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Experimental Evidence on the Effects of Early Meetings and Activation*

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Abstract

We analyse three Danish experiments with combinations of early and intensive active labour market policy. We find that frequent individual meetings between newly unemployed workers and their caseworkers have substantial (and significant) effects on employment rates in both the medium and long run. Group meetings or an "activation wall" show positive but insignificant effects. Based on information on the costs of running the experiments, active labour programmes, and public transfer payments, we analyse the impact on government budgets and we show that individual meetings improved budgets with up to 4,500 euros per unemployed worker. We also look at the impact for subgroups.

Keywords: Active labour market policy; cost–benefit analysis; randomized social experiment; treatment effect

JEL classification: J64; J68

I. Introduction

Traditional activation policies (compulsory participation in, for example, workfare or training programmes) are costly, and often do not help in terms of bringing unemployed workers quickly back into regular employment (e.g., Heckman *et al.*, 1999; Kluve, 2010; Card *et al.*, 2010). The most effective activation instrument seems to be employment subsidies in the private sector. Unfortunately, private employers are not easily persuaded

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to participate in such schemes, and it is an often mentioned concern that these schemes displace regular jobs. Hence, in many countries, this instrument cannot be expanded beyond its current limited use. Traditional training programmes (e.g., classroom training) are used more often, and they sometimes have positive effects, especially when aimed at specific groups or disadvantaged workers. However, in general, the evidence regarding their effectiveness is not compelling, especially not when we take into account that these programmes are typically quite expensive. One of the problems with activation is that initially it leads to lock-in effects. This is particularly a problem as, ideally, policies should help the unemployed workers as early as possible when they become unemployed in order to prevent long-term unemployment. The risk of lock-in and the associated dead-weight losses imply that traditional activation policies are potentially ineffective as preventive measures applied during the early phases of unemployment. In that sense, these policies are remediation measures rather than prevention measures. Hence, the need for effective early active policies remains.

In this paper, we present results from three recent randomized field experiments involving early and intensive active labour market policies (ALMPs) aimed at newly unemployed workers in Denmark, with the goal of getting them back into regular employment as soon as possible and thus preventing long-term unemployment. The three experiments we study basically contain a combination of two types of interventions: early and intensive counselling in the form of frequent meetings with caseworkers in job centres, and a so-called "activation wall". The latter refers to mandatory activation employed fairly early during the unemployment spell with the aim of generating so-called threat effects – the perceived risk of future activation should, according to this line of thought, lead to increased job search prior to participation (e.g., Rosholm and Svarer, 2008; van den Berg *et al.*, 2009). Thus, these experiments investigate the effects of novel policy approaches. We study the average impacts in both the short and long run. Using detailed data on the actual implementation of the intended treatment, we show that data on implementation are important for understanding and interpreting the results from an experiment, and thus are crucial for improving our knowledge of the effectiveness of labour market policies. Furthermore, we have obtained data on the costs of running the various programmes and the social transfers that participants receive, and we can therefore compare the realized programme costs and benefits and how they evolve over time, which is an often neglected part in policy evaluations (Card *et al.*, 2010).[1]

[1] Importantly, our analysis incorporates uncertainty from both the benefit side (the estimated employment effect of the experiment) and the cost side (the sampling uncertainty in the type of income transfer and ALMPs unemployed individuals participate in) into the calculated confidence bands associated with the impact of the experiment on government budgets.

Lastly, we study how different groups are affected by the experiments. We focus particularly on the differential effects across gender, age groups, and groups enrolled under different cyclical conditions, as well as impacts on unemployment and employment duration.

The setting in which we study these policies is the Danish labour market. The Danish labour market model, which is generally referred to as the Flexicurity Model and is recommended by the European Commission to its member states (European Commission, 2007), is an interesting setting in which to study these experiments as unemployment insurance (UI) is generally very generous (the security component) and the level of employment protection is quite low (the flexibility component). The sustainability of such a system could be challenged by high structural unemployment rates (e.g., because of low incentives for workers to leave unemployment). Therefore, ALMPs become a pivotal element in ensuring both the availability and the qualification level of the workforce. A recent strand of the literature has focused on optimal design of ALMPs in settings with high UI benefits (e.g., Andersen and Svarer, 2007; Pavoni *et al.*, 2013).

Our paper, which is purely empirical, will not be informative on optimal designs, but we can say something about the relative performance of a system with early interventions in the form of meetings or activation walls versus more traditional policies. Thereby, we provide some guidance regarding important elements of ALMPs that might be studied in an optimal design context. Our paper thus follows a line of research that is focused on other aspects of active policies, such as counselling, monitoring, and sanctions and other shorter interventions, which have a more administrative/institutional character (e.g., Kluve, 2010), and thus greatly differ from the more traditional activation/training programmes, which have been extensively evaluated.

We find that for all investigated policies the average long-run effect is positive, but the effect is statistically significant only in the case of individual meetings (between a caseworker and an unemployed). We analyse the impact of the experiments on government budgets, and we find that while all interventions improve government budgets, individual meetings are a more cost-effective policy instrument compared to both group meetings and activation walls. Individual meetings improve the government budget with close to 4,500 euros per unemployed worker, and the impact is statistically significant.

We analyse the impact on cumulated weeks in employment further, and we find interesting differences with respect to the age and gender of the unemployed worker, and the cyclical conditions. In the case of individual meetings, the effects are especially beneficial for men where they arise later than for women, and seem to arise primarily from longer subsequent employment spells rather than shorter unemployment spells. An activation wall also has a large effect for men, while there is a small temporary negative (lock-in) effect for women. Moreover, the threat effect appears to be present only when labour market conditions are good; we show this point by exploiting the fact that the experiment was conducted during the tipping point of the business cycle in 2008. Hence, differences in the week of enrolment into the experiment (week of inflow into unemployment) can be used as a proxy for different cyclical conditions.

The rest of the paper is organized as follows. In Section II, we provide a brief overview of the literature on the effects of meetings and threat effects (activation walls). In Section III, we describe the social experiments and the data used for the subsequent analysis. Section IV contains a presentation of our main results, and we also discuss subgroup effects, more specifically with respect to age, gender, and the business cycle. We also perform a cost–benefit analysis (CBA) of each intervention. Finally, Section V contains a conclusion, a discussion of policy implications, and further research.[2]

II. A Brief Review of Related Literature

There is an extensive body of literature on the impacts of traditional activation programmes (e.g., Heckman *et al.*, 1999; Card *et al.*, 2010; Kluve, 2010). Policy impacts are typically modest and not always positive.[3] The most favourable effects are found for employment subsidies, while training programmes sometimes have positive effects, especially when aimed at disadvantaged workers. Public job creation shows more negative than positive effects, possibly due to so-called lock-in effects. In this section, we briefly review the literature on meetings and activation walls or threat effects.

Meetings with caseworkers are a cornerstone of ALMPs: unemployed workers often register entry into unemployment at such meetings, and their eligibility for receiving UI benefits, for example, is assessed. Search effort is monitored at meetings, and if there is non-compliance in the form of no-show, insufficient search, or availability, a sanction might be issued. Counselling and job search assistance take place at meetings. There might be direct referral to vacant jobs. Finally, future participation in ALMPs is discussed and planned at meetings.

