Hence, differences in the week of enrollment into the experiment (week of inflow) reflect different cyclical conditions. The analysis demonstrates that early and frequent meetings with unemployed workers is an efficient way of assisting newly unemployed workers. According to e.g. Card *et al.* (2010), an often neglected aspect of policy evaluation is a consideration of programme costs against its prospective gains. We conduct a cost-benefit analysis and find that individual meetings are also the most beneficial instrument from an economic point of view.

The rest of the paper is organized as follows: first, we provide an overview of the literature on active labour market policy effects with a special emphasis on the effects of meetings between caseworkers and clients. In section 3 we describe the social experiments and the data used for the subsequent analysis. Section 4 contains a presentation of our main results and we also discuss the presence of heterogeneous effects with respect to business cycle and age. In section 5 we look closer at dynamics in the short run (first 2-3 years) and present results from a two-state duration model of employment and unemployment. In section 6 we perform a cost-benefit analysis of each experiment focussing on the 2-3 year horizon. Finally, section 7 contains a discussion of further research, policy implications, and a conclusion.

2 A review of related literature

There is an extensive literature on the impacts of 'traditional' activation programmes, see e.g. Heckman *et al.* (1999), Card *et al.* (2010), and Kluve (2010). Policy impacts are typically modest and not always positive.[2] The best effects are found for employment subsidies, while training programmes sometimes have positive effects when aimed at disadvantaged workers. Public job creation shows more negative than positive effects, possibly due to so-called lock-in effects.

One important aspect of activation policies is the presence of *ex ante* effects, which potentially apply to the enitre pool of unemployed and not just those entering actual programmes. For instance three observational studies based on Danish data and one on German data show that unemployed workers tend to leave unemployment faster when the probability of activation increases (Geerdsen, 2006; Geerdsen & Holm, 2007; Rosholm & Svarer, 2008; van den Berg *et al.*, 2009). Hence, *ex ante* effects may change our conclusion on the overall effectiveness of active policies towards a more positive view.

Another element of active labour market policies is that of meetings between caseworkers and the unemployed. Meetings are a cornerstone of active labour market policies: First, unemployed workers typically register their entry into unemployment at meetings, where also their eligibility for receiving e.g. UI benefits is assessed. Second, search effort is monitored at meetings, e.g. in the form of required documentation of job applications.

[2] For example, Card *et al.* (2010) find that, in the short term, only 39% of the surveyed studies found significantly positive effects. In the medium term, effects were slightly better, with 50% being significantly positive.

Often, the caseworker has to assess whether the unemployed person is available for work. If there is non-compliance in the form of no-show, insufficient search or availability, a sanction may be issued. Third, counseling takes place at meetings. Counseling can be help with the creation of a CV and writing applications, guidance in making career choices, general job search assistance etc. Fourthly, there may be direct referral to vacant jobs. Finally, future participation in ALMPs is discussed and planned at meetings.

Meeting attendance at certain regular intervals during unemployment periods is often mandatory, and no-show may lead to a sanction.

Thus, summarizing the differential aspects of meetings from above, meetings have the potential of affecting individual behaviour *ex ante* (fear of a sanction) as well *ex post* (search requirements, job search assistance, and potential behavioural effects from perceived future activation). In the following, we briefly review the evidence on *ex ante* effects and *ex post* effects as three of the four experiments in our study have an explicit focus on meetings.

2.1 *Ex ante* effects of meetings

The literature on the *ex ante* effects of meetings is rather new. Generally, we find that there are *ex ante* effects, although this does not seem to be the case for long-term unemployed workers. Hägglund (2006) reports from a randomized experiment conducted in Sweden and shows that, for a broad group of unemployed workers, an invitation to a meeting, aimed at monitoring search activity and assisting with more effective job search, led to an increase in the exit rate into employment by 46% already before the meeting took (or should have taken) place.

Cockx & Dejemeppe (2007) analyze the effect of a reform in Belgium in 2004 leading to more intensive monitoring of job search activities of workers with more than 7 months of unemployment. A letter was sent out to the unemployed informing them of this reform, and in general the authors find no impact. However, for better educated workers they do find significantly positive *ex ante* effects.

Black *et al.* (2003) study a profiling tool aimed at identifying workers at risk of long-term unemployment (LTU). Workers with a high estimated LTU-risk were invited to a meeting with the aim of placement in an activation programme. The selection of whom to invite was randomized, and workers reacted to an invitation by increasing job finding rates after receipt of the letter. Unemployment duration was shortened by 2.2 weeks, and the income of invited workers was higher than for the controls during the year after receipt of the letter.

2.2 *Ex post* effects of meetings: counseling

Ex post effects from counseling are generally positive, although there, like in the case of ex ante effects from above, is a tendency that the group of long-term unemployed respond less favourably.

Gorter & Kalb (1996) study a randomized experiment conducted in Netherlands, where the time allocated to counseling with caseworkers was increased. They find positive but insignificant effects on the exit rate from unemployment. 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 effectivising job search. The authors construct a difference-in-differences estimator of the impact of the Gateway. They find an increase in the employment rate of 5%-points 4 months after entry into the Gateway.

Crepon *et al.* (2005) analyze a reform implemented in France in 2001, which increased counseling without altering the amount of monitoring. They found a tendency that programmes aimed at 'better' workers increased the exit rate from unemployment, and that all programmes increased subsequent employment duration.

Hägglund (2009) analyze a social experiment conducted in Sweden, where unemployed youth were offered counseling. He found that, when aimed at all unemployed youth, there were positive effects on the exit rate from unemployment, while this was not the case when the treatment was only aimed at long-term unemployed youth. This corresponds quite well with results from a randomized Danish study, Rosholm & Svarer (2009b), where intensive counseling to long term welfare recipients did not lead to more employment.

Meyer (1995) studies five U.S. experiments aimed at better counseling. Four of the five experiments led to significant reductions in subsequent unemployment, ranging from 0.5 to 4.3 weeks.

Dolton & O'Neill (1996; 2002) analyzed the ReStart program; In England, an offer of meetings every six months for workers with more than six months of unemployment was introduced in 1989. The aim was an effectivisation of search behaviour (counseling part) and an assessment of the availability for work. A randomized experiment was conducted, and Dolton & O'Neill (1996) showed that this led to a 30% increase in exit rate from unemployment, and Dolton & O'Neill (2002) showed that five years after entry into the programme, the treatment group still had significantly less unemployment than the controls.

2.3 *Ex post* effects of meetings: Monitoring

Quite a few studies analyze the effect of increasing the rate of monitoring of unemployed workers. They tend to find positive or zero effects in the sense of reduced subsequent unemployment duration and/or increases in employment rates.

Ashenfelter *et al.* (2005) report from a U.S. randomized experiment, where search requirements were stricter for the treatment group. The increase in monitoring was only implemented during the first couple of weeks of unemployment. There was no effect of the increased monitoring on neither unemployment duration nor on the costs of unemployment benefits.

Klepinger *et al.* (2002) study another U.S randomized experiment, where unemployed workers are randomized into one of four treatments (and a control group), which involved closer monitoring of different degree and type. Unemployment duration was reduced by 5-7%. Johnson & Klepinger (1994) find similar results based on another U.S. experiment.

Van den Berg & van der Klaauw (2006) study a randomized experiment in Rotterdam with monthly meetings involving increased monitoring. They found a switch from informal to formal search channels as a result of the search and documentation requirements, and positive but insignificant effects on the exit rate from unemployment to employment. Keeley & Robins (1985) find something similar for the U.S. using observational data.

McVicar (2008) exploits exogenous variation in the number of meetings held with the aim of monitoring search activity in the U.K. The exogeneity comes from cancellation of meetings due to reconstruction work on the public employment offices. He found that exit rates from unemployment fell when meetings were cancelled.

Petrongolo (2009) and Manning (2009) both analyze the Job Seekers Allowance programme implemented in the U.K in 1996. This involved frequent meetings with a caseworker to document job search activity. They use observational data and exogenous variation in the timing of the treatment relative to the start of unemployment and find increasing exit rates out of unemployment. However, this is mainly caused by an increased exit rate into incapacity benefits.

2.4 Other aspects of meetings

Other aspects of meetings include *ex post* effects from the match between a caseworker and an unemployed and also effects from increasing the quality of the meetings. It also includes effects from actions taken by the caseworkers in relation to meetings, for instance job assignments and sanctions.

Behncke *et al.* (2008, 2010a, and 2010b) investigate the importance of the caseworker, using a survey among Swiss caseworkers linked with a data set on their unemployed workers. They show that unemployed who have a caseworker with a strong network among employers have 3%-points higher employment rates (EPR) subsequently, that workers assigned to caseworkers who are less 'cooperative' have 2%-points larger EPR, and that if caseworkers and clients are similar with respect to age, gender, and educational level, then the unemployed worker has a 4%-point higher EPR. This trilogy of studies therefore adds further insight into the importance of meetings via caseworker contacts; caseworkers provide contacts to potential employers; they put pressure on unemployed workers to search harder, and they can relate to their situation and provide useful insights on job search. These findings therefore suggest ways for policy makers to improve *ex post* effects of meetings.

Hainmueller *et al.* (2009) study 14 German jobcentres, which were allowed to hire more caseworkers so as to halve their case load. The increase in the number of caseworkers reduced the number of unemployed by 10%. Hofmann *et al.* (2010) show that the impact was larger if the additional resources were devoted to either dealing with the unemployed workers through meetings or improving the employer network, while there was no effect if they were devoted to strengthening the organizational structure of the jobcentre.

Fougere *et al.* (2009) analyse the impact of job assignments by caseworkers using French data. Assignments reduce the search effort of unemployed workers, but the assignments themselves more than outweigh this reduction, such that job finding is faster with job assignments. They find particularly strong effects for the unskilled. Engström *et al.* (2009) find no effects of assignments in a Swedish context.

Finally, if unemployed workers do not show up for meetings, or if their search efforts are deemed insufficient, sanctions can be issued; in this sense sanctions are also a result of the contacts between unemployed workers and their caseworkers. A number of studies have investigated the effects of sanctions. Lalive *et al.* (2005) and Arni *et al.* (2009) both find that the threat of a sanction may be imposed increase the job finding rates of unemployed workers. Van den Berg *et al.* (2004), Abbring *et al.* (2005), Lalive *et al.* (2005), van den Berg & Vikström (2009), Røed & Weslie (2007), and Svarer (2011) all find that sanctions issued increase the subsequent job finding rate dramatically. The range of the effect on the job finding rate is from 25% to 100% depending on the country and the severity of the sanction. Finally, Arni *et al.* (2009) and van den Berg & Vikström (2009) show that those who are sanctioned find less favourable employment than the unsanctioned in terms of wages and job duration.

2.5 Equilibrium effects

Meetings may have important general equilibrium effects, such as negative effects on untreated individuals due to increased competition in the labour market from the treated individuals - this would imply that the control group performs worse than they would have done without the presence of treatment. Positive effects could come from equilibrium responses from firms who increase the number of vacancies due to the lower mean duration of a vacancy (if treated individuals search more - or better). However, our knowledge concerning the presence of general equilibrium effects and their size with respect to meetings is still limited.

Crepon *et al.* (2012) implement a two-level randomized experiment of a counseling programme in France directed towards young graduates that spent at least 6 months in unemployment. The two-level approach consists of a randomization at the job centre level in the proportion of individuals selected into treatment, and of a randomization scheme within jobcentres into treatment and control groups. This allows testing for externalities on untreated workers (comparing untreated workers in areas with treatment to workers in areas with no treatment), but also to investigate whether the actual treatment effect varies with the proportion of individuals selected into treatment. They find no evidence of negative externalities or displacement effects.

