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ABSTRACT

Experimental Evidence on the Nature of the Danish Employment Miracle*

This paper uses a social experiment in labour market policy – providing early and intensive monitoring and programme participation in unemployment spells – to assess the nature of labour market policy effectiveness. The experiment was conducted in two counties in Denmark during the winter of 2005-6. The treatment consisted of a dramatic intensification of labour market policies. The results show that the intensification of labour market policies is highly effective, leading to increases in the exit rate from unemployment ranging from 20 to 40%. When introducing time-varying indicators for the various specific treatments actually prescribed to the unemployed workers, none of those treatments have a positive effect on the exit rate from unemployment, neither during the week in which the activity takes place, nor after the activity is completed. However, when the estimated risk of participating in an activity is included as an explanatory variable, it removes the difference in job-finding rates between treatment and control groups completely in one of the counties, and reduces it dramatically and renders it insignificant in the other county. The interpretation we attach to these results is the following; since individual treatments do not appear to be effective *per se*, but the risk of treatment is, it must be that the intensification of the policy regime increases the job-finding rate of unemployed workers.

JEL Classification: J64, J65, J68

Keywords: social experiment, labour market policy regime, treatment effect, threat effect, duration model

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1 Introduction

This paper exploits evidence from a social experiment - providing early and intensive monitoring and active labour market programme participation to unemployed workers - to derive some lessons regarding the nature of labour market policy effectiveness in general and the perceived success of the Danish labour market model in particular.

Recently, the Danish model of the labour market has received much attention in the rest of Europe, because of its perceived effectiveness in maintaining high participation rates and low unemployment rates. This successful performance is often attributed to the highly praised 'Flexicurity' model. Basically, the Flexicurity model consists of three components; 1) flexible hiring and firing rules and regulations, 2) a generous unemployment insurance and unemployment assistance system, and 3) a very active labour market policy ensuring the availability and the qualificational level of the work force.

In the 1980s, when unemployment rates were persistently high, the first two components of the Flexicurity model - flexibility and security - were already in operation in the Danish labour market, but the active labour market policy was only in its infant stages and not nearly as intensive as it has become today. Therefore, many have seen intensive active labour market policies as a pivotal component in the model. However, most researchers would agree that, in general, the direct effects on earnings and employment at the level of the individual participant in active labour market programmes are negligible (and in some cases even negative), and this is also the case in Denmark. Hence, it is this apparently paradoxical situation which we intend to shed light on in this paper.

The reform that began the intensification of the active labour market policies was introduced in 1994, and since then it has been gradually intensified until 2003, when it was loosened again in some dimensions, while it was tightened in others. For example, until 2003 it had been the case that individuals who had been unemployed for one year should participate in active labour market programmes (ALMP) for at least 80% of their remaining time in unemployment. This was called the 'active period', and it was criticized for becoming a 'programme factory', where participants were assigned to programmes not on the basis of need, but because of the rules. However, this 'active period' has been shown to be indirectly quite effective because of the so-called threat effect of ALMPs, see e.g. Rosholm & Svarer (2008), Geerdsen (2006), and Geerdsen & Holm (2007). Namely, the programmes were not effective in themselves, on the contrary, but the perceived threat of having to participate continuously in ALMPs led unemployed workers to leave the unemployment queue before entering the 'active period'. The threat

effect has also been documented by other researchers, most notably by Black *et al.* (2003), but also by e.g. Hägglund (2006).

This observation points to the more general observation made above regarding the effectiveness of active labour market policies; despite the variation in the implementation of them throughout the world, and despite their verbalized effectiveness and political popularity in e.g. the Danish Flexicurity model, econometricians have generally failed to estimate large positive employment or earnings effects. On the contrary, there is plenty of evidence that programme effects are small and in some cases even negative, see e.g. Heckman *et al.* (1999), Kluve & Schmidt (2002) and Kluve (2006). Could it be, then, that the virtues of such programmes should be sought elsewhere? The presence of the threat effect suggests that programme participation - and possibly active labour market policies in general - acts as a 'tax' on unemployed individuals' leisure time; by taxing away some of the unemployed workers' time, the marginal utility of spending time in unemployment falls, and hence, unemployment spells are shortened due to increased search activities or lower reservation wages. The threat effect materializes the moment the individual realizes that there is a positive risk of having to participate in a programme in the future, and when that risk becomes substantial, it may significantly affect the search behaviour of the individual.

In this paper, we analyse a social experiment conducted in two counties in Denmark during the winter of 2005-6. Specifically, all individuals in the two counties who became unemployed during this period were randomized into a treatment and a control group based on their birthday; those born on the 1^{st} to the 15^{th} were given the treatment, and those born on the 16^{th} to the 31^{st} were not. The treatment consisted of a dramatic intensification of labour market policies, involving information, very early mandatory participation in job search assistance programmes, highly frequent meetings with employment officers, and full-time programme participation for at least three months if they had not found employment before 18 weeks of unemployment. Since the treatment in part consists of giving the unemployed worker information about the new labour market regime, treatment starts on the day the individual receives that information. Inspection of survival functions can inform us about the treatment effect of the entire experiment, but if we want to know more about the impacts of separate components in the experiment, a carefully designed econometric analysis is required.

A duration model is the natural starting point for analysing this experiment due to its dynamic nature, since, even if there is randomisation at the time of the inflow into unemployment, there will be dynamic selection bias from the day individuals are aware of the different policy regimes administered to the treatment and control groups. Correcting for dynamic selection

bias is an intrinsic feature of the duration model, provided, of course, that it is correctly specified. Therefore, we devote some attention to developing a fairly flexible empirical model. This implies estimating the model separately for the two counties, since the intensification of labour market policies is handled quite differently by the labour market authorities in the two counties as shown in section 5.