Meetings have *ex ante* effects. Hägglund (2011) 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

[2] This paper includes an appendix to the main text and an Online Appendix, which are both complementary to the text.
[3] For example, Card *et al.* (2010) find that, in the short term, only 39 percent of the surveyed studies found significantly positive effects. In the medium term, effects were slightly better, with 50 percent being significantly positive.

activity and assisting with more effective job search, led to an increase in the exit rate into employment by 46 percent even before the meeting took (or should have taken) place. Black *et al.* (2003) study a profiling tool aimed at identifying workers at risk of long-term unemployment (LTU). Workers with a high estimated risk of LTU 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.

Ex post effects from meetings are generally positive. Meetings with the aim of increased monitoring tend to find positive or zero effects (small and insignificant). Meetings that focus more on the counselling dimension show similar effects (and may be slightly more favourable compared to solely monitoring). Van den Berg and van der Klaauw (2006) have studied 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 and Robins (1985) find something similar for the US using observational data. Gorter and Kalb (1996) study a randomized experiment conducted in the Netherlands where the time allocated to counselling with caseworkers was increased. They find positive but insignificant effects on the exit rate from unemployment. Hägglund (2009) analyses a social experiment conducted in Sweden where unemployed youth were offered counselling. 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. Crepon *et al.* (2005) analyse a reform implemented in France in 2001, which increased counselling 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.

Dolton and O'Neill (1996, 2002) analysed the ReStart programme. 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 improvement of search behaviour (counselling part) and an assessment of the availability for work (monitoring). A randomized experiment was conducted, and the authors showed that this led to a 30 percent increase in the exit rate from unemployment. The effects were long-lasting: five years after entry into the programme, the treatment group still had statistically significantly less unemployment than the controls. The authors conducted a CBA and concluded that the gains (in terms of saved UI benefits) outweigh the costs of the programme, especially for males. Petrongolo (2009) and Manning (2009) both analyse the Job Seekers Allowance programme implemented in the UK in 1996, looking at long-term and short-term impacts, respectively. This programme 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 (difference-in-differences design), and they find increasing exit rates out of unemployment. However, this is mainly caused by an increased exit rate into incapacity benefits. For the US, Ashenfelter *et al.* (2005) report from a 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 unemployment duration or on the costs of unemployment benefits. Klepinger *et al.* (2002) study another US randomized experiment where unemployed workers are randomized into one of three treatments (and a control group), which involved closer monitoring of different degree and type. Unemployment duration was reduced by 5–7 percent.[4] Lastly, van den Berg *et al.* (2012) use observational data from Danish administrative registers to study the dynamic effects of meetings. They find that the exit rates peak during the week a meeting is held and then taper off over the next eight weeks or so. Moreover, the effects of a sequence of meetings tend to be gradual increases in the exit rate from unemployment to employment.

Finally, regarding activation walls, the body of literature is relatively small. The effects shown by Black *et al.* (2003) and mentioned above could also arise from the perceived risk of activation. 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 and Holm, 2007; Rosholm and Svarer, 2008; van den Berg *et al.*, 2009). This suggests that an early activation wall might have important threat effects, but whether they are large enough to dominate dead-weight losses remains to be seen.[5]

In summary, the literature presented here has established that interactions between caseworkers and unemployed are associated with both *ex ante* and

[4] Johnson and Klepinger (1994) and Meyer (1995) report similar findings from the US, and McVicar (2008) from the UK. Meyer (1995) also presents an analysis on the cost effectiveness of a number of UI experiments involving job search programmes conducted in the US in the 1980s. He generally finds that the benefits outweigh costs.

[5] A large body of literature has established that the long-run effects associated with participation in activation programmes are more favourable than short-run effects (e.g., Card *et al.*, 2010). This is largely a result of the existence of locking-in effects, which dominate in the short run. We do not review this literature here, except for a Danish paper (Jespersen *et al.*, 2008) that illustrates the existence of locking-in effects.

ex post effects. The actual size and significance of the effects vary across studies but, in general, they are positive. Regarding the total impact from activation walls, few studies exist, but the presence of both *ex ante* effects and locking in effects is well established in the literature. In both cases, few studies investigate the cost effectiveness of these policies, and none of the studies looks at the impact on the government budget.

III. Danish Labour Market and the Experiments

In this section, we present the experimental designs and place them in the context of the Danish labour market. We then discuss the experimental design and proceed by analysing the implementation of the treatment protocols. Finally, we look at compliers and non-compliers within the treatment groups.

The Danish labour market is characterized as flexible with less employment protection legislation than most continental European countries and much more labour turnover (OECD, 2009). The Danish labour market has a tight social security net with near-universal eligibility for income transfers. Moreover, ALMPs are among the most intensive in OECD, with around 1.5 percent of GDP spent per year on active policies. There are two types of benefits for unemployed workers: UI benefits and social assistance. Approximately 80 percent of the labour force are members of a UI fund and therefore eligible for UI benefits, while the remaining 20 percent may receive means-tested social assistance. UI benefits are essentially a flat rate. As this paper is only concerned with UI benefit recipients, we present the policies that apply to them. The mutual 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 return to employment. The authorities have an obligation to help the individual improve their situation, but they also have the right to make certain demands of the individual. Under the current rules, an individual who becomes unemployed and is eligible for UI benefits has to register at the local job centre. They then have the obligation to attend a meeting with a caseworker at least every third month. They have the right and obligation to participate in an activation programme after nine months (six months 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 three experiments, who will receive this "treatment as usual".

At the meetings, several issues are discussed. First of all, advice on how to conduct an effective search is provided. For example, caseworkers might discuss which search channels are most effective, search requirements, how to construct a CV, preparing for a job interview, and what wage offers to expect when searching as an unemployed worker. Second, meetings are used to test the availability of unemployed workers for employment, to test if they meet specified job search requirements, and to issue sanctions in case of non-compliance. Third, caseworkers might also have information on specific job openings, so in some cases they can directly provide this information to an unemployed worker, thus engaging directly in the labour market match-making process. Finally, meetings are used for assessing and discussing the qualifications of the unemployed, and whether they meet the demands in the local labour market. Hence, planning of activation activities can also take place at meetings.

Description of the Labour Market Experiments

The set of randomized experiments analysed in this paper consists of three separate experiments, each with their own treatment and control group.[6] They were conducted in three different regions in Denmark. The motivation for having three different experiments rather than three treatment arms, which arguably would have led to greater external validity, was a practical concern about the ability of caseworkers to distinguish three different treatment arms and a control treatment. The experiments are summarized in Table 1.

The subjects of the experiments are individuals who become unemployed during weeks 8–29 in 2008 and who are eligible for UI benefits. Once an individual registers as unemployed, they are randomized into the treatment or control groups based on their date of birth. Individuals born on 16–31 of a month are assigned to the treatment groups, while those born on 1–15 are assigned to the control groups. No information was given to the unemployed workers on the selection rule. Hence, while this is technically not random assignment, as it is predetermined by date of birth, we treat it as such. 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.[7] This information letter marks the start of the treatment, as the worker can 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-entering later. In that case, a worker would re-enter the experimental treatment at the stage where they left it. In all three experiments, the control group receives

[6] There was a fourth experiment in the region of Southern Denmark, consisting of a combination of weekly group meetings and early activation. However, this experiment was compromised, as one job centre did not implement the intervention, and data have been frequently revised. This experiment and its results are discussed in the Online Appendix.