Gautier *et.al.* (2012) analyze general equilibrium effects of the QBW1 experiment in Denmark, using the fact that it was only implemented in 2 out of 15 counties. They compare control group workers in the treatment region with workers in unaffected regions using a difference-in-difference approach. They find some evidence that the job finding rate of the control group workers declines due to the treatment. Furthermore, they construct and estimate a structural search model to determine the effects of implementation of the treatment on a country wide basis and find that negative congestion effects are present and lower the overall treatment effect, which is however still positive and leads to lower equilibrium unemployment.

3 The Danish labour market and the experiments

This section presents the experimental design and puts it into the context of the Danish labour market. We then proceed by illustrating the degree of implementation of the treatment protocol, and we look for systematic differences between compliers and non-compliers within the treatment groups. Finally we discuss the experimental design and its relation relation to minimum detectable effects. First we will briefly describe the organization of the Danish labour market with a particular focus on active labour market policy.

The Danish labour market is characterized as flexible with less employment protection legislation than most continental European countries and much more labour turnover (see e.g. OECD, 2009). The Danish labour market has a tight social security net with near-universal eligibility for income transfers. Moreover, active labour market policies are among the most intensive in OECD, with around 1.5% of GDP spent per year on active policies.

There are two types of benefits for unemployed workers, UI benefits and social assistance. Approximately 80% of the labour force are members of a UI fund and therefore eligible for UI benefits, while the remaining 20% may receive social assistance (given that they do not have a partner who can provide for them and do not have any savings). UI benefits are essentially a flat rate. As this paper is only concerned with UI benefit recipients, we shall present the policies that apply to them.

The mutual "rights and obligations" principle is a key principle in the current Danish labour market policy. This implies the right of individuals to compensation for the loss of income, but also the obligation to take action to get back into employment. The authorities have an obligation to help the individual improve her situation and has the right to make requirements of the individual concerned.

Under the current rules, an individual who becomes unemployed and is eligible for UI benefits has to register at the local jobcentre. She then has the obligation to attend a meeting with a caseworker at least every 3^{rd} month. She has the right and obligation to participate in an activation programme after 9 months (6 if below 30 years old) of unemployment and subsequently every 26 weeks. These are the labour market policies that will be faced by individuals in the control groups of the four experiments, who will receive this 'treatment as usual'.

3.1 The experiments

The set of randomized experiments analyzed in this paper consists of four separate experiments, each with its own treatment and control group. They were conducted in four different regions in Denmark. They are summarized in Table 1.

Table 1: Overview of the 4 experiments in QBW2

Experiment	Content	Region	Jobcentres
A	Group meeting each week	Northern Jutland	Frederikshavn, Brønderslev, Hjørring
B	Individual meeting w. caseworkers every other week	Copenhagen & Sealand	Gribskov, Roskilde, Ishøj-Vallensbæk Holbæk, Vordingborg
C	Early activation (after 13 weeks)	Mid-Jutland	Aarhus
D	Group meeting each week and early activation	Southern Denmark	Esbjerg, Vejle

The subjects of the experiments are individuals becoming unemployed during weeks 8-29 in 2008 who are eligible for UI benefits. Once an individual registers as unemployed, she is randomized into treatment or control group based on her date of birth. Individuals born on the 16^{th}-31^{st} are assigned to the treatment groups, while those born on the 1^{st} to the 15^{th} are assigned to the control groups. No information was given to the unemployed workers on the selection rule.

The individuals randomized into the treatment groups then receive a letter, during the first week of unemployment, explaining the new treatment to which they will be exposed[3]. This information letter marks the start of the treatment, since the worker may react to the information on the new regime from the day the letter is read. It was not possible to escape treatment by leaving unemployment for a short while and then re-enter later on.

[3]The unemployed is not informed that she is participating in a randomized experiment, but rather that she has been chosen to participate in a pilot study.

In that case, a worker would re-enter the experimental treatment at the stage where she left it.

In all four experiments, the control group receives 'treatment-as-usual', but there may be local variations in the intensity of treatment which will be documented below. The treatmetn group recieves the same treatment plus an extra element, whcih we now present. Starting from the bottom of Table 1, the experiment labeled 'D' is a sort of reference experiment conducted in the region of Southern Denmark. 'D' is partially intended to mimic the QBW1 experiment from 2005-6, although there are some important deviations: meetings are group meetings and there is no two-week JSA course included in the treatment. Hence, this 'reference experiment' consists of less intensive interaction between caseworkers and the unemployed than was the case in QBW1. During the first 13 weeks of unemployment the unemployed worker must attend group meetings with a caseworker and a number of other unemployed workers (typically around 10). If, after 13 weeks of open unemployment, she has not found employment, she has to participate in an ALMP of at least 25 hours per week for at least 13 weeks. After 6 months of unemployment, the experimental treatment ends, and from that point on the rules regarding her treatment is similar to the control group.

The experiment labeled 'A' in Table 1 was conducted in the region of Northern Jutland, and it consisted of weekly group meetings (similar to those in Southern Denmark) during the first 13 weeks of unemployment. After these 13 weeks, the experimental treatment ends. The experiment labeled 'B' in Table 1 was conducted in the region of Copenhagen & Sealand, and consisted of individual meetings with a caseworker every other week for the first 13 weeks of unemployment, that is, a total of 6-7 meetings during the first 13 weeks of unemployment. Again, after 13 weeks of unemployment, those still unemployed in the treatment group would receive the same treatment as the control group. Note that, generally, the stated main intention of both group and individual meetings was counseling of the unemployed; no explicit extra monitoring was required to take place.

Finally, in the experiment labeled 'C', the individual would be required to participate in an activation programme for at least 25 hours per week from week 14 in unemployment until week 26. This experiment was designed specifically to investigate the presence of *ex ante* effects due to the knowledge of having to participate in an activation program, as well as *ex post* effects of actually having participated.[4]

[4] In order to test specifically for the *ex ante* effect, there should have been no actual treatment taking place from week 13 onwards. However, such a setup would not be legal according to the administrative

The aim of the entire set of experiments was to try to disentangle the positive impacts of the QBW1 experiment, and to investigate cost-reducing policies such as group meetings rather than individual meetings. First, the distinction between 'B' and 'C' informs us whether an early programme effect stems mainly from the threat of mandatory program participation (a threat effect) or from an effect generated by meetings, or both. Second, a comparison between 'A' and 'B' would shed light on whether group meetings could achieve the same impacts as individual meetings (at a lower cost). Finally, the comparison of 'A' and 'D' would tell us if the combination including group meetings would achieve better effects than just group meetings, and the comparison of 'D' to 'C' would tell us if a sequence of group meetings followed by an early and intensive activation programme would lead to better effects than just ALMP participation.

Comparisons such as the ones mentioned above rely on an assumption of comparable labour markets between the different regions. Denmark is the OECD country with the smallest regional disparity in terms of GDP per worker. Furthermore, the regions have roughly the same GDP pr. capita (Danish Business Authority, 2009), and all regions contain large cities (by Danish standards). From an international point of view these differences appear small, and therefore we believe that comparisons across experiments are valid.[5] Simple proportions test indicate that the Mid-Jutland region (Experiment C) is different from the other regions in some respects; the fraction of young individuals is higher. This implies that the fraction of married individuals is lower and it also explains the higher degree of income transfer recipients (which includes study grants). Furthermore, the region of Copenhagen & Sealand (Experiment B) has a larger pool of immigrants among all insured unemployed, reflecting the fact that Copenhagen is the capital of Denmark - and the port of entry for most immigrants - therefore a larger fraction of immigrants live there. Finally, Figure 1 shows the estimated Kaplan-Meier survival estimates from the first unemployment spell for the four control groups. These estimates are very similar and therefore support the validity of comparisons between regions keeping the differences mentioned above in mind.[6] The figure also illustrate the very dynamic enviroment in which we do our policy evaluation, half of the control groups will have exited from unemployment after 10 weeks.

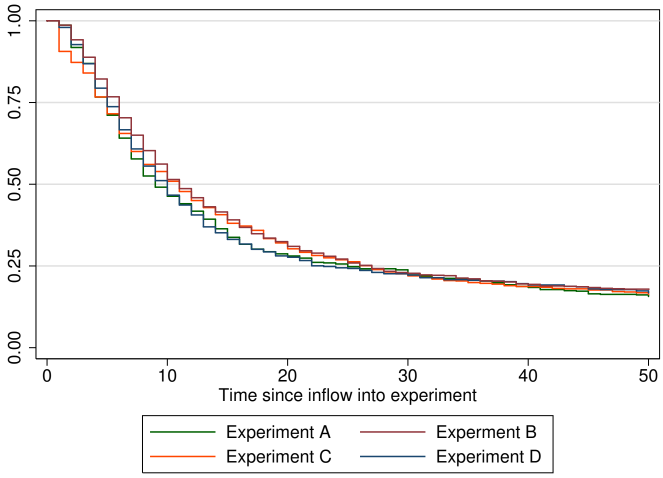
regulations. Moreover, there could have been severe ethical concerns with such an experiment.

[5] In the appendix we have tabulated descriptive statistics for all four control and treatment groups.

[6] Various rank tests do not reject the null hypothesis of equality of survivor functions.

"Failure" is defined as employment, selfsufficiency or education.

Figure 1: Kaplan-Meier survival estimates for control groups



3.2 Data

The data are extracted from administrative registers merged by the National Labour 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. The administrative data are used for determining eligibility for UI benefit receipt and for determining whether the jobcentres meet their requirements in terms of meetings and activation intensities. The information is therefore considered highly reliable.

The event history data set includes detailed information on: labour market status and history (employment, unemployment, in education, on leave, etc.), etnicity, gender, residence, marital status and UI fund membership.

5411 individuals registered as unemployed in one of the 11 jobcentres which were part of the experiments, between week 8 and week 29 of 2008, both weeks inclusive. Their distribution on treatment and control status and on the four experiments can be seen in Table 2.[7]

[7]We have also analyzed the inflow into the experiment and found that the number of individuals entering every week is very similar in the treatment and control group.

Table 2. Composition of the sample

	Men		Women	
Experiment, region	Treatment	Control	Treatment	Control
A (group meetings)	304	303	261	310
B (individual meetings)	376	455	343	371
C (early activation)	393	405	454	428
D (group meetings + early activation)	247	247	266	248

We have tabulated the averages of selected individual characteristics for each of the sub-samples in the appendix. We find no significant deviations from random assignment.