Preliminary results show that the intensification of labour market policies has a large impact, leading to increases in the exit rate from unemployment ranging from 20 to 40%, varying by region and elapsed unemployment duration. This implies a reduction in average unemployment duration of 2.2 weeks (approximately 15%) in both counties. These results are roughly the same as those found by Graversen & van Ours (2008a, 2008b), who were the first to use the data from this experiment.

This paper uses the results found by Graversen & van Ours (2008) as a starting point, and the contributions of this study are the following: first, we provide more detail on the actual treatments administered to treatment and control group members. Specifically, we include information on all programmes attended and all meetings held between the unemployed and their case worker in the treatment as well as the control group. It turns out that meetings are particularly important. Second, we conduct the analysis separately for the two regions, something which is important, given the difference in actual treatments administered in the two regions. Third, we discuss in detail the concept of dynamic selection bias and the assumptions under which more can be learnt from the data generated by the experiment. Fourth, and most importantly, we estimate the risk of participating in an activity (a meeting or a programme) in a given week and include this as an explanatory variable in the equation for the hazard rate from unemployment to employment, in addition to all the indicators of actually receiving some treatment. Finally, we discuss in depth the implication of the results as well as the lessons and policy implications that may be derived from them.

When controlling for treatment group status as well as time-varying indicators for the various specific treatments actually prescribed to the unemployed workers - job search assistance, meetings, programmes etc. - a highly intriguing picture of labour market policy impacts emerges: None of the specific treatments have positive effects, indeed, some of them have large negative lock-in effects, and some even have large negative post-programme effects. When including the estimated 'risk-of-activation' and 'risk-of-meetings' variables, we find that they have a strong positive effect on the job-finding rate, and for one of the counties they 'explain' the entire difference in job-finding rates between treatment and control groups. Moreover, it appears as if the intensification of the meeting frequency is much more important than

the intensification of programme participation rates.

The remainder of the paper is organised as follows; in the next section, we provide a brief and selective survey of the literature on counseling & monitoring and on threat effects, and in section 3 the social experiment is described. Section 4 presents descriptive statistics on the data, and section 5 describes the realised treatments. In section 6, the methodological framework used for the analysis is presented, and section 7 contains our main results and some robustness checks. Finally, conclusions and policy lessons are derived in section 8.

2 The Literature on Counseling & Monitoring and Threat Effects

Crepon *et al.* (2005) evaluate the effects of intensive counseling schemes administered to French unemployed workers. The aim of these schemes is to improve the quality of assignment of workers to jobs. They find that some of these schemes significantly reduce unemployment duration, and moreover they find that they all improve the match quality in the sense that the time until unemployment recurrence is prolonged. Hence, they argue that it is important to investigate the longer term programme effects on employment duration as well as unemployment duration.

Van den Berg & van der Klaauw (2006) estimate the effect of counseling and monitoring on the transition rate to employment. They use data from a social experiment and find no evidence that counseling and monitoring affect the exit rate to work. Moreover, they find that increased monitoring leads to a shift from informal to formal job search.

Cockx & Dejemeppe (2007) use a regression discontinuity design to estimate the threat effect of monitoring the search activity of the unemployed in Belgium. They find that the more employable workers react to the threat of monitoring by finding employment faster. They also look at counseling schemes and find that counseling can have an impact on job-finding rates for some workers.

Kjærsgaard *et al.* (2008) assume random variation in the timing of planned meetings with case workers and find that some types of meetings have threat effects in the sense that job-finding rates increase up to four weeks before the meeting was planned to take place, and moreover, some types of meetings have an effect after the meeting has taken place. This holds especially for meetings where programme participation is planned. These results again suggest the presence of monitoring and threat effects, but also meet-

ings with a counseling content have positive effects suggesting an additional counseling effect.

Hägglund (2006) uses experimental data from three Swedish demonstration programmes to investigate threat effects. He finds that, when applied to a broad group of unemployed workers, a programme consisting of job-search activities in groups combined with increased monitoring led to a two week reduction in unemployment duration through an increase in the exit rate to employment after referral to the programme but before the actual programme start. Increased monitoring alone did not have the same effect. Moreover, similar programmes targeted specifically to the young and the highly educated did not show evidence of threat effects.

For U.S. data, Black *et al.* (2003) find that the unemployed react to the notification of programme participation by letter by increasing their job-finding rate by as much as 50%. Incidentally, this corresponds to a reduction of unemployment duration of 2.2 weeks.

Geerdsen (2006) shows that, for a group of Danish unemployed workers, the exit rate from unemployment rises sharply as they approach the time of mandatory programme participation. He uses legislative changes in the timing of the mandatory programme participation period to identify the threat effect of programmes.

Rosholm & Svarer (2008) use a different identification strategy, namely, they estimate the risk of having to participate in a programme jointly with the job-finding rate (the timing-of-events model of Abbring & van den Berg, 2003), but they also include the programme participation hazard as an explanatory variable in the job-finding rate. They find a significant threat effect of ALMPs, which reduces average unemployment duration by 2.5 weeks. Geerdsen & Holm (2007) use a similar, but not completely identical, technique and find basically qualitatively similar results.

Summarising this selective literature review, there is a tendency towards finding positive effects of counseling and monitoring policies, although the evidence is not unanimous. With respect to threat effects, the evidence seems to be quite unidirectional; there are strong threat effects of monitoring policies as well as active labour market programmes.