[7] The unemployed individual is not informed that they are participating in a randomized experiment, but rather that they have been chosen to participate in a pilot study.

Table 1. *Overview of the three experiments*

Experiment	Content	Region	Job centres
A	Group meeting each week	Northern Jutland	Frederikshavn, Brønderslev, Hjørring
B	Individual meeting with caseworkers every other week	Copenhagen and Sealand	Gribskov, Roskilde, Ishøj-Vallensbæk, Holbæk, Vordingborg
C	Early activation (after 13 weeks)	Mid-Jutland	Aarhus

treatment as usual, but there might be local variations in the intensity of treatment, which are documented below.[8] The treatment group receives the same treatment as the control group plus the extra elements presented in Table 1.

The experiment labelled "A" in Table 1 was conducted in the region of Northern Jutland. During the first 13 weeks of unemployment, the unemployed worker must attend group meetings each week with a caseworker and a number of other unemployed workers (typically around 10).

The experiment labelled "B" was conducted in the region of Copenhagen and Sealand, and consisted of individual meetings with a caseworker every other week for the first 13 weeks of unemployment (i.e., a total of six to seven extra meetings during the first 13 weeks of unemployment). Note that, generally, the stated main intention of both group and individual meetings was counselling of the unemployed; no explicit extra monitoring was required to take place by the public authorities. However, the perception of the meetings from the point of view of the unemployed worker might have been different.

The experiment labelled "C" was conducted in the region of Mid-Jutland. Here, individuals would be required to participate in an activation programme for at least 25 hours per week from week 14 in unemployment at least until week 26. This experiment – the activation wall – was designed specifically to investigate the combined impact of both *ex ante* effects due to the knowledge of having to participate in an activation programme (threat effects), and locking-in and *ex post* effects of actually having participated (an actual direct programme effect). An evaluation of the experiment will pick up a mixture of these effects, and thereby we can assess the overall efficiency in this particular setting; for instance, whether locking-in effects dominate other effects, or vice versa. Note that in order to quantify the full *ex ante* effect separately, there should have been no actual treatment taking place from week 13 and onwards. However, such a set-up would not

[8] In the CBA, we also take such variation into account.

be legal according to the administrative regulations, and ethical concerns could also be present.

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 to determine eligibility for UI benefit receipt and to determine whether the job centres meet their obligations in terms of meetings and activation intensities. The information is therefore considered highly reliable.

The event history data set includes detailed weekly information on labour market status[9] and history (employment, unemployment, in education, on leave, etc.), meeting attendance and programme participation, ethnicity, gender, residence, marital status, and UI fund membership. There were 5,528 individuals registered as unemployed in one of the nine job centres that were part of the experiments between weeks 8 and 29 of 2008, both weeks inclusive.[10] We have removed all immigrants from the sample because of a concern that immigrants are occasionally assigned an administrative birthday of January 1 when they receive their residence permits. Because the randomization was done by date of birth, this led to an unequal distribution of immigrants across treatment and control groups, which could bias the results. This leaves us with a total sample of 4,730 individuals in the three experiments. The distribution on treatment and control status in the three experiments can be seen in Table 2.[11]

Each person is followed until the end of January 2013. Given the evaluation window (weeks 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

[9] Labour market status is calculated based on information from the register on payments of public income transfers. Data will also tell us whether individuals are employed (in unsubsidized jobs) or not using information from the E-income register, containing information from employers about their employed workers (we do not have information on hours). Finally, there is a residual labour market category, called “self-sufficient”, consisting of the self-employed and individuals who are not working in the market or are not receiving any income transfers (e.g., housewives).

[10] In the following, we use “time since experiment start” to denote the duration since individuals were assigned to treatment and control groups. We only use the information about the week of inflow to construct subgroups in order to approximate different cyclical conditions in the section on heterogeneity effects (see Section IV).

[11] We have also analysed the inflow into the experiment and we have found that the number of individuals entering every week is similar in the treatment and control groups.

Table 2. *Number of individuals in treatment and control groups*

Experiment	Treatment	Control
A (group meetings)	655	705
B (individual meetings)	805	832
C (early activation)	887	836

Table 3. *Summary statistics for Experiment B: individual meetings*

Characteristics	Control Average	Treatment Average	p-value
Age (years)	40.13	40.40	0.64
Aged under 25	0.13	0.11	0.24
Aged 25–49	0.60	0.63	0.26
Aged above 49	0.27	0.26	0.69
Married	0.62	0.60	0.53
Transfer degree	0.26	0.26	0.76
Transfer degree < 0.2 last year	0.63	0.63	0.88
Transfer degree $\varepsilon(0.2; 0.5)$ last year	0.15	0.16	0.65
Transfer degree > 0.5 last year	0.22	0.21	0.82
Share of new unemployed	0.97	0.98	0.67
Transfer degree < 0.2 last three years	0.66	0.63	0.20
Transfer degree $\varepsilon(0.2; 0.5)$ last three years	0.23	0.25	0.19
Transfer degree > 0.5 last three years	0.11	0.11	0.87
Share in UI funds for academics	0.06	0.07	0.42
Share in "manufacturing" UI fund	0.23	0.20	0.08
Share in other UI fund	0.14	0.14	0.79
Number of observations	805	832	
p-value from joint test	0.48		

Notes: Transfer degree is determined as the fraction of the last year spent on some kind of public support (social assistance, UI, study aid, etc.). Membership in the UI fund for academics is generally possible for workers who obtained a bachelor degree or above from a university. The p-values are the p-values associated with the coefficient on treatment status from a simple linear regression where we regress a given characteristic on treatment status (we use Huber–White standard errors). The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

back in time, although the employment information is available only from 2008 onwards. In Table 3, we present summary statistics for the individual characteristics of the members of the treatment and control groups for Experiment B (individual meetings).[12] We test the equality of mean values of characteristics and we do not reject covariate balance at the 5 percent level. We reject covariate balance at the 10 percent level in only one out of 16 tests. Similar tables for Experiments A and C are presented in Tables A1 and A3 in the Appendix to the main text (combining all three tables, only in three cases out of 48 tests is the p-value below 10 percent, and in only one case is the p-value below 5 percent). We have also

[12] This is the experiment that shows the strongest results, and therefore we have chosen to show the balancing table for this experiment in the main text.

compared the control groups across the different experiments in order to assess how similar the individuals are. This provides some guidance about whether we can compare the effects of the experiments across regions. In the Online Appendix (Table OA.1), we show tests of equality of means across regions, and from these we conclude that the population in Experiment C (early activation) is different from the population in the two other experiments. The populations in the experiments with meetings look more similar, although there are still some differences. These findings suggest that we should be cautious in making too tight comparisons across the experiments.

Implementation

In this subsection, we present evidence on the implementation of the three 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 for unemployed individuals. We have also tabulated these intensities by gender, and we have found no remarkable differences in this dimension. The figures should be regarded as lower bounds on the actual implementation in the job centres, as unemployed individuals participating in, for instance, two meetings in a given week will only be counted once, and individuals transiting into employment in a given week are not included. To compare control groups across the experiments, we have also exploited the data on the implementation and the existence of multiple control groups and different time profiles of treatment to assess whether we can find any evidence on substitution effects due to the experiment (i.e., control groups being treated to a smaller extent).[13] We have not found such differences (Figure OA.2 in the Online Appendix shows this comparison). This is also supported by Figure OA.1, which shows the Kaplan–Meier survival curves for the three control groups. A rank test cannot reject that they are pairwise equal.