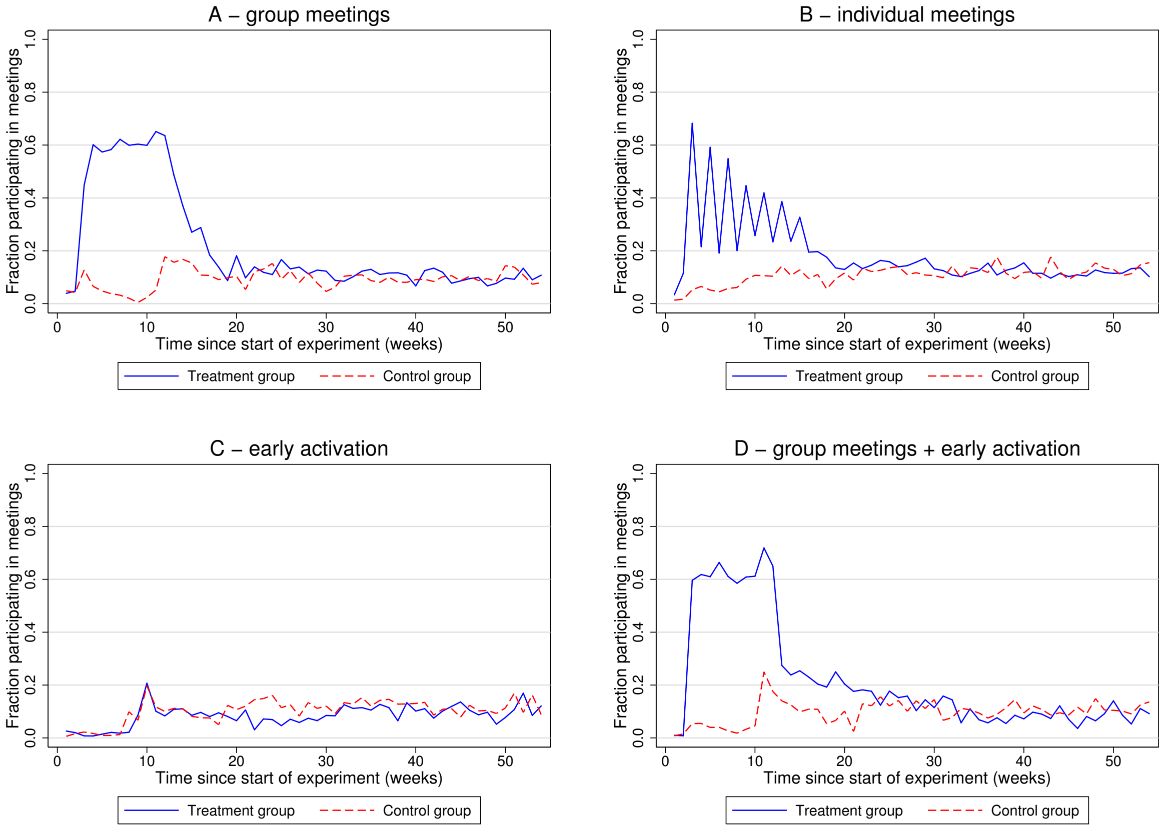
We have weekly information on labour market status, meeting attendance, and programme participation for each person in the experiment. Each person is followed until the end of January, 2013. Labour market status is calculated based on information from the register on payments of public income transfers, which is used to construct the labour market states 'unemployment' and 'other public income transfers'. Data will also tell us whether individuals are employed or not using information from the E-income register, containing information from employers about their employed workers. 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 8-29 in 2008), all individuals can be followed for at least 237 weeks (there are 53 weeks in 2009) and for at most 258 weeks after their entry into unemployment. We can also follow individuals back in time, although the employment information is available only from 2008 and onwards.

3.3 Implementation

In this subsection, we present evidence on the implementation of the four experiments. To show the degree of compliance to the experimental protocol, we show a set of figures on the weekly meeting intensities and activation intensities. We have also tabulated these intensities on gender and we have found no remarkable differences in this dimension.

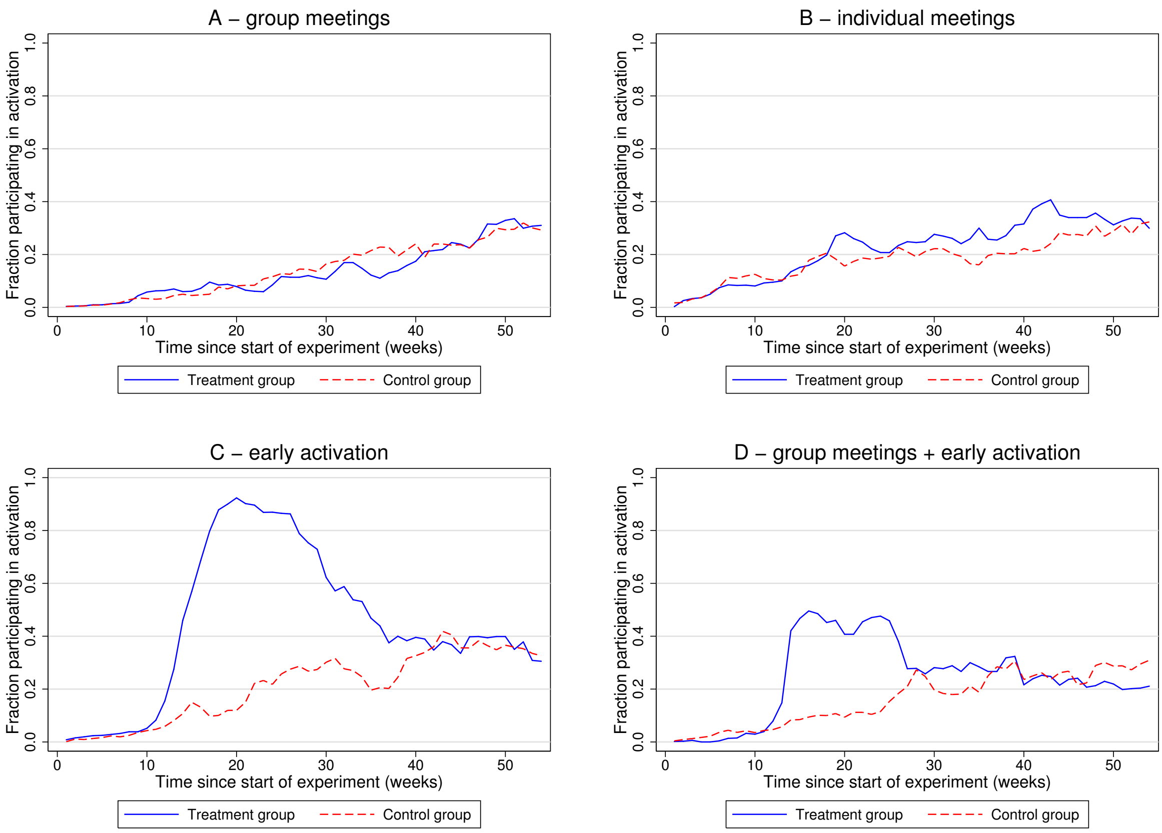
Figure 2: Weekly meeting intensities



Note: Meeting intensities for those who are still unemployed in a given week.

Figure 2 plots the weekly meeting intensity in the 3 regions for the treatment and control group. In Experiments A and D the treatment group were intended to participate in group meetings on a weekly basis. In both projects, we see that only around 60% of the those in the treatment group who were still unemployed in a given week participated in meetings in any of the first 13 weeks. In Experiment B, we observe a saw-tooth pattern reflecting the fortnightly meetings. Summing the meeting intensities for two consecutive weeks, the fortnightly meeting intensity begins around 90% and then falls to about 65% around week 13. In Experiment C there was no intention of extra meetings, and this is also what we observe in the data. Hence, even though participation in meetings does not comply completely to the requirements of the experiment, the treatment groups in the three relevant projects attended significantly more meetings than did the corresponding control groups during the early phases of the unemployment spell. We look more into the characteristics of thoose that actually recieve treatment in the next section. The meeting rate for the treatment and the control groups is the same after the period of the experimental treatment in all regions. Notice, however, that the sequence of intensive meetings continues a few weeks beyond week 13 of the unemployment spell. We interpret this as an implementation lag in the treatment process, as well as a consequence of meetings cancelled earlier in the unemployment spell due to sickness, job search, etc.

Figure 3: Weekly activation intensities



Note: Activation intensities for those who are still unemployed in a given week.

Figure 3 shows weekly activation intensities. In the two projects with scheduled early activation (C and D), we see a sharp increase in the activation intensity around week 13. Again, not everyone in the treatment groups was activated between week 13 and 26, but the activation intensity is much higher for the treatment group than for the control group, especially in Experiment C. In Experiment D, which was conducted in two jobcentres in Southern Denmark, it turns out that one of the jobcentres did not implement early activation at all. That is, for the treatment group in that jobcentre, the treatment was similar to the treatment in Experiment A.

Further analysis of the type of activation to which the unemployed in the treatment groups were assigned reveals that the unemployed are assigned to programmes with the intention to upgrade and clarify their skills (i.e. educational and training programmes). These are typically programmes with a duration of around 4 weeks. This category of programmes is the most commonly used activation instrument in Denmark (see e.g. Danish Economic Council, 2007).

In all regions we observe an increase over time in the activation intensity for those who remain unemployed in the control groups. This follows naturally from the large focus on active labour market policy in the Danish flexicurity model (see e.g. Andersen & Svarer, 2007). After the end of the experimental treatment period (in week 26), the activation intensities for treatment and control groups converge rather quickly. In Experiment B a marginally larger fraction of the remaining unemployed in the treatment group is activated compared to the control group, possibly reflecting outcomes from the meetings with caseworkers (or dynamic selection out of the group).

All in all, the meetings and activation intensity figures reveal that the treatment groups to a large extent received the intended treatments (with the exception of one jobcentre in Southern Denmark), and they were treated much more intensively than the control groups in the relevant dimensions. As compliance to the treatment protocol is not 100%, the treatment effects that we will determine below can be regarded as "intention to treat effects". We will not report ATE effects as we believe that the ITT effects are really the policy relevant effects in this setting, since it reflects the 'modus operandi'. Furthermore, compliance is not a static concept in our experiments (more on this below).

Lastly, we consider the treatment of the control groups between different regions to analyze whether their treatment in some regions was given lower than normal priority due to the intensified treatment requirement for the treatment groups. This could for instance be due to lack of economic ressources (a substitution effect). Note that, in order to avoid such effects, extra ressources were given to the jobcentres in compensation for the intensified treatment requirement. Using the fact that the timing and content of the treatment varies between regions we analyze the presense of such effects. Therefore we compare the meeting and activation intensities between the different regions over time to determine whether there are systematic differences in the treatment of the control groups in different regions. The figure is reported in the appendix, and although there is local variation, we do not find any systematic differences between the regions.[8]

[8] Possibly with the exception of experiment C that to some extent has a higher activation intensity (especially around 40 weeks). This is very likely due to a higher fraction of young unemployed for whom there are more strict rules regarding activation participation.

3.4 Non-compliance

The analysis above has established that, although the treatment intensity is larger in the treatment group than in the control group, there are individuals in the treatment group who do not recieve the intended treatment (or at least not at the intended timing). The identification of compliers and non-compliers to the treatment protocol is not straightforward in our setting where treatment is essentially a whole sequence of, for instance, meetings; a non-complier one week may be a complier in the subsequent week and so forth. We also see a clear implementation lag of treatment, such that individuals might recieve treatment after the intended treatment period. Note that non-compliance can either be a result of the lack of action from job centres (no meeting scheduled) or from individuals in the treatment group (e.g. due to sickness), and that there might be reasonable arguments for the job centres not to give the intended treatment, if for instance the individual has informed the job centre that he has found a job and begins work in, say, a few weeks.

With these problems in mind it is, nevertheless, still important to analyze the degree of compliance and relate it to the intention-to-treat effects we present in the next section. If there are systematic differences between compliers and non-compliers, ATE effects are not just directly obtainable by scaling the ITT effects using the proportion of compliers (which again is problematic in our dynamic setting, as the degree of compliance differs over time and people can leave to employment at any point in time).

To analyze the degree of compliance, we construct a variable (*excess treatment*)which adds up the number of meetings/weeks of activation an individual has participated in 5 and 10 weeks after treatment should have started[9] and subtracts the ideal treatment had compliance been 100% (and taking into account that individuals should only be treated when unemployed). This deals with the fact that we see some very short employment spells in the data which implies that the amount of intended meetings (activation) vary between individuals.[10]

excess treatment is regressed on a set of covariates (all the variables used in Tables 6-9 in the Appendix), job centre dummies and the local unemployment rate (at the job centre level).

[9] According to the design protocol i.e. week 1 for Exp. A,B,D and week 10 for Exp. C

[10] Our accumulated treatment measure, which we call excess treatment, is thereby calculated as the provided treatment subtracted the ideal treatment had there been 100% compliance with the treatment protocol.