3 The Social Experiment

This social experiment in labour market policies was carried out in the counties of Southern Jutland and Storstrøm.[1] Workers eligible for unemployment

[1] Until January 1st, 2007, there were 14 counties in Denmark. On that date, the counties were merged into five regions. The observation period for this study, however, ends before

insurance (UI) benefits who became unemployed during the weeks 43-2005 until 8-2006, that is, essentially from the beginning of November 2005 until the end of February 2006, were randomly assigned into a treatment and a control group, based on their date of birth in the month. Those born on the 1^{st} to the 15^{th} were assigned to the treatment group, while those born on the 16^{th} to the 31^{st} were assigned to the control group. Those in the treatment group were given the following treatment:

1. After approximately 1.5 weeks in open unemployment (that is, during the second week of their unemployment spell) they receive a letter telling them about the new policy regime. They are not told explicitly that it is an experiment with randomisation, but they are told that they are taking part in a 'pilot study' regarding a new labour market policy regime. They are also told about the contents of the new labour market policy regime, which is described below:

2. After 5-6 weeks of unemployment, they shall participate in a two-week Job Search Assistance (JSA) programme.

3. Thereafter, they shall meet frequently with a case worker in order to ensure that they are searching actively and in order to assist them in their job search. In the country of Southern Jutland, meetings will take place every fortnight, and in the county of Storstrøm meetings will take place each week.

4. Those who have not found employment after four months of unemployment have to participate in an unspecified programme of at least three months' duration.

The timeline in terms of unemployment duration for the treatment group is shown graphically in Figure 1. Note that it is not possible to escape treatment by leaving unemployment for a short period. Persons in the treatment group who return to unemployment during the period of the experiment will enter the experiment at the stage where they left it, i.e. if a person left unemployment for a few weeks just before having to participate in a full time programme, his duration clock is not reset when he re-enters unemployment.

< Figure 1 about here >

2007.

In both the treatment and control groups, unemployed workers would have to attend a CV/basic registration meeting within four weeks after becoming unemployed. Persons in the control group are subjected to the ordinary labour market policy, implying that they have to attend meetings with case workers once every three months, and after one year of unemployment they are required to participate in an unspecified programme of unspecified duration. They have the right to participate in a programme of their own choice for up to six weeks during the first year in unemployment. The time span between programmes should hereafter be shorter than six months.[2]

The regions have the possibility to use private contractors for conducting elements of the active policy, and this possibility applies to both treatment and control groups. We shall return to this later.

4 Data

The data used for this study come from the administrative databases used by the case workers to register their dealings with the unemployed workers, from the central register on the labour market (CRAM) that registers UI benefit payments, and various other administrative registers. All data are collected by The National Labour Market Board and made available to the research community. The data contain weekly information on the type of transfer received and the activities undertaken in each week (meetings, programmes, etc.) for each worker. It is thus a very informative and interesting data set.

4.1 Sample Selection

The total inflow to open unemployment in the period between week 43 of 2005 and week 8 of 2006 was 5180 individuals. Some of these are removed from the sample subsequently for various reasons, see Table 1. First, some are not assigned correctly to treatment and control groups based on an inspection of their birthday. Second, some are unemployed due to 'bad weather' and due to 'work-sharing arrangements', and since these categories of unemployed workers are not supposed to take part in the experiment, they are removed from the sample. In the third step, we remove persons who die or emigrate during the observation period, since we do not know the exact date of the event. Finally, in the fourth step we remove individuals who do not receive

[2] The reform of labour market policy in 2003 involved exactly this loosening, where the intensity of the labour market policies is reduced dramatically, from the requirement to participate in programmes 80% of the remaining time in unemployment to the requirement to participate in a programme of unspecified duration every six months.

any UI benefits during the first six weeks of their unemployment spell. This is done because the receipt of UI benefits defines the unemployment spell. Some of these are simply categorised incorrectly by the case worker as UI recipients, although in fact they may receive unemployment assistance or some other benefit, in which case different rules apply to them. The reason for choosing a six-week window is that we want to include persons who have just left education or who have quit their job themselves, and these events involve a five-week sanction period, in which the person may not receive UI benefits.

< Table 1 about here >

The sample selection process, especially the last stage, removes a larger fraction in Southern Jutland county - around 15% - than in Storstrøm county, where only 12% are removed. We are not able to come up with a convincing explanation for this, but there are no remarkable differences in the fractions removed between treatments and controls.

The resulting sample has 4513 individuals, and this is the sample which we shall analyse in the following. For each individual we determine the length of the unemployment spell that triggered their participation in the experiment. The unemployment spell is considered to have ended when the individual has not received UI benefits or some related benefit for more than four consecutive weeks.

4.2 Descriptive Statistics

Besides the information used to construct unemployment spells, the data contain some further information that can be used to construct control variables. Table 2 reports descriptive statistics by region and treatment status. First, we know the calendar week in which the unemployment spell began. As it can be observed, most unemployment spells begin in the start of a month, and there is a particularly large fraction becoming unemployed in the first week of January. The reason that no unemployment spells began in week 51 is that the employment agencies were closed, since this was the week of Christmas.

< Table 2 about here >

Gender and age of each worker is also known, and the latter is used to construct indicators for age intervals. Moreover, we have information about the ethnicity of the worker, and it is used to create indicators for being a native Dane, being an immigrant or descendant originating in a Western country, or being an immigrant or descendant originating in a Non-Western country. The fact that all workers are UI fund members allows us to construct indicators of groups of UI funds, which are considered to be decent proxies for educational attainment (which is unfortunately not available for this sample). Finally, since we have weekly information on public income transfer receipt going back to 1994, we can calculate some past labour market history information. We construct three variables, the first measures the fraction of time spent on public income transfers the 52 weeks immediately before becoming unemployed, the second measures the same fraction 53-104 weeks before becoming unemployed, and the last one does the same for 105-156 weeks before that.

From Table 2 it is seen that there is a difference in unemployment duration between treatment and control groups in both regions, and there is also a difference in the fraction of uncensored unemployment spells. This is the first indication that the treatment has a positive impact in the sense of reducing unemployment duration. However, for the control variables there are no remarkable differences between treatments and controls, although there are minor regional differences. Thus, the sample selection process does not seem to have invalidated the experimental nature of the data.