Figure 1 plots the weekly meeting intensity in the three regions for the treatment and control groups. In Experiment A, the treatment group was intended to participate in group meetings on a weekly basis. Only around 60 percent of the treatment group, who were still unemployed in a given week, participated in meetings (during the first 13 weeks). After 52 weeks, unemployed treated individuals will, on average, have participated in roughly seven meetings more than individuals in the control group. In Experiment B, we observe a saw-tooth pattern reflecting the fortnightly meetings. Summing the meeting intensities for two consecutive weeks, the

[13] To avoid such effects, extra resources were given to the job centres in compensation for the intensified treatment requirement.

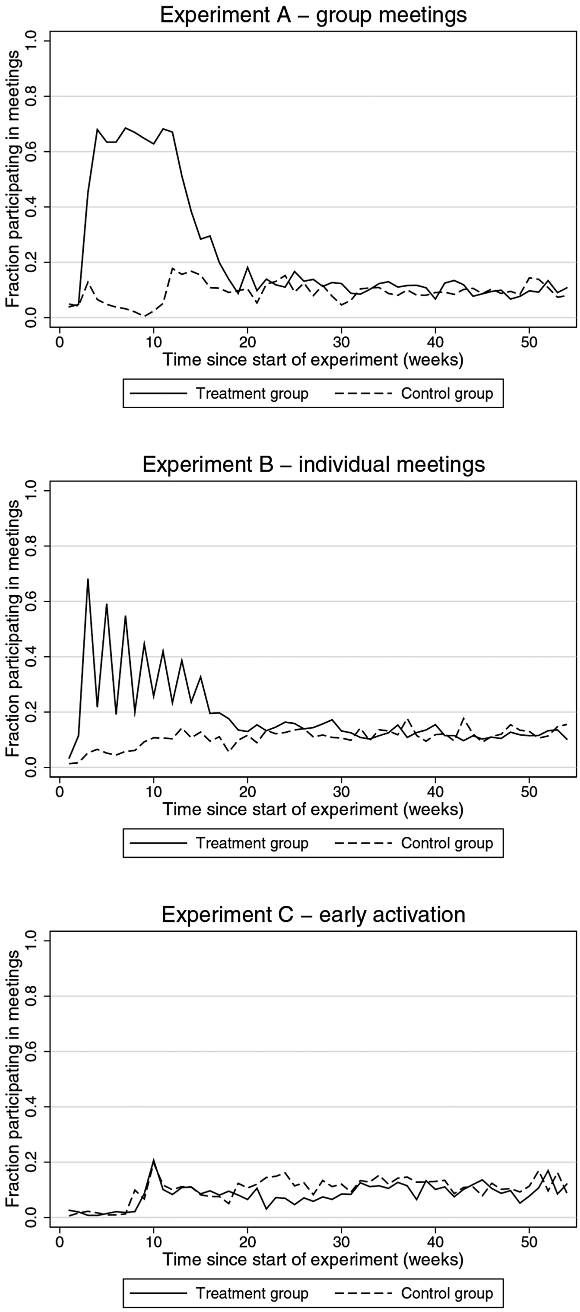

Fig. 1. Weekly meeting intensities (for those who are still unemployed in a given week)

fortnightly meeting intensity begins around 90 percent, and then falls to about 65 percent around week 13. After 52 weeks, treated unemployed have participated in five meetings more, on average. 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 with the requirements of the experiment, the treatment groups in the two relevant projects attended substantially more meetings than did the corresponding control groups during the early phases of the unemployment spell. In the following, we look more deeply into the characteristics of those that actually receive treatment. The meeting rate for the treatment and 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 2 shows weekly activation intensities. In Experiment C with early activation, there is a sharp increase in the activation intensity around week 13. Again, not everyone in the treatment groups was activated between weeks 13 and 26, but the activation intensity is much higher for the treatment group than for the control group. After 52 weeks, treated unemployed have participated in 13 weeks of activation more than the control group, on average. 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 four to eight weeks. This category of programmes is the most commonly used activation instrument in Denmark (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 ALMP in the Danish Flexicurity Model (e.g., Andersen and Svarer, 2007). After the end of the experimental treatment period (in week 26), the activation intensities for the treatment and control groups converge rather quickly. In Experiment B, a larger fraction of the remaining unemployed in the treatment group is activated compared to the control group. This could reflect outcomes from the meetings with caseworkers, or alternatively just dynamic selection out of the group (in the following, we show that a higher share of individuals in the treatment group are employed at this point).

Overall, the meetings and activation intensity figures reveal that the treatment groups, to a large extent, received the intended treatments, and they were treated much more intensively than the control groups in the

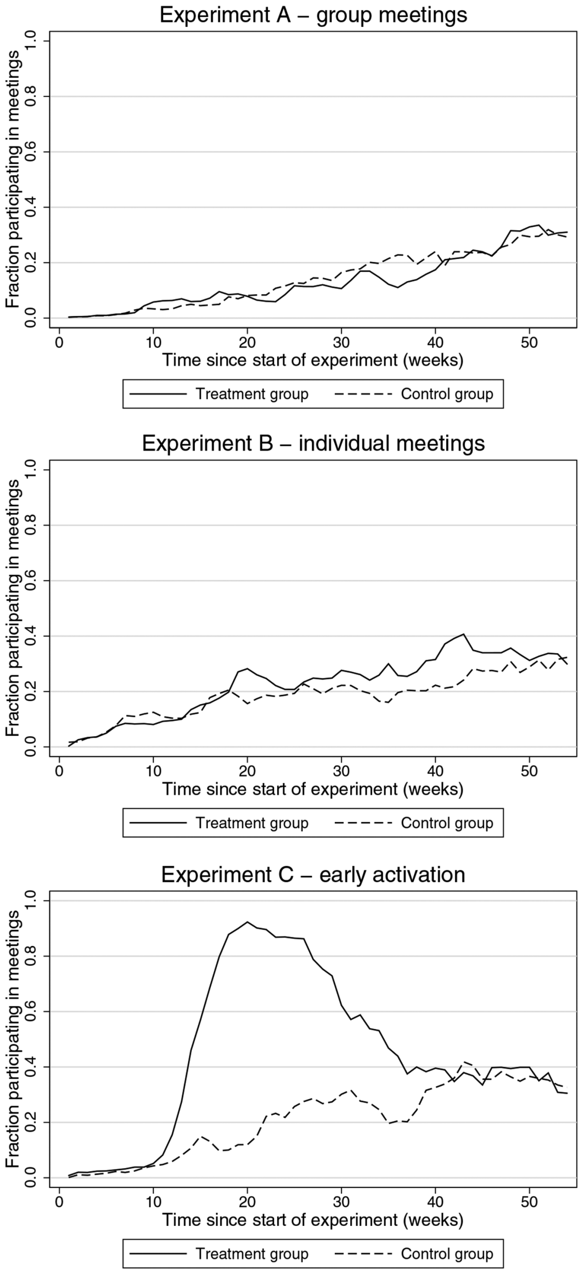

Fig. 2. Weekly activation intensities (for those who are still unemployed in a given week)

relevant dimensions. As compliance to the treatment protocol is not 100 percent, the treatment effects that we determine below can be regarded as intention to treat (ITT) effects. We do not report average treatment effects (ATEs) as we believe that the ITT effects are really the policy-relevant effects in this setting as they reflect the modus operandi. Furthermore, compliance is not a static concept in our experiments.[14] The analysis also highlights how important the data on the actual implementation of the treatment are for our understanding of the effects that we present in the following. Compliance is never perfect.