In Table 3, we summarize the mean 'excess treatment', which is negative in Experiments A,B and D implying that in week 5 after inflow individuals in Experiment A (group meetings) have recieved 2 meetings less than intended on average. As this difference hardly grows when we look at the achieved treatment after 10 weeks we subscribe a part of this effect to the implementation lag earlier documented. Table 3 also reports R^2 statistics from our regressions[11]. Generally we have very low explanatory power in our regressions, which implies that the main part of the variation in achieved treatment is due to factors unobserved to us. In terms of significant explanatory variables we mainly see jobcenter differences (significant jobcenter dummies) but some small differences also prevail.

In Experiment A, individuals who have experienced more unemployment during the past 3 years are less likely to participate in the group meetings. In Experiment B, individuals who have earlier been sicklisted, or worked in the construction or manufacturing industry are less likely to participate in the individual meetings. In Experiment C individuals aged 50 or above and unemployed who are member of the UI-fund for academics are activated to a larger extent. Finally, in Experiment D older aged individuals are less likely to participate in meetings. Interestingly, in Experiment C and D there also seems to be an inflow effect, such that individuals enrolled into the experiment in the last half of the inflow period are participating in meetings/activation to a smaller extent.

Table 3: Excess treatment

	Exp. A	Exp. B	Exp. C	Exp. D
Week 5:				
Mean *excess treatment*	-2.01	-0.5	0.43	-2.18
R^2 of estimated model	0.07	0.07	0.09	0.07
Week 10:				
Mean *excess treatment*	-2.35	-0.73	0.30	-2.78
R^2 of estimated model	0.08	0.11	0.11	0.05

Note: The estimated regression model is a linear regression model where explanatory variables are job centre dummies and the local unemployment rate plus the variables reported in tables 6-9. The dependent variable is the difference between provided and intended treatment.

Although there are indications that weaker groups of unemployed (older workers or

[11] Output from regressions are available upon request, we summarize all significant findings from our regressions after 10 weeks below.

previously unemployed) comply to a smaller degree, the findings in Table 3 suggest that non-compliance to treatment is largely random and therefore do not compromise the results presented below.

3.5 Minimum Detectable effects

Inference from randomized experiments are often challenged by limited sample sizes. Power calculations in our setting shows that we can only hope to detect moderately large effects[12]. Minimum detectable effects (MDEs), i.e. the smallest true treatment effect given a specified level of statistical power, significance and given a specific statistical test, are determined by the sample size and the standard level of variation in a sample, where the later is high due to a very dynamic labour market. The Danish labor market is characterized by a very high turnover which implies quite large variation employment status over time. For example, the annual gross outflow (and inflow) rate from Danish firms is about 27%, compared to 14-20% in most other European countries. Hence, the standard deviation of accumulated employment over almost five years is likely to be large.

In the present case, with total sample sizes of 500-875 after splitting by gender, using a significance level of 90% (95%) and a power of 80%, the MDEs are in the range of 0.17-0.22 standard deviations (0.19-0.25). Explaining, say, 25% of the variance by covariates would bring the MDEs down to 0.15-0.19 (0.17-0.22). Hence, to decrease the minimum detectable effect size we have tried various specifications, where we include covariates as conditioning variables in order to reduce the residual variation in the sample. Not surprisingly we found that conditioning on past individual history helps decrease the residual variation in our sample to some extent (for more on this, see e.g. Bloom, 2006). The overall patterns of effects are not changed by inclusion of the history variables, which is also to be expected, as the variation accounted for by labour market history is independent of treatment status, due to the randomization design.

4 Main results

In this section we present the effects from each of the four experiments. The main outcome is the accumulated number of weeks employed from the start of the experiment

[12] We use the program Optimal Design to determine mimimum detectable effects. Results are available on request.

until week t, and then we let t vary from 1 to 237 weeks.

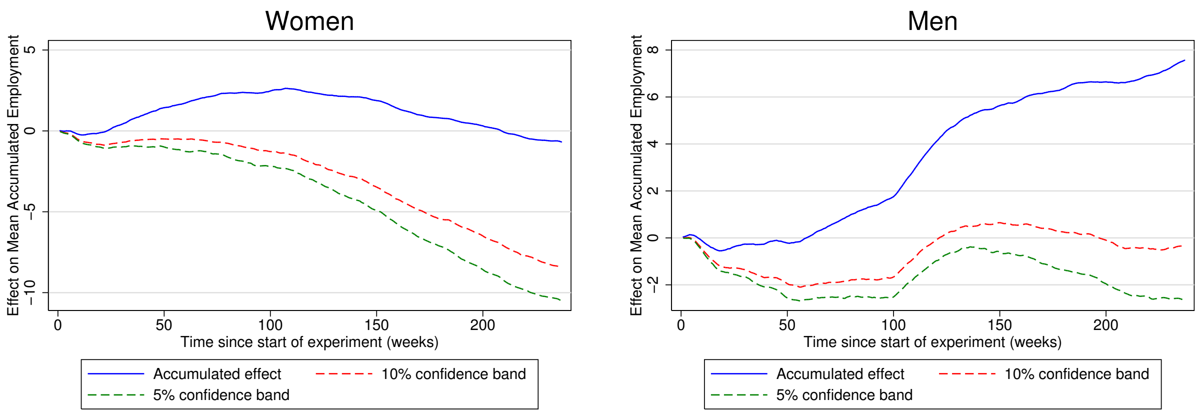
The effect of treatment in a given week (β) is estimated in the following regression:

$$W_{it} = \alpha + \beta_t T_{it} + \gamma H_{it} + \varepsilon_{it}$$

where W_{it} is the accumulated number of weeks in employment t weeks after enrollment into the experiment, T_{it} denotes treatment status and H_{it} is a measure of previous employment history.[13] The treatment effect at time t, β_t, measures the average number of extra weeks spent employed for the treatment group compared to the control group from the beginning of the experiment until t weeks later. We also report the relevant bound of the confidence interval of the effects both at a 5% and 10% level corresponding to a one-sided test, where the hypothesis is that the effect is above 0. Our hypothesis is that all interventions have positive effects, as they are motivated by, and consists of elements from, QBW1, which showed large positive effects. The confidence intervals are obtained by bootstrapping, where each individual in a given bootstrap sample is followed for all 237 weeks.[14].

4.1 Experiment A: Group meetings

Figure 4: The employment effect of Experiment A (group meetings)



Note: The figure shows the accumulated difference in the employment rate between the treatment and control groups. The one-sided confidence bands are obtained by bootstrapping.

[13] H_{it} measures the number of weeks the individual was employed in the timespan from -10 to $-10-t$ weeks before enrolment into the experiment.

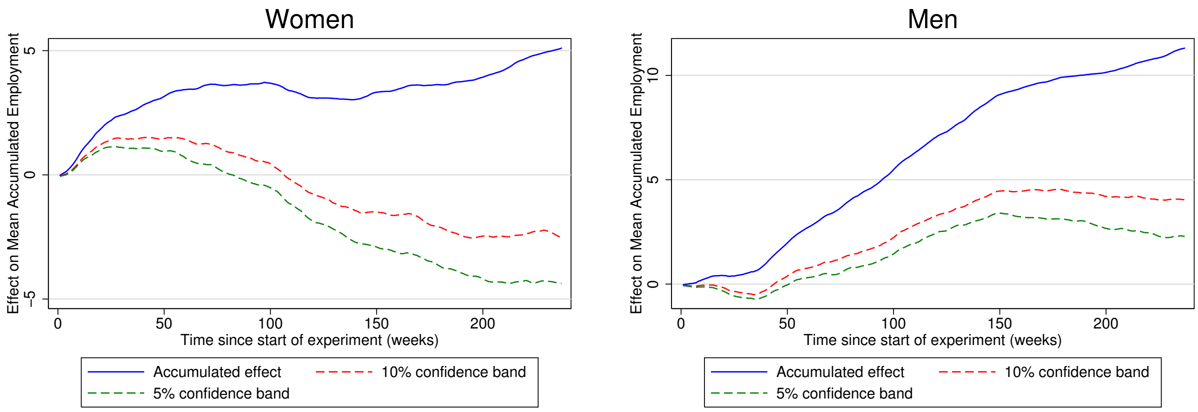
[14] Adding additional explanatory variables in the regressions does not change neither the results nor the confidence bounds noticeably.

Figure 4 shows the employment effects of Experiment A, the group meetings. For women, there overall effect is not signifcantly different from zero. There are indications of positive employment effects in the first part of the period, but these wear out, and after 2 years the difference in accumulated employment between the treatment and control gruop is 0. For men employment differences accumulate quite dramatically after 2 years, such that after 4 years, men in the treatment group have accumulated 7 weeks employment more than those in the control group. The wide confidence bands shows that although the effect is large, the robustness is not. The effect for men is only marginal significant.

The fact that the effect accumulate at later periods suggests that the primary channel through which group meetings affect employment is via longer job duration rather than shorter unemployment duration, and this result appears to hold much more strongly for men than for women.

4.2 Experiment B: Individual meetings

FIGURE 5: THE EMPLOYMENT EFFECT OF EXPERIMENT B (INDIVIDUAL MEETINGS)



Note: The figure shows the accumulated difference in the employment rate between the treatment and control groups. The one-sided confidence bands are obtained by bootstrapping.

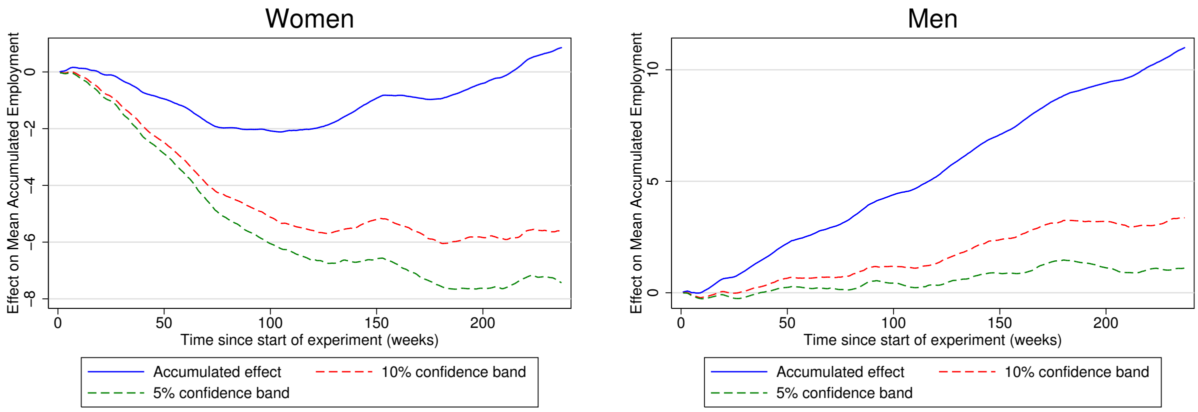
Figure 5 shows the effects of the Experiment B, fortnightly individual meetings. Both women and men benefit greatly from participating in individual meetings. Two years after entry into unemployment, the treatment group has accumulated an overall average of around 4-6 weeks more of employment than the control group. Considering that the total employment rate of the control group over the two-year period is slightly below 50%, this corresponds to more than a 10% increase in the employment rate over the two-year period after entry into the programme. This is a very large effect and it is also statistically significant at the 5% level for men and also for women (at least within the first 18 months). For women, the effect tends to stabilize after two years, while for men, it continues to accumulate such that, after almost five years, men in the treatment group have accumulated around 11 weeks more employment than men in the control group.

As in Experiment A, observe that men respond much later than women to the treatment, suggesting that women find jobs faster, while men keep their jobs longer (this is analyzed in section 5). In the appendix, we plot the difference in treatment effects between men and women for the initial 50 and 250 weeks. The impact on accumulated number of weeks of employment for women is significantly different from their male counterparts already from the first few weeks of the experiment and continues to be so for the first 50 weeks. In the longer run men accumulate more weeks than women. This further illustrates that there are subtle differences in terms of dynamic behaviour across gender.