5 What is The Treatment?

Before going into details with the estimation of the treatment effect, some space is devoted to discussing the treatment itself. Although the treatment appears to be quite precisely defined, there is still some scope for discretion by region and by case worker, and we also have to take into account that the control group outcomes are not the result of non-treatment but rather of a different, less intensive, treatment. We have remarkable data about the precise treatment administered to each individual. For each worker, we know all programmes in which the worker participates as well as the exact timing of these programmes. We also know date and type of each meeting attended. Moreover, we know if the treatment of the worker is being handled by case workers in the employment agency or if it has been contracted out to a private agency.

Most of the following discussion is based on figures and numbers not included in the paper, but these figures are obviously available on request. The

timing of the first CV/basic registration meeting should be fairly equal across the treatments and controls, as all unemployed workers have to attend this meeting during the first four weeks in unemployment. In Southern Jutland, this appears to be the case, and after 10 weeks 97% in the treatment group and 95% in the control group have attended this meeting.[3] In Storstrøm county, this meeting is not held as often, and it is more likely to be held with persons in the treatment group than in the control group. After 10 weeks in unemployment, only 76% among those in the treatment group and 70% in the control group have attended the first CV/basic registration meeting.

The entry rates into the first JSA programme are plotted in Figures 2 and 3. There is a clear difference between treatment and control groups in both counties. In Storstrøm county, control group members receive the treatment much more rarely; indeed, after 12 weeks of unemployment, 84% of those in the treatment group have entered the programme, but only 31% in the control group have done so.[4] However, the treatment is given with some delay compared to the intended timing; after 7 weeks of unemployment, only 53% in the treatment group have begun the treatment. In Southern Jutland, this treatment is not given to control group members at all, or at least only very rarely. After 30 weeks of unemployment, only 1% in the control group has entered a JSA programme. In the treatment group, attendance to this programme is somewhat below what it should have been, as can be witnessed by the programme entry rates in Figure 3. Moreover, after 12 weeks of unemployment only 63% in the treatment group have entered this programme, and those who did enter did so later than intended; after 7 weeks only 16% had begun the programme.

< Figures 2-3 about here >

There are four types of meetings held with the unemployed workers; first, there is the CV/basic registration meeting discussed above. Here, the unemployed worker meets with a case worker and prepares a CV to be published on the 'internet CV-bank', which is a database where employers can find CV's of workers available for work. In 'job-plan' meetings, an action plan for getting back to work is formulated and agreed upon between case worker and client. The agreements include agreeing on programme participation, both

[3] These numbers are based on the estimated survival functions for the time until this type of meeting. They are not shown here, but are available on request.

[4] This information is again based on the estimated survival function for entry into this type of programme. Plots are available on request.

type and approximate dates of entry. Next, there are 'contact-meetings', which are meetings in which the progress and job search strategies of the unemployed worker is discussed. Changes to the agreed action plan can also be made during these meetings. Finally, there are job assignment meetings and job interviews, which are typically spaced sequentially and quite close in time. In the job assignment meeting, the case worker informs the worker about an open vacancy, and it is decided whether the worker should apply for the job. If so, a job interview is organised at the firm with the vacancy. Here, the case worker typically does not participate, but the event is still registered as a meeting.

In Storstrøm country, the 'contact-meeting' rate increases to almost 40% during the period between the first JSA programme and the programme that has to begin around weeks 16-18. The intensity of these meetings continues to be higher in the treatment group than in the control group until week 35. In Southern Jutland county, the increase in the contact-meeting rate for this type of meetings sets in a little later, which probably reflects the later treatment time for the JSA programme. The meeting rate comes close to 30% and it stays above the meeting rate in the control group throughout the entire observation period.

The other types of meetings have lower intensities, but for all of them there is a higher intensity in the treatment group than in the control group in both counties over most of the observed duration of unemployment.

Figures 4 and 5 plot the overall meeting intensity, that is, the fraction of those still unemployed in a given week who attend any type of meeting. This reveals that except in week 4, the meeting intensity is much higher in Storstrøm county than it is in Southern Jutland county. However, in Storstrøm county it is around 0.4, where it should have been 1. Hence, case workers meet with unemployed workers only every 2.5 weeks on average, where they should have done so each week. In Southern Jutland, the meeting rate is around 30% in the period between first and second programme, implying that case worker and client meet every 3 weeks on average, where they should have done so every other week.

< Figures 4-5 about here >

When it comes to programme participation, regions and case workers had some discretion in how to allocate workers to programmes. Basically, they had a selection of 5 types of programmes to choose from; first there were the JSA programmes described above. These consist in determining

the capabilities of the unemployed worker and helping her to search for jobs she would be able to perform.

The second type of programme is 'private sector temporary employment subsidy jobs'. These involve temporary employment in a private firm at the negotiated wage, where the employment agency pays a subsidy to the firm of around half the wage. These jobs typically have a duration of 6 months.

The third type of programme is temporary employment within the public sector. These typically last 6-12 months.

The fourth type of programme is classroom training programmes, which confound anything from IT courses to courses in brick-laying to truck drivers' license courses, but the focus is mostly on classical classroom training courses, hence the denomer. These are typically of a shorter duration, ranging from a few weeks to courses of very long duration, but with the majority being shorter than 3 months.

Finally, there are vocational training programmes in firms. These typically last a few months.

Again, figures for participation intensity in each programme type are available on request but not included due to space considerations. In both regions, 8-12% of unemployed workers in the treatment group participate in 'private-sector temporary employment subsidy jobs' around weeks 20-30, while the same fraction in the control groups is around 4-6%.