In the Online Appendix, the issue of non-compliance is described in more detail. The main conclusion is that around 90 percent of the variation in accumulated compliance status (see the Online Appendix Table OA.2) is due to factors unobserved by us. Thus, there are few systematic differences between complier and non-complier based on observed characteristics. From the 10 percent of the variation we can explain, we do see some indications that the non-compliers are generally the "weaker" unemployed (e.g., those having a history of unemployment and sick listing), which could also support the fact that the ITT effects are not directly transferable to the ATEs (if we believe weaker unemployed are, for instance, less likely to benefit from treatments). Naturally, focusing on ITT effects implies that there is no correction for a potential difference in compliance rates between regions, and therefore effects could be driven by more effort from certain regions/job centres (which simply treat more). This point serves to motivate why the CBA, which we present in Section IV is a crucial element in the evaluation, and why data on the actual implementation of policies are important. In the CBA, we use the actual costs encountered and thereby we take the compliance rates into account in assessing the effectiveness of different treatments.

IV. Empirical Results

In this section, we present the effects from each of the three experiments. We report the treatment effect on weeks in employment for each week in a long sample window after the experiment started. The main outcome is the accumulated number of weeks employed from the start of the experiment until week t, and then we let t vary from 1 to 237 weeks.[15] The effect

[14] A complier one week is likely to be a non-complier the next week. Furthermore, scaling by compliance degree (to obtain ATEs) would imply that we assume away *ex ante* effects, which have been found earlier in the literature (see above).

[15] Because we evaluate the experiment in a dynamic setting, we choose this outcome as a summary statistic of the (potential) differential effects over time. This would also allow for even small differences between the fraction employed in treatment and control groups

of treatment in week t for individual i (β_t) is estimated in the following regression:

$$W_{it} = \alpha_t + \beta_t T_i + \gamma_t H_{it} + \varepsilon_{it}.$$

Here, W_{it} is the accumulated number of weeks in employment t weeks after enrolment into the experiment, T_i denotes treatment status, and H_{it} is a measure of previous employment history.[16] 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 side of the two-sided confidence interval of the effects at both 5 and 10 percent levels.[17,18]

Figure 3 shows the effects of three experiments. The effects are shown as the accumulated difference in employment. In Figure A1 in the Appendix to the main text, the employment rates for the control groups are shown. These are compared to Figure 3 to get an impression of the relative size of the effects (we also comment on relative size below).

For the experiment with group meetings, the difference in accumulated employment is close to five weeks after 237 weeks, but the effect is not statistically significant. The fact that the effect starts accumulating after a year suggests that the primary channel through which group meetings affect employment is via longer employment duration rather than shorter unemployment duration.[19] For the experiment with individual meetings, the effect is significantly positive until around four years after the experiment, and thereafter it is marginally insignificant at the 5 percent level. Contrary to the two other experiments, the effects start to accumulate from the week of entry into unemployment. After 237 weeks, the difference between the treatment and control groups is more than seven weeks in employment. Considering that the mean employment rate of the control group in the sample period is close to 55 percent, this corresponds to an increase of

to accumulate over time. Note also that we focus on weeks in registered employment, and therefore weeks as self-sufficient (see the Data subsection in Section III) are not counted as a part of the treatment effect.

[16] Note that H_{it} measures the number of weeks without public income support in the time-span from -10 to $-10 - t$ weeks before enrolment into the experiment.

[17] For instance, for positive effects, we do not report the upper bound of the confidence interval.

[18] In practice, we run a regression for each t with Huber–White standard errors. We have also estimated standard errors clustered at the individual level in a panel bootstrap procedure, and this does not change the following results. The results are also robust to other model specifications that account for probability mass in zero weeks of accumulated employment (e.g., a tobit model). Finally, the Online Appendix shows that the results are unaffected by conditioning on different combinations of covariates (see Tables OA.3 and OA.4).

[19] We investigate the timing of effects in the subsection on heterogeneous effects.

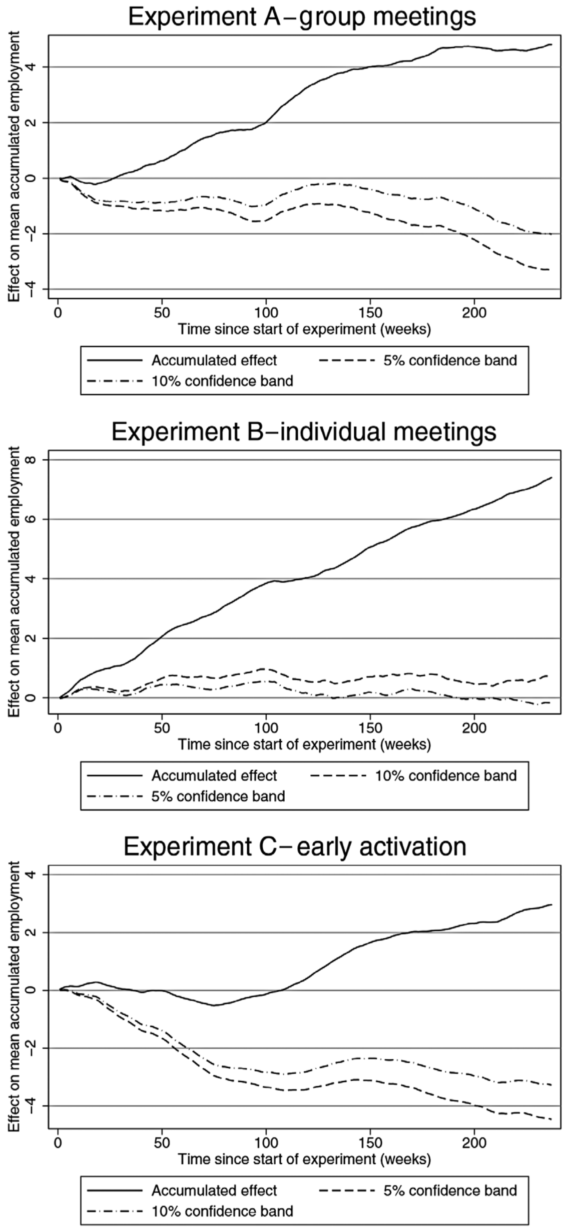

Fig. 3. Employment effect of the three experiments
Notes: The figures show the accumulated difference in the number of weeks employed between the treatment and control groups. We only show the relevant side of the two-sided confidence bands. Confidence bands are obtained from linear regressions with Huber–White standard errors.

around 5 percent in employment over the period after entry into the programme.[20] For the experiment with early activation, there is no significant effect on the difference in accumulated employment, and the effect only becomes positive after around two years.