The results from this experiment confirm the patterns found in the literature, that there are positive effects of intensified counseling for the unemployed. This holds even in the Danish case of already fairly intensive counseling. Compared to the effects of the group meetings in Experiment A, the effect is much larger when individual meetings are used. Naturally, the cost of having individual meetings is larger. In the cost-benefit analysis later in this paper, we show that the extra costs of having individual meetings are strongly dominated by the positive effects on accumulated employment.

4.3 Experiment C: Early activation

FIGURE 6: THE EMPLOYMENT EFFECT OF EXPERIMENT C (EARLY ACTIVATION)



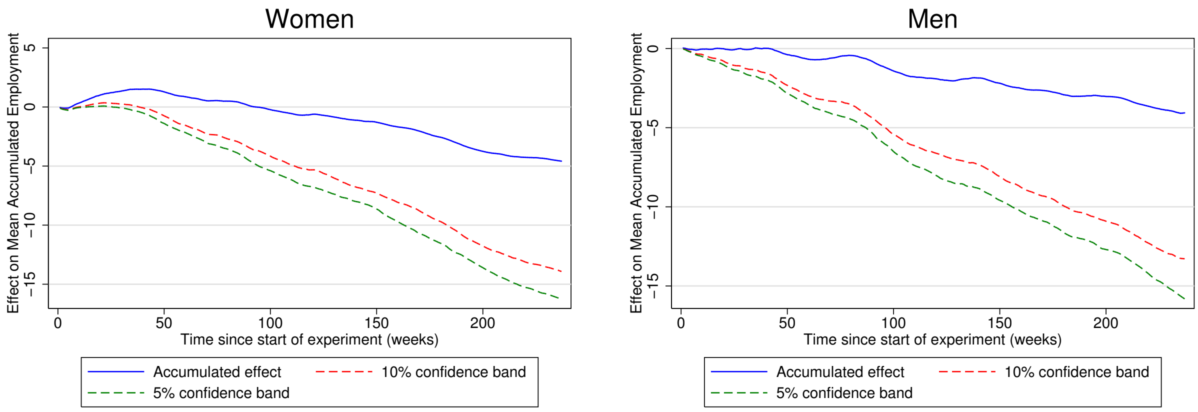
Note: The figure shows the accumulated difference in the employment rate between the treatment and control groups. The one-sided confidence bands are obtained by bootstrapping.

Figure 6 shows the effect of early activation (after 13 weeks of unemployment). The difference between women and men is remarkable. In the appendix we provide the gender difference for each t with confidence bands (again for 50 and 250 weeks), and, as in experiment B, it is clear that there are statistically significant differences across gender, and furthermore they persist over time. From Figure 6 it appears that women do not react at all to the anticipated activation, and if anything their accumulated employment is reduced (albeit not significantly), presumably due to locking-in effects. For men, the effect starts accumulating already after 9 weeks of unemployment, suggesting that at least a part of the observed effect is an *ex ante* effect, when compared to Figure 3, which shows that the activation intensity does not start to increase before weeks 13 to 17. For women, the small negative impact dies out over time, while for men, the positive effect continues to accumulate to 11 weeks after almost five years.

Given that the effect for men may be a threat effect, we would have expected to observe higher transition rates back into unemployment, such that an initial positive effect would tend to decline or at least not increase further over time. As shown, we find the opposite, suggesting that threat effects not only scare male workers out of unemployment but keep them from returning - and the effect is statistically significant. The finding that men react to the threat of activation, but women do not, is consistent with the results by Rosholm & Svarer (2008), Geerdsen & Holm (2007), and also Black *et al.* (2003). Comparing these results to the finding from QBW1, it seems that the positive effect for women may be driven by the individual meetings, whereas for men we see both an effect from the threat of mandatory programme participation (a threat effect) and an effect generated by meetings.

4.4 Experiment D: Group meetings and early activation

Figure 7: The employment effect of Experiment D (early activation + group meetings)



Note: The figure shows the accumulated difference in the employment rate between the treatment and control groups. The one-sided confidence bands are obtained by bootstrapping.

Figure 7 shows the effect of combining group meetings with early activation. We observe a positive and significant effect for women initially, but compared to the case in Experiment A it appears that when we combine meetings with early activation, the effect stops accumulating after less than a year. After two years there is no difference in accumulated employment between the treatment and the control groups. For men the effect is close to zero the first year, whereafter the difference between employment in the two groups favours the control group, although not significantly so. This finding illustrates the importance of evaluating ALMPs over time as conclusions might change due to the dynamics of subsequent employment and unemployment spells.[15]

It is interesting that, whereas there was a positive effect for men of early activation in isolation, this is no longer the case when combined with group meetings. We do not have a good explanation for this deviation. We have tried to exclude the job centre that did not comply with the treatment protocol[16], but this did not change main results.

As a robustness check of our findings above, we have also performed the same analysis where we construct one control group from the 4 regions and compare this synthetic control group to the different treatment groups. The central findings still remain, there

[15] Card *et al.* (2010) show in their survey that programme evaluations with a longer time horizon are more likely to find positive impacts.

[16] See working paper version of the paper for the results (IZA discussion paper 6970).

are effects from treatment in the experiments involving individuals meetings (B) and early activation (C).

4.5 Heterogeneous treatment effects

The analysis presented in the last section creates a strong case for the effect of meetings for both women and men. The results also reveal intriguing differences between men and women in terms of behavioural responses to the different treatments. To further analyze whether treatment effects vary along different dimensions we have investigated the interaction of treatment with age, business cycle conditions and other explanatory variables (the variables used in tables 6-9). This part is based on a simple linear regression of accumulated weeks of employment in week 100 and 230 on treatment status, age group or business cycle indicator, and their interactions.[17] The results are robust to other model specifications that account for probability mass in zero weeks of accumulated employment (e.g. a tobit model).

Related to age, the only statistically significant finding is that it is especially young men who react to (the threat of) early activation. The result shows that young men have accumulated 22 weeks more in employment than their counterparts in the control group after 230 weeks.

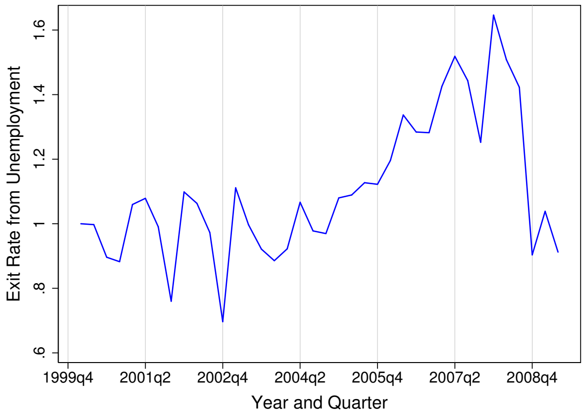
Next, we exploit the fact that the inflow period of QBW2 gives an opportunity to relate the effectiveness of the differential treatments to the business cycle. Figure 9 depicts the normalized (first quarter of 2000=1) outflow rate from unemployment to employment of individuals entering unemployment from 2000 until 2009 conditional on a wide range of explanatory variables.[18] The figure illustrates the large impact of the financial crisis in Denmark. It illustrates that the crisis led to a collapse of outflow rates from unemployment from the beginning of the 3rd quarter of 2008 and onwards. Performing a similar estimation by region, we have found that this time pattern is present in all regions. This implies that individuals becoming unemployed in the last part of the inflow period (week

[17] Other interactions were not found to yield interesting insights. To save space we do not include the results in the paper, but they are of course avaliable upon request.

[18] This rate is determined by estimating a piecewise constant unemployment duration model on the inflow to unemployment during the period 2000-first half of 2009, including a wide range of explanatory variables, including labour market history and demographics, and specifically time-varying quarterly dummies, which are shown in the graph and captures any variation over time in the outflow rate to employment that is not accounted for by observed variables.

16-29) of the experiment will potentially experience worse labour market conditions, as they become unemployed very close to this dramatic decline in outflow rates.[19]

FIGURE 9: THE NORMALIZED EXIT RATE FROM UNEMPLOYMENT



Source: Own calculations based on an estimated duration model

In it only in Experiment C (early activation) we find a business cycle dependent effect. Here, men who become unemployed in the first part of the experiment, when labour market conditions were good, accumulate 12 weeks more in employment than those becoming unemployed closer to the time when the Danish economy was hit by the financial crisis. After 230 weeks the effect is still of the same size but no longer significant, suggesting that the treatment group members enrolled at later points in time eventually reach the same employment rates as those in the treatment group who enter earlier. Overall the inflow effect suggests that the threat effect of having to attend early activation programmes is more prominent during favourable economic conditions. It is presumably easier to react to a threat of activation when there are plenty of jobs to choose from.

5 The dynamics of treatment effects

Next, we focus mainly on the short term effects of the programmes in order to provide explanations for the mechanism behind the reported treatment effects above. We provide

[19] As earlier mentioned the inflow into control and treatment groups is stable, so we can in fact treat the treatment and control groups in "good" or "bad" times as a separate experiment. It is important to keep in mind, that we assume that the intensity and efficiency of treatment is constant wrt. week of inflow below.

insight into the dynamics of the treatment effects by extending the analysis to a multi-state duration framework. We analyze the effects of treatment on unemployment duration and subsequent employment duration. We include all spells of employment and unemployment experienced during the two-year period (approx. 100 weeks) following the start of the experiment.[20] Initially we present the estimation framework and then we proceed with the results.

5.1 Evaluation method

For each experiment, we have two random samples of the inflow into unemployment implying that the distribution of any unobserved variables is independent of treatment status at the time of inflow. However, already one week into the unemployment period, those in the treatment group will become aware of the experiment and this might alter their future behaviour and thereby violate the assumption of identical distributions in unemployment. This leads to identification issues, because of the so-called dynamic selection problem. Specifically, the random assignment only ensures comparability of the treatment and control groups at the start of the unemployment spell. At later times treated units with characteristics that have a positive/negative interaction effect with treatment on the transition probability leave the initial state first/last, so that these characteristics are under/over represented among the treated relative to the controls. This selective outflow from unemployment confounds any simple comparison using observed hazard rates for the treated and controls. Therefore, we cannot just compare transition rates between the treatment and control group as these will be biased estimates of the treatment effects if this selection process on unobserved variables is not accounted for. The transition rates will capture both a treatment effect and a selection effect (for more on this issue see Abbring & van den Berg, 2005).

We can illustrate this point in a single-spell single-state model. Denote the observed hazard rate at time t as $\theta(t|X, D)$ where X is the observed heterogeneity of individuals and D is an indicator for being assigned to the treatment group. Imagine that the information letter is sent at time 1 and that U represents an unobserved variable (say, motivation or ability). Due to randomization into treatment, U will be independent of treatment status at the time of inflow, implying that $\theta(t|X, D = 1) = \theta(t|X, D = 0)$ for $t = 0, 1$. However,

[20] Treatment group assignment has no severe effects on transition rates into other labour market states such as self-sufficiency or other public income transfers. If anything, time spent in such states is reduced. These results are not reported but are available upon request.

for $t \geq 2$ this will not necessarily be the case, as the received information is likely to change the behaviour of those in the treatment group from their "normal behaviour" (the control group). The observed hazard rate for $t \geq 2$ is equal to

$$\theta(t|X,D) = E_U[\theta(t|X,D,U)|T \geq t],$$

which depends on the distribution of the unobserved variable conditional on survival until t. If treatment affects behaviour, the distribution of this unobserved variable is likely to differ between the control and treatment groups for $t \geq 2$. Hence, $\theta(t|X,D)$ is likely to differ both due to a direct treatment effect and due to a composition effect for $t \geq 2$.