With respect to participation in public-sector temporary jobs, some remarkable differences are revealed; In Storstrøm county this type of programme is used for 8-12% of the unemployed workers in the treatment group in weeks 20-30, while the control group only is given this treatment at a rate of 2%. However, in Southern Jutland county this programme is hardly used during the first 30 weeks in unemployment, and to the extent that it is, there is no difference in treatment intensity between treatment and control groups.

Classroom training participation rates also differ widely between the two counties; in Storstrøm county, the participation rate goes up in the treatment group through the entire period, peaking at 40% in week 45. This reflects a steady inflow and fairly long durations in this type of programmes. The participation rate also increases in the control group, but at a much lower level, around 15%. In Southern Jutland county, these programmes are only used about half as often, as the participation rate peaks around 20% for the treatment group, but then the participation rate is also much lower for the control group, peaking at 5%.

As regards vocational training in firms, the control group treatment is quite similar in the two regions, around 1%, while the treatment group is participating in this type of programme more often in Southern Jutland county than in Storstrøm county. However, the participation rates are also

quite low in the treatment groups for this programme.

In Figures 6 and 7, the overall programme participation rates are plotted as functions of unemployment duration, that is, the fraction participating in any programme in a given week among those who are still unemployed in that week. Comparing treatment groups, the programme participation rate is generally higher in Storstrøm county, where it increases earlier and peaks at 45% around week 35, while in Southern Jutland county it peaks at only 35% around week 35. This is disappointing, since the programme participation rate was supposed to be 100% in the treatment group in the weeks 18-30. The fraction treated in the control groups is around 22% in Storstrøm county and around 12% in Southern Jutland county, so the difference in programme participation rates between treatment and control groups is similar in the two regions, but the levels differ.

< Figures 6-7 about here >

In general, the activity level, in terms of meetings and programme participation, is higher in Storstrøm county than in Southern Jutland county, but as shown above, this holds for both treatments and controls. Figure 8 plots the difference in activity rates between treatment and control groups in the two counties, where the activity rate at a given duration is defined as the number of unemployed individuals participating in a meeting or a programme at that duration (week) divided by the total number of unemployed surviving in unemployment at least until that duration. In unemployment duration weeks 3-10, the difference between in treatment intensity between treatment and control groups is much higher in Storstrøm than in Southern Jutland, while in the weeks 11-18 the difference in treatment intensity is lower, but the treatment group still gets relatively most treatment in Storstrøm. In weeks 19-33, the difference in treatment intensity between treatments and controls is about the same in the two regions, and from week 33 and onwards the difference between treatment and control group treatment intensity is larger in Southern Jutland.

< Figure 8 about here >

Finally, we take a look at the extent to which the process of counseling is delegated to private employment agencies in the two regions and for the treatment and control groups. This is described in Figures 9 and 10. It is

seen that in Southern Jutland county, slightly above 80% of the treatment group are handled by private contractors, while this is only the case for approximately 40% of the control group. In Storstrøm county, there are no large differences in the extent to which private contractors are used, the fraction is around 30% in both the treatment and the control group.

< Figures 9-10 about here >

The general pattern thus seems to be that in Storstrøm county, the experiment has been conducted more in line with the experimental protocol, while in Southern Jutland, enforcement has been more lax, and moreover the treatments administered also differ by handling agency in Southern Jutland. However, none of the counties come close to the prescribed treatment intensities in the treatment group. Moreover, there is a difference in the timing and intensity of the initial JSA programme, there is a difference in the meeting intensities, and there is a difference in programme participation intensity and type in the two regions. There are, in essence, two different experiments, one in each region, and they should therefore be analysed separately.

6 Methodology

The aim of the policy experiment is to reduce unemployment duration, that is, by prescribing an early intervention scheme, it is hoped that unemployed workers may be helped and motivated to search more effectively and/or lower their reservation wages. Thus, it would be in the spirit of the policy aim to investigate the impact of the treatment on the duration of unemployment spells. This could be done by comparison of the survival functions for the treatment and control groups, which was done by Graversen & van Ours (2008b). They found clear evidence of a positive treatment effect in the sense that survival rates in the treatment group are much lower than in the control group. However, they did not perform the analysis separately by regions.

By formulating parametric duration models, one would in principle be able to extract more information regarding which of the specific policy instruments applied to the treatment group have an effect and which do not. However, such an analysis would have to deal with specificational issues, with issues of dynamic selection bias, and - given that enforcement of the experimental protocol is not perfect - with endogenous choice of treatments. In order to avoid these issues altogether, one might be tempted to investigate

alternative modeling strategies. However, given the dynamic nature of the various treatments involved, and the necessity to exploit the variation in the timings of these treatments for identification, duration models appear to be the most obvious choice if one wants to extract more information from the experiment than what is revealed by survival functions or employment rates. This implies that we have to deal with the issues mentioned above. In this section, we will discuss how to deal with them one by one. We start by formulating the fundamental evaluation problem in a duration model context. Then we proceed to discuss distributional issues and dynamic selection bias, and finally we turn to the issue of endogeneity of the instrument package administered to a given individual in the treatment and in the control group.