In sum, all three experiments increase the employment rates for the treatment group. However, the effects are only statistically significant for the experiment involving individual meetings. Lack of statistical significance in the two other experiments could be because of no real effect, high standard errors due to relatively few observations and a "noisy" outcome, or both.[21] In the next subsection, the effects of the experiments are compared with the costs of running the programme.

Effects on Government Budget

In this section, we contrast the costs of running each of the three experiments with the gains from increasing employment rates. Above, we have documented differences in the implementation of the treatments across the experiments, and our analysis here offers a way to compare the employment gains from different treatments to the realized costs. We focus on the direct impact on the government budget. The government budget is affected by reduced income transfers, by increased taxes from increased production, and by the costs of running the programmes.

The costs are split into costs of income transfers and costs of operating ALMPs (called programme costs).[22] 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 and cover both regular activities and the increased activities due to the experiment (see Figures 2 and 3).[23] Cost data are provided by the National Labour Market Authority. Individual meetings last between 15 and 30 minutes (the information on the duration and size of meetings is specific to each job centre and is provided by the participating job centres), and group meetings last two to three 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. The actual

[20] The effect is calculated as $7.4w/(0.55 \times 237w) = 5.7$ percent.

[21] In the Online Appendix, we show that the main findings are unaffected by conditioning on more covariates in the regressions presented in Figure 4 (see Table OA.3 and OA.4).

[22] In the Online Appendix (see Section 5), we provide more details about the cost components and other components in the CBA.

[23] This implies that we take into account that an increase in employment rates in the treatment group also affect the costs associated with regular ALMP activities, which have been documented above.

(observed) number of meetings, weeks in activation, and received transfers are used in the calculations, and hence the compliance to the treatment protocol is taken into account when assessing the cost effectiveness of the interventions.

Public income transfers represent only a reallocation of income, and hence in a traditional cost–benefit calculation they would not be included (see the Online Appendix for an example). However, they have a direct effect on the government budget. In Denmark, public transfers are subject to income taxes. According to the Ministry of Labour, the average tax rate for recipients of unemployment benefits is 37.5 percent. In addition, their consumption results in further tax payments to the government of 24.5 percent through value-added taxes, energy taxes, etc. In addition, we assume that employed workers are able to obtain work at an annual wage of approximately 40,000 euros (with 46 working weeks).[24] We assume further that all the gains from increased production accrue to the workers (this implies that we do not have to consider increases in revenues from the taxes of firms, etc.). The impact on the government budget is then the saved income transfers after taking into account the fact that paying out social transfers also leads to increases in tax payments from both income taxes and taxes on consumption. To this we add the increase in tax payments and indirect taxes, which the increased production generates.

In Table 4, we show the effects on the government budget 237 weeks since entry into the experiment. We use an annual discount rate of 3 percent,[25] and we provide 95 percent confidence intervals based on bootstrapping. For the bootstrap, we draw repeated random samples of individuals with replacement, and in each subsample we compute the various components in the CBA. This allows us to take account of uncertainty from both the benefit side (the estimated employment effect of the experiment) and the costs side (the sampling uncertainty in the type of income transfer and ALMPs in which unemployed individuals participate).

Table 4 shows that individual meetings with caseworkers lead to the largest net gains to government budgets. Experiment B is the only experiment where the net gain is statistically significant. The discounted net gain per unemployment spell is 4,457 euros. The net gain to the budget is also positive for the two other interventions, but not significantly so. The net gain from individual meetings is almost twice as large compared to group meetings, and the employment effect and low programme costs are the major factors behind this result.[26]

[24] This is the average annual wage of an unemployed that began a new job in 2008.

[25] We have also tried discount rates of 2 and 4 percent, and our conclusions are not affected.

[26] The low programme costs in Experiment B are driven by two things. First, the treatment is relatively cheap (a maximum of seven individual meetings). Secondly, the treatment generates

Table 4. *Effect on government budget after 237 weeks (per individual)*

	A Group meetings	B Individual meetings	C Early activation
Saved income transfers[a]	3,303	3,631	1,392
Gain from saved transfers[b]	1,558	1,713	657
Value of increased production[c]	4,263	6,508	2,607
Gain from increased production[d]	2,251	3,438	1,377
Cost of programme (after 26 weeks)[e]	48	−26	358
Costs of programme (after 237 weeeks)[f]	903	47	440
Net effect on budget ($b+d-f$)	2,906	5,104	1,594
Discounted effect on budget[g]	2,539	4,457	1,392
Confidence intervals	[−1812; 6368]	[486; 8215]	[−2485; 4856]

Notes: [a] Calculated as the difference in public transfers paid to treatment versus control group in the first 237 weeks. [b] Effect of saved transfers when adjusted for direct (37.5 percent) and indirect (24.5 percent) taxes. [c] Based on an annual income of 40,000 euros. [d] The effect from taxes on value of increased production. [e] Direct programme costs after 26 weeks (includes both treatment and regular programme participation). [f] Direct programme costs after 237 weeks (includes both treatment and regular programme participation). [g] Discounted effect using 3 percent annual discount rate. Standard errors are calculated using bootstrapping.

As a supplement to the analysis of the effects of the experiments on the government budget, Figure OA.5 in the Online Appendix presents the outcome of a more classical CBA (welfare gains under distortive taxation). Here, the full gain from production is included in the calculation (assuming again that the wage of the worker is a measure of the gain). We assume that the individuals do not value any lost leisure.[27] In addition, we assume that the marginal cost of public funds is 20 percent, meaning that to finance a given transfer to the unemployed the loss to society is 20 percent (this is the official rate recommended for cost–benefit calculations by the Danish Ministry of Finance). When reducing transfers, the gain to society amounts to 20 percent of the saved transfers. The saved transfers as such are not included in the CBA as they are simply a transfer internally in society. The costs are the direct costs of running the programme corrected for the marginal costs of public funds needed to finance the extra costs.[28] The CBA

an immediate and substantial increase in the outflow from unemployment, which reduces the number of individuals in the treatment group who participate in both regular and treatment activities. Similar reasoning explains the difference in programme costs between Experiments A and C. While the cost associated with participation in activation programmes is larger than for group meetings (see the Online Appendix), a substantial fraction of the treatment group is in employment after 13 weeks when early activation starts (see Figure A1). Therefore, the impact on government budgets is smaller in Experiment C than in Experiment A.

[27] See Maibom (2015) for an analysis that quantifies the value of lost leisure, and thus extends the analysis.

[28] The Online Appendix gives an example of the whole calculation (Table OA.13).

gives the same message as the analysis on the gain to government budgets. That is, the experiment with individual meetings generates a positive return, whereas the two other experiments do not give a statistically significant positive return.

Both CBAs ignore general equilibrium effects, and the interpretation should keep that in mind. Also, for the later analysis there is some discussion in the literature (e.g., Kreiner and Verdelin, 2012; Jacobs, 2013) on whether marginal costs of public funds should be included. There are different practices in the literature, and we do not take a clear stand on whether they should be included or not. The main message will not be altered by assuming marginal costs of public funds to be zero (welfare gain under lump-sum taxation). It would, of course, be interesting to know what the full-blown effects would be on social welfare. Is there, for instance, a positive return in a CBA that includes effects on welfare for the unemployed and on the functioning of the labour market, where potential effects on wages and total employment are taken into account? Based on the analysis presented in this paper, we have no information on these effects, and we therefore present the effects on the government budget as the main indicator of the return of running the experiments. Gautier *et al.* (2012) present a CBA of another Danish experiment with early activation. They use a search-matching model to assess the effects of welfare and labour market performance, and they find that the partial equilibrium effects can change substantially when general equilibrium effects are taken into account. The finding relies on rather strong functional assumptions, and we do not pursue a similar type of analysis in this paper. However, we emphasize that the effects of the experiments on government budgets only provide a partial picture of the return of introducing more intensive ALMP.