Duration models allow us to account for this selection bias by explicitly modelling the selection process out of the state of interest.The cost of this is the imposition of distributional assumptions. Abstracting away from our single-state model, we also need to deal with spurious correlations arising from non-random selection into employment, when we extend the problem above to post-unemployment outcomes (employment duration). To do so, we allow for transition-specific unobserved terms to be correlated across states. Note that the random effects assumption, needed in the mixed proportional hazard (MPH) model presented below, according to which treatment and unobserved explanatory variables are independent in the inflow to unemployment, is satisfied by construction due to the random assignment to treatment.

We use a non-parametric specification for the unobserved heterogeneity distribution, and we do not impose *a priori* a fixed number of mass points in the distribution of the unobserved components. Instead we rely on the Akaike Information Criterion to decide the number of mass points. The baseline hazard is piecewise constant. We control for various explanatory variables and estimate the models separately for men and women, as the above analysis has shown very different behavioural patterns over time. The method of estimation is NPMLE (see e.g. Heckman & Singer, 1983), and we treat individuals moving to other states than employment and unemployment as censored observations. The two hazard rates, from unemployment to employment and from employment to unemployment, are assumed to have a MPH form:

$$\theta_j(t \mid X_j, U_j, D) = \psi_j(t) \exp(X_j' \beta_j) \exp(\delta_j(\tau) D) \exp(U_j) \text{ for } j = ue, eu$$

where $\psi_j(t)$ is the baseline hazard for the transition j. Treatment causes a shift upward or downward in the hazard rates. We allow for time-varying treatment effects; $\delta_j(\tau)$, where τ denotes time since entry into the experiment (for unemployment, $\tau = t$, while for employment spells, τ is equal to the duration of unemployment plus the elapsed employment duration. The time-variation is chosen to capture the change in treatment intensity around week 16 (see the comments above on implementation lags). Formally, this means that we take $\delta_j(t \mid X_j) = \delta_j^1 1(\tau \leq 16) + \delta_j^2 1(\tau > 16)$.

5.2 Estimation results, dynamic treatment effects

Table 3 reports the results from the estimation of the model specified above. Explanatory variables, similar to those in section 4, are included in the estimations but not reported in the table to save space.

Table 3: Estimates from the duration model

	Men		Women	
	Coeff	Std.err	Coeff	Std.err
Experiment A (group meetings)				
$\delta_{ue}(1-16)$	-0.066	0.127	-0.004	0.136
$\delta_{ue}(17+)$	-0.016	0.117	0.080	0.122
$\delta_{eu}(1-16)$	-0.073	0.380	0.529	0.490
$\delta_{eu}(17+)$	**-0.318**	0.130	0.029	0.232
Experiment B (individual meetings)				
$\delta_{ue}(1-16)$	0.017	0.108	*0.192*	0.116
$\delta_{ue}(17+)$	0.050	0.104	0.090	0.129
$\delta_{eu}(1-16)$	-0.082	0.404	-0.424	0.494
$\delta_{eu}(17+)$	**-0.283**	0.136	-0.044	0.177
Experiment C (early activation)				
$\delta_{ue}(1-16)$	0.143	0.103	0.036	0.109
$\delta_{ue}(17+)$	-0.039	0.095	**-0.224**	0.112
$\delta_{eu}(1-16)$	-0.084	0.409	0.047	0.407
$\delta_{eu}(17+)$	-0.140	0.134	0.000	0.163
Experiment D (meetings + activation)				
$\delta_{ue}(1-16)$	-0.029	0.140	*0.217*	0.125
$\delta_{ue}(17+)$	-0.029	0.139	-0.040	0.139
$\delta_{eu}(1-16)$	-0.435	0.403	-0.600	0.455
$\delta_{eu}(17+)$	-0.111	0.151	0.129	0.182

Note: bold (italic) figures indicate significant at the 5% (10%) level.

The treatment effects of Experiment A (group meetings) show no effect on job finding for men, and only a small insignificant effect for women after 16 weeks of unemployment. This is consistent with the lack of an early impact, especially for men, in Figure 4. For men who find employment, the employment separation rate is significantly lower in the treatment group, while for treated women, exit rates from employment back into unemployment are increased, albeit insignificantly.[21]

[21] Note that very few find a job and leave it again before week 16 after entry into the experiment. This explains the large standard errors on the treatment effects on the hazard out of employment before week 16.

The findings thus explain quite well the observed pattern of accumulated effects presented in Figure 4. For women, there is a slight increase in job finding rates after some time, which might suggest a counselling or network effect arising from more efficient job search or access to new search channels. For men, the effect arises entirely due to longer lasting employment spells. One interpretation of this result is that meetings/counseling improve the match between employers and employees, and if this is the case, the effects are likely to grow even further over time. The impact relative to the baseline hazard is a 27% decrease in the transition rate back into unemployment ($\exp(-0,318) - 1$).

Similar findings for men follow from Experiment B (individual meetings); however, in contrast to Experiment A there is a small positive impact on job finding (not significant) and once again a significant negative effect on the transition rate from employment back into unemployment for individuals in the treatment group. The small positive effect on the transition rate out of unemployment is important, as the combined effect leads to a much larger impact on accumulated employment than group meetings, cf. Figure 5. For women there is an immediate effect of meetings on the transition rate into employment (significant at the 10% level), whereas this was not the case with the group meetings. The effect on the transition rate from employment into unemployment is negative - albeit smaller and not significant compared to the one for men.

The estimates suggest that the accumulated 5 week employment gain for men observed after 2 years in Figure 5 comes mainly from more stable employment, whereas for women the 3.5 week effect comes mainly from faster job finding. In the figures above, this difference is seen as the accumulated gain from treatment for men set in at a much later stage than for women.

For Experiment C (early activation) we see a much higher effect for men from treatment group assignment in the first 16 weeks when compared to the first two experiments. This indicate *ex ante* effects in the form of threat/motivation effects from the anticipated future enrollment into an activation programme. We also observe a negative effect on the transition rate out of subsequent employment, such that the *ex ante* effects do not imply less stable job relations as one might have expected. However, none of these effects are statistically significant, in spite of the large (and significant) accumulated effects found in Figure 6. We explain this by the fact that both of the mentioned effects work in the same direction. In section 5.2, this presumed *ex ante* effect was shown to arise mainly during the more favourable cyclical conditions. If we introduce an interaction between our treatment group indicator and an indicator for becoming unemployed during good and bad economic conditions, the results show a significant positive effect during the good cyclical conditions (coefficient 0.1773, std.err. 0.0896) and a small, insignificant, negative effect during bad conditions.

Women respond remarkably different than men to early activation - there is no sign of *ex ante* effects, and instead we see a large significantly negative effect, on the transition rate out of unemployment from week 17 and onwards. This is presumably a standard locking-in effect from participation in activation programmes. For women, there is no effect on the transition rate from employment back into unemployment, which leads to an overall negative (insignificant) impact on accumulated employment after 2 years in Figure 6.

The results from Experiment D (early activation + group meetings) shows a slight negative effect on job finding for men, and that they tend to have a smaller exit rate from jobs. Overall the results from the duration model does not support the findings in Figure 7 that showed a negative effect for men. The missing link is that the transition rate to other types of public income transfers was higher for the treatment group, and this seems to drive the lower employment rate. For women we see a positive effect on the transition rate out of unemployment in the first 16 weeks, which is similar to what we had in the case of individual meetings. In relation to Figure 7, the initial positive effect on transitions into employment is, however, counteracted by a higher transition rate back into unemployment for employed individuals in the treatment group. Notice that comparing transition rates between Experiments C and D, there was a positive effect for men of early activation in isolation, which is no longer found when combined with group meetings.

Finally, we compare the above findings with those reported in relation to the QBW1 experiment.[22] The studies find that on average individuals in the treatment group leave unemployment around 20% faster than individuals in the control group. This is an effect of about the same size as what we have reported for women exposed to meetings and for men exposed to the threat of early activation. For men, we find an additional favourable effect in Experiments A-C on their subsequent employment spell. This is also the finding in Blasco & Rosholm (2011). They include subsequent employment spells and find that overall the experiment reduces unemployment reoccurrence for men, but not for women. Hence, the results found here are in accordance to what was found in QBW1 and may hence help to explain the findings there is a combined effect of frequent individual meetings

[22]See eg. Rosholm (2008), Graversen & van Ours (2008a & 2008b) and Blasco & Rosholm (2011)

and the threat of future activation. It also underlines the importance of allowing for effects in subsequent employment spells in evaluating ALMPs.

We are not able to explain why women find jobs faster and men tend to keep them longer when exposed to meetings. Nor why men react to perceived future activation by searching more and finding better jobs, while women do not. Some of it may be due to different search focus - women tend to find employment in the public sector and men in the private sector. Furthermore, caseworkers are mostly female and often have experiences from other jobs in the public sector, which may enable them to help women better. Public jobs typically last for a long time, which might explain why there is no impact on exit from jobs for women. It might also just be the case that predominantly female caseworkers are better able to help female job searchers, as suggested by Behncke *et al.* (2010b). Finally, psychological and behavioural reactions might differ between men and women; for example, women may listen more to the advice of the caseworkers, while men are more self-confident and perhaps therefore less inclined to take advice.[23] However, this is all speculation, and data on behavioural aspects would be necessary to shed light on this issue. In fact, the National Labour Market Authority of Denmark has recently decided to supplement all new experiments with surveys before and after on behavioural issues, specifically in order to gain more insight into the behavioural nature of such effects.

6 Cost-benefit analysis

In this section we contrast the costs of running each of the four experiments with the gains obtained by increasing employment rates. In addition, we adjust for the marginal costs of providing public funding via taxation. The CBA calculates the net gains accumulating over the first two years after entry into the experiment. We have chosen this time horizon in order to focus on short term effects and not rely to heavily on time effects several years after treatment (also we avoid taking discounting into account).

The costs are split into costs of income transfers and costs of operating the active labour market policy (called programme costs). The costs of income transfers are calculated based on weekly per individual costs of a given income transfer. Programme costs are provided as average costs of operating activation programmes of a given type (cost data are provided by the National Labour Market Authority), individual meetings last between 15 and 30 minutes (information on average meeting duration is provided by the

[23] This explanation was suggested to us when we presented the results to an audience of case workers.

participating jobcentres), and group meetings last 2-3 hours and have 6-30 participants per meeting). The price of a meeting per worker is then calculated by multiplying its duration with the hourly costs of a caseworker and dividing by the number of participants.

Public income transfers represent only a reallocation of income, hence, in the cost-benefit calculation, we only include the marginal costs of providing public funds via taxation, assumed to be 20% (this is the official rate recommended for cost-benefit calculations by the Danish Ministry of Finance). Costs of operating the active labour market policy, however, are a genuine extra cost and as such is multiplied by 1.2 in order to accommodate the marginal costs of public funds (MCPF). On the benefits side, we assume that employed workers are able to obtain work at approximately the average weekly wage rate for previously unemployed workers (approximately EUR 40,200 per year divided by 46 working weeks). We assume further that the wage is equal to the marginal cost of production, such that all the gain from increased production accrue to the workers.[24] We assume that there will be no future effects of the programme beyond the two years time horizon. Any additional accumulation of employment gains will therefore tend to improve on the result. Finally, we ignore any general equilibrium effects that might be present (see e.g. Crepon *et al.*, 2011; Gautier *et al.*, 2012). The results of the cost-benefit analysis is shown in Table 4.

[24]Note that we do not perform separate calculations for males and females in order to save space.