6.1 The Evaluation Problem

The outcome of interest is T, the duration of unemployment spells. Denote potential outcomes in the treatment state and non treatment state by T_1 and T_0, respectively, and define the treatment indicator D, where a value of 1 indicates receipt of treatment. Treatment in the present case denotes eligibility for the entire package administered to those in the treatment group, that is, the treatment is the new, more intensive, labour market policy regime. In the sense of Abbring & van den Berg (2005), randomization essentially takes place at the time of inflow to unemployment, treatment starts immediately, and there is full compliance. In that respect, the treatment is the same as the intention to treat. We can specify the average treatment effect on the expected duration of unemployment as

$$\Delta = E[T_1 - T_0]$$

Given our definition of the treatment, there is a one-to-one correspondence between randomisation into the treatment group and actual receipt of the treatment, this parameter is equal to the treatment effect on the treated, since the treated constitute a random sample from the inflow to unemployment. Hence,

$$\Delta = E[T_1 - T_0|D = 1]$$

Given randomisation, the unobserved counterfactual can be replaced by the expected outcome in the control group, that is,

$$\Delta = E[T_1|D = 1] - E[T_0|D = 0]$$

Now, for several reasons it is not straightforward to estimate Δ, firstly due to right-censoring, and secondly, as shown below, due to dynamic sample

selection effects. Hence, we formulate treatment effects also in terms of relative escape rates from unemployment. At time t, the hazard (escape) rate in the treatment and non-treatment states are $\theta_1(t)$ and $\theta_0(t)$, respectively, and the treatment effect at time t on the hazard rate is

$$\Delta_\theta(t) = \frac{\theta_1(t)}{\theta_0(t)}$$

6.2 Specification Issues and Dynamic Selection Bias

Suppose workers differ by observed and unobserved characteristics, X and V, respectively. Suppose V are random draws from the distribution $F_V(\cdot)$. Without loss of generality, assume for the moment that all characteristics are unobserved to the researcher. At duration times 0 and 1, the hazard rates are still the same in the treatment and control groups, since they have the same information set. Formally,

$$\begin{aligned} \theta_1(0) &= \theta_0(0) \\ \theta_1(1) &= \theta_0(1), \end{aligned}$$

where the subscript on the hazard rates indicates treatment status. In general, however, this equality does not hold for values of t larger than 1, since in the second week of unemployment, individuals in the treatment group are provided with information about the new labour market policy regime (and hence the treatment starts), and that information may immediately affect their behaviour. Let the hazard rates out of unemployment be

$$\theta_i(t|V) = \lambda_i(t)V, \qquad i = 0, 1,$$

that is, they are mixed proportional hazard rates. In this case, the value of the observed hazard rate at time $t \geq 2$ is equal to

$$\begin{aligned} \theta_i(t) &= E_V\left[\theta_i(t|V)|T_i \geq t\right] \\ &= \lambda_i(t) \cdot E_V[V|T_i \geq t] \end{aligned}$$

since the expectation has to be taken over those still unemployed at time t. The expression

$$E_V[V|T_i \geq t] = \int V \cdot dF_V(V|T \geq t)$$

depends on the distribution of V conditional on survival in unemployment at least until t. This distribution can be derived in the following manner:

$$F(T_i \geq t, V) = F_V(V|T_i \geq t) \cdot P(T_i \geq t),$$

and by a straighforward application of Bayes' rule, we obtain the following

$$\begin{aligned} F_V(V|T_i \geq t) &= \frac{F(T_i \geq t, V)}{\Pr(T_i \geq t)} \\ &= \frac{\Pr(T_i \geq t|V) \cdot F_V(V)}{\int \Pr(T_i \geq t|V) \cdot dF_V(V)} \end{aligned}$$

It is clear that as long as $\theta_i(\cdot)$ does not depend on i, there is no problem, but when information about the experiment arrives (in the form of a letter) to the nemployed workers, the experiment starts affecting the hazard rates of individuals in the treatment and control groups differently, and therefore the conditional distribution $\Pr(T_i \geq t|V)$ depends on i. This phenomenon is known in the literature as 'dynamic sample selection', and it implies that, although the distribution of unobservables is identical in the treatment and control groups at the time of randomization, $T = 0$, once the experiment starts affecting the exit rates from unemployment, the distribution of unobservables among the survivors in unemployment will start to differ between the treatment and control groups. The issue of dynamic selection in duration model contexts is treated in detail by Abbring & van den Berg (2005).

Hence, an identification strategy must be able to account for dynamic selection bias. This is exactly the strength of the duration model framework, where the hazard rates - the selection process out of the state of interest - are explicitly modeled.

We shall begin by making an assumption of a mixed proportional hazard (MPH) model, that is, the hazard rates can be written in the following form

$$\begin{aligned} \theta_1(t|X, V) &= \lambda_1(t) \cdot \exp[f_1(X)] \cdot V \\ \theta_0(t|X, V) &= \lambda_0(t) \cdot \exp[f_0(X)] \cdot V \end{aligned}$$

We further assume that $f_1(\cdot) = f_0(\cdot) = f(\cdot)$. This is of course a restrictive assumption, but one that is in the spirit of the MPH assumption. It implies that the treatment affects the baseline hazard, but does not affect the way in which observed explanatory variables affect the hazard rates. The observed hazard rate can now be written in the following form:

$$\theta(t|x, d, v) = \lambda_0(t) \cdot \exp[f(x) + d\delta_\theta(t)] \cdot v,$$

where

$$\delta_\theta(t) \equiv \ln \lambda_1(t) - \ln \lambda_0(t)$$

This duration model nonparametrically identifies the time-varying treatment effect on the hazard rate, given the mixed proportional hazards assumption and the randomised treatment.

If one wants to proceed non-parametrically, Abbring & van den Berg (2005) show that with the assumption of proportional hazards and an additional assumption of monotonicity of the effect of treatment on the relative hazard - e.g. the treatment effect is positive (negative) at all durations - the difference in the empirical hazard rates constitute a lower (upper) bound on the true treatment effect.

6.3 Inference Regarding Specific Treatments

A certain package of treatments is administered to each individual in the treatment group, and these treatments are to some extent perceived as being exogenous. However, some do not even participate in the initial JSA treatment, although they have unemployment spells that are long enough that they should have participated. Moreover, the meetings intensities are lower than that which was intended, and the full-time programme participation from week 16-18 onwards is not enforced perfectly, either. This leads to the realisation that the specific treatments in the treatment groups (as well as in the control groups, where case workers and unemployed workers have more discretion) may be endogenous, even if the assignment to the treatment group is purely exogenous, rendering causal inference regarding their impacts tricky at best.