Heterogeneous Effects

The analysis we have presented has shown that individual meetings, in particular, had positive effects on employment rates. In this subsection, we present the results for different subgroups. In the Online Appendix, we show the results of the experiments separately for males and females (Figure OA.4), and we also present a series of tables (separately for males and females) with results from a linear regression of accumulated weeks of employment in week 237 on treatment status, age group, business cycle indicator, and their interactions (Tables OA.5– OA.10).

The business cycle indicator is potentially interesting because the experiment began just prior to the onset of the financial crisis. Figure 4 depicts the normalized (first quarter of 2000 = 1) outflow rate from unemployment to employment of individuals entering unemployment from 2000 until

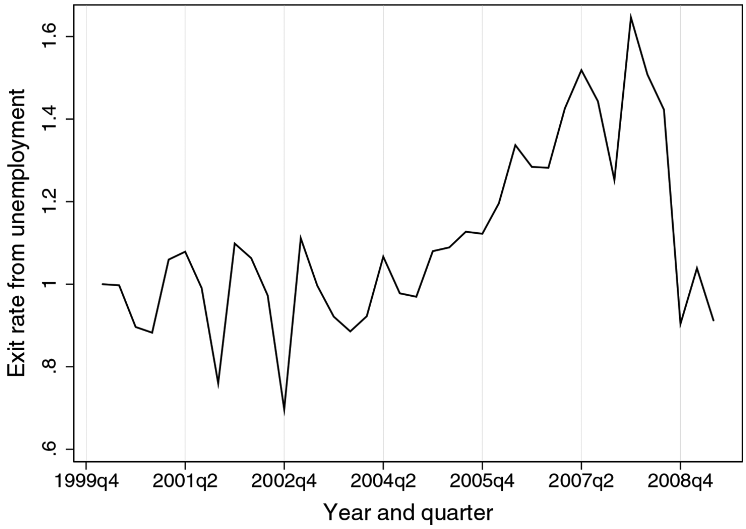

Fig. 4. Normalized exit rate from unemployment
Source: Own calculations based on an estimated duration model.

2009, conditional on a wide range of explanatory variables.[29] The figure illustrates the 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 third quarter of 2008 onwards. This implies that individuals becoming unemployed in the last part of the inflow period (weeks 16–29) of the experiment potentially experience worse labour market conditions conditional on elapsed duration, as they become unemployed very close to this dramatic decline in outflow rates.[30]

Browsing through the tables in the Online Appendix, it is also clear that most of the interaction effects are insignificant and that there are

[29] This rate is determined by estimating a piecewise constant unemployment duration model. The sample consists of the inflow into unemployment during the period from 2000 to the first half of 2009. The specification includes a wide range of explanatory variables, including labour market history and demographics, and specifically time-varying quarterly dummies, whose estimated coefficients are shown in the graph and capture any variation over time in the outflow rate to employment that is not accounted for by observed variables.
[30] In practice, therefore, we analyse the impact of the experiment in two subsamples defined by whether individuals enrolled in good (weeks 8–15) or bad (weeks 16–29) times. As mentioned earlier, the inflow into control and treatment groups is stable, so we can in fact treat the treatment and control groups in good and bad times as a separate experiment. It is important to keep in mind that we assume that the intensity and efficiency of treatment are constant with respect to the week of inflow in the following.

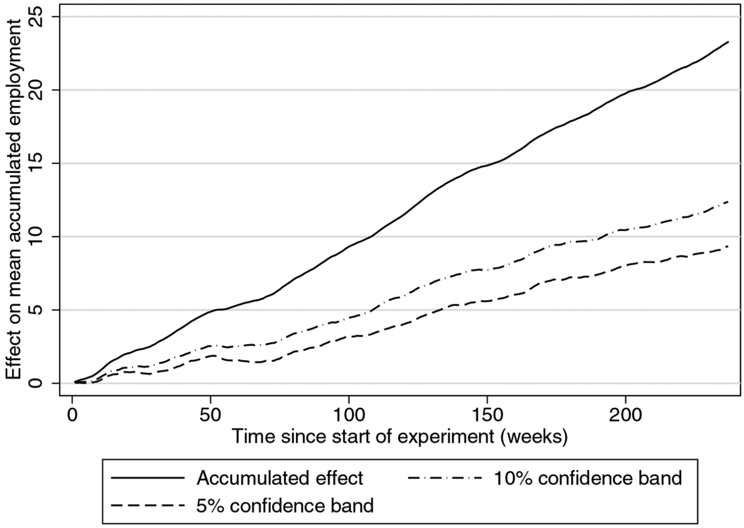

Fig. 5. Effects for Experiment C: early activation for men who enrol early

no clear patterns in the results across characteristics. From the figures with the results estimated separately by gender (Figure OA.4 in the Online Appendix), it appears that men exhibit larger positive effects from both individual and group meetings than women. In the case of group meetings, the effect now becomes statistically significant (although only marginally) and males spend 10 weeks more in employment in the treatment group. When we look at the various interactions in the subsequent tables, we see that, in particular, married men exhibit large positive effects (Tables OA.8 and OA.9). For the experiment with early activation, there is also a positive (but insignificant) effect for men, and from the tables we see that this is particularly true for young males and males who enrol early in the experiment (Table OA.10). In Figure 5, this latter finding is emphasized by showing the effects for this particular subsample.

The finding that the effects of early activation are particularly strong for men is consistent with, for example, Rosholm and Svarer (2008). Here, it is also found that men react more to the perceived risk of activation than women. The positive impact from the business cycle situation suggests that labour market prospects, which improve job finding rates, might be beneficial for the effects of active labour market programmes. In Table OA.11 in the Online Appendix, the findings from a multistate duration model are presented. The model investigates the impact of being in the treatment group on unemployment and employment duration two years after enrolment in the programme separately for males and females. The main findings are that, for men, the improvement in employment from meetings is caused by a reduced exit from their subsequent employment spells. That is, the duration analysis indicates that the effects from attending meetings for men is not that they find jobs faster, but that they stay employed longer compared to the control group. In addition, the duration results find that there is a locking-in effect for women in Experiment C with early activation (see also Figure OA.4). This highlights the trade-off by having activation demands early in an unemployment spell. Early activation might motivate unemployed workers to increase job search, but for those who do not manage to find a job, participation in activation reduces time for job search, and can be detrimental to job finding while enrolled in activation.

V. Conclusion

In this paper, we present the labour market effects of three randomized experiments conducted in Denmark in 2008. The experiments entailed different combinations of early and intensive treatment in terms of meetings and activation. The analysis documents some differences in the design of the experiment and its actual implementation. In particular, treatment compliance is not 100 percent but, nevertheless, it is still substantial. Because the group of non-compliers is generally a group of weaker unemployed workers and because the literature has shown important anticipation or *ex ante* effects, the focus is on the ITT effects.