Table 4: Cost-benefit analysis (Per unemployed worker)

	Costs	Corrected for MCPF
Northern Jutland - Experiment A		
Saved income transfers	1486	297
Saved programme costs	-237	-284
Saved total costs	1249	13
Accumulated gain in employment (weeks)		1.52
Value of increased production		1329
Net result of CBA (in €)		**1342**
Copenhagen & Sealand - Experiment B		
Saved income transfers	1569	314
Saved programme costs	41	49
Saved total costs	1610	363
Accumulated gain in employment (weeks)		4.99
Value of increased production		4362
Net result of CBA (in €)		**4725**
Mid-Jutland - Experiment C		
Saved income transfers	412	82
Saved programme costs	-295	-354
Saved total costs	117	-272
Accumulated gain in employment (weeks)		1.75
Value of increased production		1530
Net result of CBA (in €)		**1258**
Southern Denmark - Experiment D		
Saved income transfers	108	22
Saved programme costs	-366	-440
Saved total costs	-258	-418
Accumulated gain in employment (weeks)		-1.37
Value of increased production		-1198
Net result of CBA (in €)		**-1616**

Table 4 shows that individual meetings with caseworkers are not only the most effective instrument in terms of increasing employment, they also lead to the largest net gains to society. The net gain per new unemployment spell is around EUR 4725, and even the isolated cost calculation for the public sector shows net savings (the 2^{nd} column of Table 4). Group meetings - although their impact was more modest - also give a surplus in the cost-benefit analysis, since the costs of running these meetings are fairly low. The same is true for early activation, where the positive effects found for men were sufficient to outweigh the slightly negative effects for women and the costs of running the programmes. For the (imperfectly conducted) experiment with group meetings as well as early activation, the cost-benefit analysis reveals a deficit of about EUR 1600 per unemployment spell. As mentioned in the literature review, these results are likely to change in the presence of general equilibrium effects from the programmes. General equilibrium effects could potentially improve as well as worsen the cost-benefit calculation, although Gautier *et al.* (2012) indicate that overall gain becomes smaller but does not disappear, when general equilibrium considerations are included, at least for the experimental design in QBW1. On the other hand, effects lasting beyond the two-year observation period will increase the gains of treatment and thereby increase the profitability of the programmes.

7 Conclusion

We have analyzed the effects of four randomized experiments conducted in Denmark in 2008. The experiments entailed different combinations of early and intensive treatment in terms of meetings and activation. A previous experiment showed that the combination of meetings, job search courses, and early activation reduced the length of unemployment spells and was economically attractive (see Graversen & van Ours, 2008a & 2008b, and Rosholm, 2008). The purpose of the set of experiments analyzed in the present study was to test which of the several instruments used in QBW1 caused these large effects and whether similar results could be achieved using cheaper instruments such as group meetings.

The evidence we present is quite compelling; fortnightly individual meetings between newly unemployed workers and caseworkers can increase employment rates over the next two years by 10%, corresponding to 5 weeks, and our cost-benefit analysis shows that the surplus per new unemployment spell is around EUR 4725. We find it remarkable that having to attend 6-7 meetings during the first 13 weeks of the unemployment spell can have such a large effect on subsequent employment rates. Nevertheless, the positive effect of individual meetings for newly unemployed workers is highly consistent with the results found in the literature on the effects of meetings between caseworkers and unemployed workers. The policy advice is obvious; increasing the frequency of meetings with caseworkers is strongly recommendable.

The accumulated effect of meetings is large for both men and women, but it starts materializing much earlier for women than for men. A multi-state duration analysis suggests that the explanation is that women find jobs faster, while men keep them longer, as a result of these meetings. A couple of potential explanations for these differences by gender are that

- most caseworkers are female, and unemployed women may receive better job search assistance from them than men (cf. Behncke *et al.*, 2010b),
- women and men work in different labour markets, with men being more likely to work in the private sector and women in the public sector. Again, female caseworkers may have better knowledge of public-sector vacancies than they have of vacancies in the private sector.[25]

Unfortunately we have no information at present allowing us to test these hypotheses.

In the experiment involving group meetings we see a similar gender pattern but the effects are more modest, leading to about 2 extra weeks of employment over a two-year period. We therefore conclude that one cannot achieve the same remarkable employment results using group meetings instead of individual meetings.

The positive and economically attractive results found in relation to meetings have also implied that a new wave of experiments will start in 2013. They will explore different aspects w.r.t. to meetings and try to explore the quality-quantity trade-off using a cross-cutting design to investigate the effects of increasing meeting intensity versus the quality of the meetings, or both.

Early activation also shows positive effects for men, especially young men, and especially during the more favourable cyclical conditions, while for women there was actually a negative effect of early activation due to lock-in effects. The effect comes from a threat effect (*ex ante* effect) and more stable subsequent employment. This evidence on threat effects of early activation for men but not for women corresponds well to results found by Rosholm & Svarer (2008), who found such threat effects for men but not for women. A couple of potential explanations for these differences are that

[25] An additional explanation suggested by a group of female caseworkers when these results were presented for them was that men tend to be more confident/proud/stubborn, and that advice therefore takes longer to 'sink in'.

- unemployed men work in the untaxed sector, and therefore when facing mandatory activation, they prefer ordinary employment

- men dislike activation for other reasons, while women value the social network provided.

Again, we have no additional information allowing us to test these different hypotheses, but the behavioural gender differences documented here certainly warrant further future research on these experiments and on the differential impact of active labour market policies on men and women.

Finally, it should be mentioned that the present study does not in any way take into account general equilibrium effects or substitution effects arising from the experiment. Gautier *et al.* (2012) and Crepon *et al.* (2012) offer some insights, but overall we still know very little about the presence of general equilibrium effects and their size with respect to the actual policy implemented. It might very well be the case that meetings lead to more positive effects if they improve the match between workers and firms and this way improve the profitability of a vacancy. It could also be the case that the general equilibrium effects differ between whether the stated intention of the meetings is counseling or monitoring - likely favouring the former. This is also left for future study.

We believe that the results obtained in this paper shed some light on the reason for the Danish success in having obtained a low structural unemployment rate prior to the recent global economic crisis. Before the crisis the structural unemployment rate was estimated to be around 3.5% as compared to 9-9.5% in 1993. Since then, active labour market policies have been introduced and continuously tightened during the 1990s and early 2000s. Especially, meeting intensities have increased, early activation has been introduced (mandatory activation was pushed forward from 4 years to 9 months of unemployment during this period), and noncompliance with the rules has led to sanctions. Moreover, we believe that the results point to possible improvements of the policy conducted, with even more focus on early individual meetings, which are much cheaper than full-time programme participation. The fact that the threat effect of early programme participation disappears during a cyclical downturn may warrant further analysis of optimal cyclical labour market policies.

References

[1] Abbring, J. & G. van den Berg, 2005. "Social Experiments and Instrumental Variables with Duration Outcomes", *IFS Working Paper*, WP05/19.

[2] Abbring, J., G. van den Berg & J. van Ours, 2005. "The Effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment", *Economic Journal*, 115, 602–630.

[3] Andersen, T.M & M. Svarer, 2007. "Flexicurity – Labour Market Performance in Denmark", *CESifo Economic Studies*, 2007, 53 (3), 389–429.

[4] Arni, P., R. Lalive & J. van Ours, 2009. "How Effective are Unemployment Benefit Sanctions? Looking beyond Unemployment Exit", IZA Discussion Paper 4509, Forthcoming in *Journal of Applied Econometrics.*

[5] Ashenfelter, O., D. Ashmore & O. Dechênes, 2005. "Do unemployment insurance recipients actively seek work? Evidence from randomized trials in four U.S. States", *Journal of Econometrics*, 125, 53-75.

[6] Behncke, S., M. Frölich & M. Lechner, 2008. "Public Employment Services and Employers: How Important are Networks with Firms?", *Zeitschrift für Betriebswirtschaft (Journal of Business)*, 151-178.

[7] Behncke, S., M. Frölich & M. Lechner, 2010a. "Unemployed and Their Caseworkers: Should They be Friends or Foes?," *Journal of The Royal Statistical Society Series A*, 173(1), 67-92.

[8] Behncke, S., M. Frölich & M. Lechner, 2010b. "A Caseworker Like Me - Does the Similarity between Unemployed and Caseworker Increase Job Placements?", *Economic Journal,* 120, 1430-1459.

[9] Black, D. A., J. A. Smith, M. C. Berger & B.J. Noel, 2003. "Is the Threat of Reemployment Services more Effective Than the Services Themselves? Evidence from Random Assignment in the UI system. *American Economic Review*, 93, 1313-1327.

[10] Blasco, S. & M. Rosholm, 2011. "The Impact of Active Labour Market Policy on Post-Unemployment Outcomes: Evidence from a Social Experiment in Denmark", *IZA Discussion Paper* No.5631.

[11] Bloom, H., 2006. "The Core Analytics of Randomized Experiments for Social Research", *MDRC Working Papers on Research Methodology.*

[12] 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.

[13] Card, D., J. Kluve & A. Weber, 2010. "Active Labour Market Policy Evaluations: a Meta Analysis", *Economic Journal*, 129, 452-477.

[14] Cockx, B. & M. Dejemeppe, 2007. "Is the Notification of Monitoring a Threat to the Unemployed? A Regression Discontinuity Approach," CESifo Working Paper Series 2042.

[15] Crepon, B., M. Dejemeppe & M. Gurgand, 2005. "Counseling the Unemployed: Does it Lower Unemployment Duration and Recurrence?, *IZA Discussion Paper* No. 1796.

[16] Crepon, B., E. Duflo, M. Gurgand, R. Rathelot & P. Zamora, 2012. "Do Labor Market Policies have Displacement Effect? Evidence from a Clustered Randomized Experiment", Forthcoming Quarterly Journal of Economics.

[17] Danish Business Authority, 2009. "The Danish Regions in an International Perspective" (in Danish).

[18] Danish Economic Council, 2007. "The Danish Economy", Spring Report (www.dors.dk)

[19] Dolton, P. & D. O'Neill, 1996. "Unemployment Duration and the Restart Effect: Some Experimental Evidence", *Economic Journal*, 106, 387–400.

[20] Dolton, P. & D. O'Neill, 2002. "The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Experimental Evidence from the United Kingdom", *Journal of Labor Economics*, 20, 381–403.

[21] Engström, P., P. Hesselius & B. Holmlund, 2009. "Vacancy Referrals, Job Search and the Duration of Unemployment: a Randomized Experiment", IZA Discussion Paper 3991.

[22] European Commission, 2007. "Towards Common Principles of Flexicurity: More and Better Jobs through Flexibility and Security", Directorate-General for Employment,Social Affairs and Equal Opportunities, Brussels.

[23] Fougère, D., J. Pradel & M. Roger, 2009 “Does the Public Employment Service Affect Search Effort and Outcomes?”, *European Economic Review*, 53, 846-869.

[24] Gautier, P., P. Muller, B. van der Klaauw , M. Rosholm & M. Svarer, 2012. "Estimating Equlibrium Effects of Job Search Assistance", draft.

[25] Geerdsen, L. P., 2006. “Is There a Threat Effect of Labour Market Programmes? A study of ALMP in the Danish UI system”, *Economic Journal*, 116, 738–750.

[26] Geerdsen, L. P. & A. Holm, 2007. ”Duration of UI periods and the Perceived Threat Effect from Labour Market Programmes”, *Labour Economics*, 14, 639-652.

[27] Gorter, C. & G. R. J. Kalb, 1996. “Estimating the Effect of Counseling and Monitoring the Unemployed using a Job Search Model”, *Journal of Human Resources*, 31, 590–610.

[28] Graversen, B.K. & J. C. van Ours, 2008a. “How to Help Unemployed Find Jobs Quickly; Experimental Evidence from a Mandatory Activation Program”, *Journal of Public Economics*, 92, 2020-2035.

[29] Graversen, B.K. & J. C. van Ours, 2008b. “Activating Unemployed Workers Works; Experimental Evidence from Denmark”, *Economics Letters*, 100, 308-310.