Let the specific time-varying treatments administered to the treatment group be captured by the (time-varying) vector of indicators $D_1(T)$, and define specific treatments in the control group by $D_0(t)$. We now have the following model

$$\theta(t|x, d, d_0(t), d_1(t), v) = \lambda_0(t) \cdot \exp[f(x) + d\delta_\theta(t) + d_1(t)\delta_1(t) + d_0(t)\delta_0(t)] \cdot v$$

Note that we allow for the possibility of a treatment effect over and above the sum of specific treatment effects, which is possible if the sum of specific treatment impacts does not add up to the total treatment effect, which could be the case if e.g. specific treatments have 'ex ante' impacts, or if the knowledge of being in the treatment group affects individual behaviour, e.g. through awareness of the increased intensity of monitoring and programme participation requirements.

If we estimate this model with maximum likelihood, not taking into account the endogeneity of specific treatments, the estimated parameters may be biased. However, to the extent that there is random variation in the timing of the treatment, the 'timing-of-events' specification of Abbring & Van den Berg (2003) can be used to identify treatment effects for those in the treatment groups as well as for those in the control groups. This requires

separate estimation of the time until treatment and the type of treatment for individuals in treatment and control groups. Given the number of treatments in the package and the presence of repeated treatments (meetings), this strategy seems infeasible. Instead, our strategy shall be to estimate the parameters in the model above, conditioning on a large selection of explanatory variables, including past labour market histories, so that a conditional independence assumption may be invoked to identify specific treatment effects. This is obviously a short-cut, but given the infeasibility of the timing-of-events specification, and given the lack of access to instrumental variables, the strategy chosen produces impact estimates that are valid under an assumption of conditional independence, the validity of which can be judged by inspection of the included observed explanatory variables.[5] To the extent that a bias may remain, we shall discuss the likely direction of it. A simple logit model, estimated only for members of the treatment group, for participating in an activity (defined as a meeting or a programme) in a given week as a function of duration, starting week, and the same set of background variables as those used in the models specificed below reveal that men are less likely to participate in an activity, persons with more past unemployment are less likely to participate, as are the oldest workers in the sample, while the young are much more likely to participate. Members of certain UI funds (construction) are much more likely to participate. Hence, it *is* necessary to invoke a conditional independence assumption, as an unconditional independence assumption is certainly violated.[6]

7 Results

7.1 Non-parametric analyses

Figures 11 and 12 show Kaplan Meier survival functions in unemployment for unemployed workers in Storstrøm and Southern Jutland county, respectively. Confidence bands are calculated from the standard errors suggested in Lancaster (1990). The figure for Storstrøm county shows that the survival functions start diverging almost from the first week in unemployment, and the confidence intervals are non-overlapping from around week 7 in un-

[5] In the context of Abbring & van den Berg (2005), the randomization indicator itself is an instrumental variable. However, this precludes the case in which knowledge of the experiment can have ex ante effects, hence, in our case, using randomization as an IV would not identify the specific treatment effects due to dynamic selection taking place before the actual treatment.

[6] These results are not reported in the paper but are available on request from the author.

employment. Half of the unemployed workers in the treatment group have left unemployment after 11 weeks of unemployment, while the halving time for the control group is 13 weeks. At 18 weeks of unemployment - when programme participation should ideally be administered full time to every one in the treatment group - there are 28% left in unemployment (including programme participation) in the treatment group versus 37% in the control group. After 32 weeks the confidence intervals around the survival functions start overlapping again. For Southern Jutland county, the survival functions do not start diverging until week 6 in unemployment. After 10 weeks in unemployment the confidence bands are non-overlapping, and they continue to be non-overlapping for the remainder of the period in unemployment until the 40th week. In the treatment group, the halving time is around 11-12 weeks, while in the control group it is around 13-14 weeks, i.e. approximately the same as in Storstrøm county. After 18 weeks in unemployment, there are 31% left in unemployment in the treatment group compared to 39% in the control group. This is for both groups slightly 2-3 percentage points more than in Storstrøm county. For both counties, the Integrated survival functions suggest a reduction in the average duration of unemployment over the first 46 weeks of slightly more than 2.2 weeks, in Storstrøm county from 16.4 to 14.2 weeks, and in Southern Jutland from 17.2 to 15.1 weeks.

< Figures 11-12 about here >

Figures 13 and 14 show the corresponding empirical exit rates from unemployment. It is seen that, in both regions, the raw exit rates out of unemployment are higher in the treatment group than in the control group at least until week 20. However, these results are likely to understate the 'true' effect of the treatment due to dynamic selection bias; as members of the treatment group leave unemployment faster, there is the possibility that exit is selective with respect to unobservables (and all explanatory variables are unobserved for the Kaplan-Meier estimator). Indeed, as mentioned above, under certain assumptions, the Kaplan-Meier hazard rates constitute a lower bound for the true treatment effect.

< Figures 13-14 about here >

In conclusion, it is apparent that the treatment, defined as the labour market regime administered to those in the treatment group, has reduced

unemployment duration by increasing the exit rates from unemployment, at least in some duration intervals. What remains is to see whether we can determine which of the specific treatments, if any, have contributed to the overall effect, and whether the overall effect can be attributed to specific duration intervals, once we try to eliminate dynamic selection bias.

7.2 Parametric models

All models were estimated by including a selection of background characteristics and allowing for unobserved heterogeneity. Since the fraction of survivors in unemployment is fairly low, the maximization of the log likelihood invariably ends up preferring a specification with two unobserved 'types', one small group (1-2% of the inflow into unemployment) of 'immune'persons with a zero exit rate from unemployment, and the other a large group with a positive exit rate from unemployment. The coefficients to observed explanatory variables do not change very much between model specifications, therefore they are only reported in Appendix Table A1 for a basic specification called Model 1 below, and we shall not spend time discussing each coefficient here.[7] The general pattern is that men have higher exit rates from unemployment than women, the exit rates decline with age in general, and those with more dependence on public income transfers in the past have lower exit rates.