Individual meetings between newly unemployed workers and caseworkers increase employment rates over the next four and a half years by 5 percent, and they improve the government budget by close to 4,500 euros per unemployed worker. The positive effect of individual meetings for newly unemployed workers is consistent with the findings in the literature on the effects of meetings between caseworkers and unemployed workers, and it indicates that a strong focus on close interaction between caseworkers and unemployed workers early in the unemployment period might be a profitable part of ALMP. Group meetings, which are clearly cheaper per unit treated than individual meetings, also have a positive impact on employment rates, but the results found in the current study are not statistically significant (except if we look at males only). Early activation provides the least favourable outcome of the three experiments analysed, and further analysis indicates that although there might be *ex ante* effects on exit rates for some unemployed (and especially when job-finding possibilities are high) there are also indications of locking-in effects, which highlights the trade-off between introducing intensive and time-consuming activities early in the unemployment spell.

The strength of the current analysis is that it is possible to relate the findings on employment with the costs of running the programme, and hereby the paper contributes with a more elaborate analysis than what is typically seen in the literature on the evaluation of ALMP. However, there is still a big step towards a more comprehensive CBA analysis that also considers general equilibrium effects and welfare effects. We see that as a natural path for future research in this area.

Appendix

To illustrate the composition of unemployed workers across regions, in this appendix we provide summary statistics on the composition of the treatment and control groups for each experiment. In addition, Kaplan–Meier survival plots for the control groups in each experiment are compared. Figure A1 shows the Kaplan–Meier survival rates for control groups in each of the three regions. Log-rank tests do not reject the null hypothesis of equality of survivor functions.

Table A1. *Summary statistics for Experiment A – group meetings*

Characteristics	Control Average	Treatment Average	p-value
Age (years)	39.97	39.21	0.23
Aged under 25	0.12	0.15	0.16
Aged 25–49	0.60	0.58	0.53
Aged above 49	0.27	0.26	0.69
Married	0.60	0.58	0.36
Transfer degree	0.27	0.28	0.38
Transfer degree < 0.2 last year	0.62	0.62	0.87
Transfer degree $\varepsilon(0.2; 0.5)$ last year	0.16	0.14	0.25
Transfer degree > 0.5 last year	0.22	0.24	0.42
Share of new unemployed	0.99	0.97	0.03
Transfer degree < 0.2 last three years	0.61	0.60	0.67
Transfer degree $\varepsilon(0.2; 0.5)$ last three years	0.28	0.25	0.36
Transfer degree > 0.5 last three years	0.11	0.15	0.07
Share in UI funds for academics	0.02	0.03	0.46
Share in “manufacturing” UI fund	0.32	0.29	0.21
Share in other UI fund	0.10	0.10	0.72
Number of observations	705	655	
p-value from joint test	0.42		

Notes: Transfer degree is determined as the fraction of the last year spent on some kind of public support (social assistance, UI, study aid, etc.). The p-values are the p-values associated with the coefficient on treatment status from a simple linear regression where we regress a given characteristic on treatment status (we use robust standard errors). The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

Table A2. *Summary statistics for Experiment B – individual meetings*

Characteristics	Control Average	Treatment Average	p-value
Age (years)	40.13	40.40	0.64
Aged under 25	0.13	0.11	0.24
Aged 25–49	0.60	0.63	0.26
Aged above 49	0.27	0.26	0.69
Married	0.62	0.60	0.53
Transfer degree	0.26	0.26	0.76
Transfer degree < 0.2 last year	0.63	0.63	0.88
Transfer degree $\varepsilon(0.2; 0.5)$ last year	0.15	0.16	0.65
Transfer degree > 0.5 last year	0.22	0.21	0.82
Share of new unemployed	0.97	0.98	0.67
Transfer degree < 0.2 last three years	0.66	0.63	0.20
Transfer degree $\varepsilon(0.2; 0.5)$ last three years	0.23	0.25	0.19
Transfer degree > 0.5 last three years	0.11	0.11	0.87
Share in UI funds for academics	0.06	0.07	0.42
Share in "manufacturing" UI fund	0.23	0.20	0.08
Share in other UI fund	0.14	0.14	0.79
Number of observations	805	832	
p-value from joint test	0.48		

Notes: Transfer degree is determined as the fraction of the last year spent on some kind of public support (social assistance, UI, study aid, etc.). The p-values are the p-values associated with the coefficient on treatment status from a simple linear regression where we regress a given characteristic on treatment status (we use robust standard errors). The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

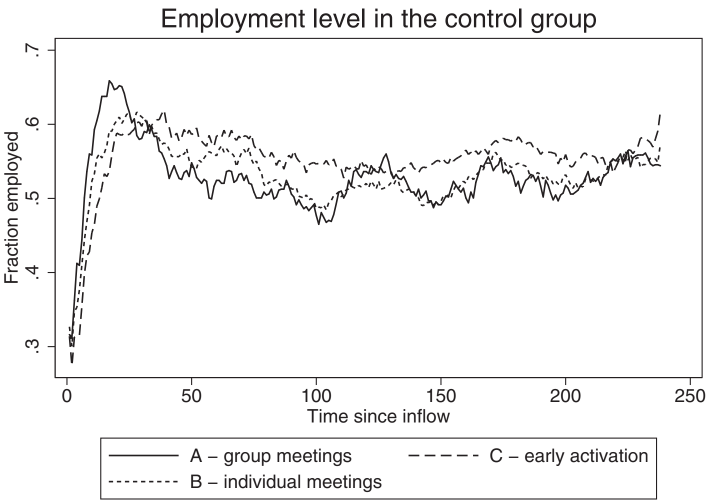

Fig. A1. Employment rates for the control groups

Table A3. *Summary statistics for Experiment C – early activation*

Characteristics	Control Average	Treatment Average	p-value
Age (years)	36.21	36.24	0.94
Aged under 25	0.12	0.13	0.57
Aged 25–49	0.72	0.69	0.20
Aged above 49	0.16	0.18	0.30
Married	0.46	0.50	0.12
Transfer degree	0.44	0.44	0.58
Transfer degree < 0.2 last year	0.46	0.47	0.29
Transfer degree $\varepsilon(0.2; 0.5)$ last year	0.19	0.17	0.20
Transfer degree > 0.5 last year	0.35	0.36	0.79
Share of new unemployed	0.99	0.99	0.37
Transfer degree < 0.2 last three years	0.45	0.47	0.43
Transfer degree $\varepsilon(0.2; 0.5)$ last three years	0.20	0.17	0.28
Transfer degree > 0.5 last three years	0.35	0.36	0.96
Share in UI funds for academics	0.32	0.30	0.39
Share in "manufacturing" UI fund	0.10	0.10	0.94
Share in other UI fund	0.06	0.06	0.85
Number of observations	836	887	
p-value from joint test	0.63		

Notes: Transfer degree is determined as the fraction of the last year spent on some kind of public support (social assistance, UI, study aid, etc.). The p-values are the p-values associated with the coefficient on treatment status from a simple linear regression where we regress a given characteristic on treatment status (we use robust standard errors). The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

Supporting Information

The following supporting information can be found in the online version of this article at the publisher's web site.

Online Appendix

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