[30] Hainmueller, J., B. Hofmann, G. Krug & K. Wolf, 2009. ”Do More Placement Officers Lead to Lower Unemployment”, *IAB Discussion Paper* No.13.

[31] Hägglund, P., 2006. “Are There Pre-Programme Effects of Swedish Active Labour Market Policies? Evidence from Three Randomized Experiments”, *Economic Letters.* 2011, vol 112 no. 1 pp. 91-93.

[32] Hägglund, P., 2009. “Experimental Evidence from Intensified Placement Efforts among Unemployed in Sweden”, *IFAU Working Paper* 2009:16.

[33] Heckman, J.J., Lalonde, R.J., Smith, J A., 1999. "The economics and econometrics of active labour market programs", *Handbook of Labor Economics* 1(3), 1865-2097

[34] Heckman, J.J. & B. Singer, 1984. "A method for minimizing the Impact of Distributional Assumptions in Econometric models for Duration Data", *Econometrica,* 52(2), 271-320.

[35] Hofmann, B., G. Krug, F. Sowa, S. Theuer & K. Wolf, 2010. “Kürzere Arbeitslosigkeit Durch Mehr Vermittler”, *IAB Kurzbericht 9/2010.*

[36] Johnson, T. R., & D. H. Klepinger, 1994. “Experimental Eevidence on Unemployment Insurance Work-Search Policies”, *Journal of Human Resources*, 29, 695–717.

[37] Keeley, M. C. & P. K. Robins, 1985. “Government Programs, Job Search Requirements, and the Duration of Unemployment”, *Journal of Labor Economics*, 3, 337-362.

[38] Klepinger, D.H., T.R. Johnson & J.M. Joesch, 2002. “Effects of Unemployment Insurance Work-Search Requirements: The Maryland experiment”, *Industrial and Labor Relations Review*, 56, 3-22.

[39] Kluve, J., 2010. "The Effectiveness of European Active Labour Market Programs", *Labour Economics*, 17, 904-918.

[40] Lalive, R., J. Van Ours & J. Zweimüller, 2005. "The Effect of Benefit Sanctions on the Duration of Unemployment”, *Journal of the European Economic Association*, 3, 1-32.

[41] Larsson, L. (2009), "Evaluation of Swedish Youth Labor Market Programs", Journal of Human Resources, 38, 891-927.

[42] Manning, A., 2009. “You Can’t Always Get What You Want: The Impact of the UK Jobseekers Allowance”, *Labour Economics*, 16, 239-250.

[43] McVicar, D., 2008. “Job Search Monitoring Intensity, Unemployment Exit and Job Entry: Quasi-Experimental Evidence from the UK”, *Labour Economics*, 15, 1451-1468.

[44] Meyer, B., 1995. “Lessons From the US Unemployment Insurance Experiments”, *Journal of Economic Literature*, 33, 91-131.

[45] OECD (2009): OECD Employment Outlook - Tackling the Jobs Crisis. OECD.

[46] Petrongolo, B., 2009. “The Long-Term Effects of Job Search Requirements: Evidence from the UK JSA reform”, *Journal of Public Economics*, 93, 1234-1253.

[47] Rosholm, M., 2008. “Experimental Evidence on the Nature of the Danish Employment Miracle”, *IZA Discussion Paper* No. 3620.

[48] Rosholm, M & L. Skipper, 2009. "Is Labor Market Training a Curse for the Unemployed? Evidence from a Social Experiment", *Journal of Applied Econometrics*, 24, 338-365.

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

[50] Rosholm, M. & M. Svarer, 2009b. “Kvantitativ evaluering af Alle i gang”, Report in Danish: http://www.ams.dk/Reformer-og-indsatser/Udvikling-og-forsog/Alle-i-gang.aspx

[51] Røed, K. & L. Weslie, 2007. “Unemployment Insurance in Welfare States: Soft Constraints and Mild Sanctions”, *IZA Discussion Paper* No. 2877.

[52] Svarer, M., 2011. “The Effect of Sanctions on Exit from Unemployment: Evidence from Denmark”, *Economica*, 78, 751-778.

[53] Van den Berg, G., B. van der Klaauw & J. van Ours, 2004. ”Punitive Sanctions and the Transition Rate from Welfare to Work”, *Journal of Labor Economics*, 22, 211-241.

[54] Van den Berg, G. & B. Van der Klaauw, 2006. “Counseling and Monitoring of Unemployed Workers: Theory and Evidence from a Controlled Social Experiment”, *International Economic Review*, 47, 895–936.

[55] Van den Berg, G., A. Bergemann & M. Caliendo, 2009. "The Effect of Active Labor Market Programs on Not-Yet Treated Unemployed Individuals", *Journal of the European Economic Association*, 2009, 7, 606-616

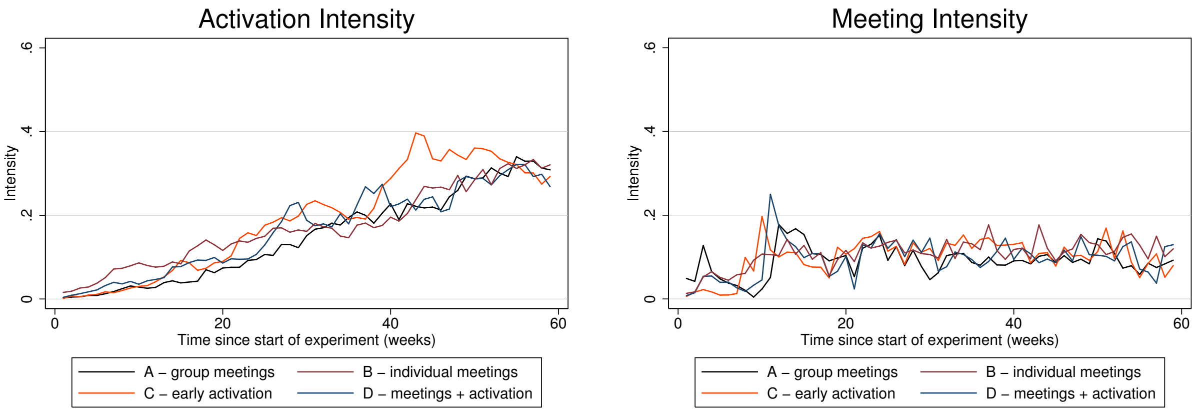
[56] Van den Berg, G. & J. Vikström, 2009. ”Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality”, *IZA Discussion Paper* No. 4325.

[57] Vikström. J., M. Rosholm & M. Svarer, 2011. "The Relative Efficiency of Active Labour Market Policies: Evidence from a Social Experiment and Non-Parametric Methods", *IZA Discussion Paper* No. 5596

Part I

Appendix:

Figure 10: Meeting and activation intensities for the control group



Tables 5-8: Descriptive statistics for the control group and the treatment group

Northern Jutland (A):	**Control group**		**Treatment group**	
	Men	Women	Men	Women
Characteristics	Average	Average	Average	Average
Age (years)	40.17	40.16	40.17	39.79
Under 30	0.22	0.20	0.27	0.22
30-49	0.53	0.56	0.46	0.54
Above 49	0.25	0.24	0.27	0.24
Marriage	0.44	0.63	0.45	0.63
Danish origin	0.93	0.93	0.94	0.90
Western origin. not Danish	0.01	0.02	0.03	0.02
Non-Western	0.06	0.05	0.03	0.08
Transfer degree < 0.1 last 3 years	0.17	0.08	0.15	0.06
Transfer degree ϵ (0.1;0.5) last 3 years	0.62	0.46	0.62	0.44
Transfer degree > 0.5 last 3 years	0.2	0.46	0.23	0.5
Prior unemployment spell (days)	0.19	0.19	0.40	0.33
Share of new unemployed	0.95	0.96	0.94	0.97
Share in "Construction" industry UI fund	0.17	0.02	0.20	0.03
Share in "Manufacturing" industry UI fund	0.44	0.25	0.40	0.27
Share in Other UI fund	0.07	0.09	0.10	0.10
Number of observations	303	310	304	261

Copenhagen & Sealand (B):	**Control group**		**Treatment group**	
	Men	Women	Men	Women
Characteristics	Average	Average	Average	Average
Age (years)	40.9	40.17	41.34	40.67
Under 30	0.23	0.18	0.21	0.16
30-49	0.49	0.60	0.48	0.63
Above 49	0.29	0.22	0.31	0.21
Marriage	0.54	0.69	0.51	0.58
Danish origin	0.8	0.77	0.88	0.85
Western origin, not Danish	0.03	0.03	0.02	0.03
Non-Western	0.16	0.20	0.10	0.12
Transfer degree $<$ 0.1 last 3 years	0.21	0.09	0.21	0.12
Transfer degree ϵ (0.1;0.5) last 3 years	0.62	0.49	0.60	0.54
Transfer degree $>$ 0.5 last 3 years	0.17	0.42	0.19	0.34
Prior unemployment spell (days)	0.49	1.05	0.23	0.35
Share of new unemployed	0.92	0.91	0.94	0.93
Share in "Construction" industry UI fund	0.15	0.01	0.10	0.01
Share in "Manufacturing" industry UI fund	0.35	0.16	0.35	0.1
Share in Other UI fund	0.13	0.15	0.12	0.15
Number of observations	455	371	376	343

Mid-Jutland (C)	**Control group**		**Treatment group**	
	Men	Women	Men	Women
Characteristics	Average	Average	Average	Average
Age (years)	37.08	35.81	37.46	35.72
Under 30	0.33	0.33	0.33	0.39
30-49	0.5	0.56	0.48	0.48
Above 49	0.17	0.11	0.2	0.13
Marriage	0.4	0.44	0.43	0.46
Danish origin	0.85	0.86	0.84	0.86
Western origin, not Danish	0.03	0.05	0.04	0.07
Non-Western	0.12	0.10	0.12	0.07
Transfer degree $<$ 0.1 last 3 years	0.12	0.07	0.19	0.13
Transfer degree ϵ (0.1;0.5) last 3 years	0.45	0.3	0.4	0.3
Transfer degree $>$ 0.5 last 3 years	0.43	0.62	0.4	0.57
Prior unemployment spell (days)	0.1	0.05	0.27	0.06
Share of new unemployed	0.97	0.98	0.96	0.98
Share in "Construction" industry UI fund	0.07	0.00	0.07	0.00
Share in "Manufacturing" industry UI fund	0.14	0.08	0.19	0.06
Share in Other UI fund	0.08	0.06	0.07	0.08
Number of observations	405	428	393	454

Southern Denmark (D)	**Control group**		**Treatment group**	
Characteristics	Men	Women	Men	Women
	Average	Average	Average	Average
Age (years)	39.28	39.37	40.81	39.58
Under 30	0.31	0.22	0.24	0.20
30-49	0.43	0.57	0.47	0.60
Above 49	0.26	0.21	0.28	0.20
Marriage	0.49	0.63	0.46	0.61
Danish origin	0.85	0.86	0.87	0.91
Western origin, not Danish	0.07	0.05	0.05	0.02
Non-Western	0.08	0.09	0.08	0.07
Transfer degree < 0.1 last 3 years	0.17	0.06	0.13	0.07
Transfer degree ϵ (0.1;0.5) last 3 years	0.60	0.48	0.63	0.48
Transfer degree > 0.5 last 3 years	0.23	0.46	0.25	0.45
Prior unemployment spell (days)	0.05	0.23	0.19	0.12
Share of new unemployed	0.98	0.97	0.97	0.97
Share in "Construction" industry UI fund	0.10	0.02	0.13	0.01
Share in "Manufacturing" industry UI fund	0.41	0.21	0.37	0.19
Share in Other UI fund	0.10	0.14	0.10	0.13
Number of observations	247	248	247	266

Figure 11: Gender-Differences in effects in Experiment B(Top) and C (Bottom)