As a benchmark, we first estimate the model with a completely free specification of the baseline and the treatment effect. In essence, this corresponds to estimating separate baseline hazard functions for treatments and controls, and the treatment effect is then the difference between the two. These estimates are presented in Figure 15, where the multiplicative weekly treatment effects have been smoothed using a centered three-week moving average. A couple of interesting patterns emerge; first of all, there is a positive treatment effect in both counties, and in the duration interval of the experiment (weeks 2-30), those in the treatment group have a 20-40% higher exit rate from unemployment than those in the control group. This result basically holds in both counties, although there are some differences. Interestingly, the effect early in the unemployment spell appears to be larger in Southern Jutland (where the activity rates were lower). From weeks 10-26, the effect is approximately of the same size in the two counties, but from week 26, the effect becomes significantly negative in Storstrøm county, while it continues to be positive in Southern Jutland.

[7] All detailed parameter estimates are of course available on request from the author.

< Figure 15 about here >

Table 3 reports results from two different specifications of a parametric duration model, one where we estimate an average treatment effect, called Model 1, and one where we allow the treatment effect to vary with the duration of unemployment, which we call Models 2. The intervals for the treatment effect in Model 2 have been set so as to allow for changes in treatment effects according to the progression in the experiment, cf. Figure 1.

The results in Table 3 thus represent estimates of the effect of being in the new treatment regime. In both regions, there is a significant positive time-invariant treatment effect of about the same size, corresponding to a 20-22% increase in the exit rate from unemployment. In Model 2, it can be seen that the treatment effect is large during the weeks 5-15 of unemployment duration in both counties, but, as was also observed in figure 15, it is largest in Southern Jutland county. In the weeks 16-22 it stays large in Storstrøm, while it starts withering off in Southern Jutland. After the 22nd week of unemployment, there is no longer a significant treatment effect.

< Table 3 about here >

Table 4 shows Model 3, in which we still have time-varying indicators for belonging to the treatment group, as in Model 2, but now we also include time-varying indicators of programme participation and completion, and attendance of meetings in the current and in past weeks. These indicators are included for both treatment and control groups members, and the effects of these specific treatments are allowed to vary between treatment and control groups. It is seen that none of the specific treatments administered have positive impacts in general. However, the time-varying indicators for belonging to the treatment group are still positive and significant for the same intervals as in Model 2. Some of them are even larger in this specification, as if to compensate for the negative effect of the specific treatments.

< Table 4 about here >

These results indicate that it is not the actual meetings and programme participation periods that affect the individuals' job-finding rates. This is

somewhat surprising given the relatively large treatment regime effect estimated in Model 2. One possible explanation for the positive treatment effect of the overall programme is that there is a 'threat effect' of the treatment regime itself; what generates the positive treatment effects are the perceptions of the more intensive labour market policy regime; the knowledge that in the future you will be monitored weekly and placed in programmes continuously apparently induces either more active job search or lower reservation wages. Alternatively, it may be the specific knowledge that you have to participate in some specific event that triggers the increase in job-finding rates.

Unfortunately, we cannot measure the impact of future events (planned meeting and programme participation periods) directly, as we have only information on meetings and programmes that actually took place. That is, meetings that are cancelled because the person found a job the week before the planned meeting are not available in this data set.[8] Hence, the direct effect of announcements of meetings and programme participation periods cannot be estimated from the available data.

However, we can estimate the weekly likelihood of participating in some activity; the activity rate, and then we can include the activity rate as an additional explanatory variable in the duration model. The activity rate is 0 in the weeks when activities are taking place, because in these weeks, the effect of participating are captured in the coefficients to the 'in programme' indicators. This approach is similar in spirit to that used by Rosholm & Svarer (2008). If significantly positive, the coefficient on the activity rate captures the effect that individuals who - in a given week - are more at risk of having to participate in some activity are more likely to leave unemployment.

Note that the activity rate is not perfectly collinear with the set of included explanatory variables in the duration model, because of the interaction it entails between the explanatory variables and the duration of unemployment; since the activity rates vary from week to week, and across individuals, this interaction identifies its coeficient in the duration model. For further discussion, see Rosholm & Svarer (2008).

In Table A2 in the Appendix, we report the results from a logit model for participating in any activity, meeting or programme, in a given week, for each region. In the logit model, the dependent variable takes the value 1 for an individual in a given week, if the person attends a meeting or participates in a programme in that week. The activity rates are plotted in Figure A1

[8] Kjærsgaard et al. (2008) employ another data set and provide a more general analysis of meetings and monitoring regimes in a non-experimental setting.

in the appendix (these are also the activity rates underlying the differences that were plotted in Figure 8). As explanatory variables, we include all the background information included in the duration models above as well as weekly duration indicators and treatment group indicators, and we allow for time-varying treatment regime parameters as in the duration model. The results indicate that allocation to activities is not completely random; most of the variation in activity comes from the treatment group (since their activity rates are much higher than in the control group), and including interactions between belonging to treatment and control groups and other explanatory variables does not dramatically alter these results). For example, the young and Non-western immigrants are more likely to participate in some activity, while in Southern Jutland, those with a lot of past unemployment are more likely to be in some activity at any point in time. In Storstrøm, there is a tendency towards the opposite. There is also quite a bit of variation between activity rates between different UI funds, for example academics participate much more in activities in Southern Jutland, while white collar workers participate much less, while in Storstrøm technicians and construction workers have high activity rates.

As mentioned above, the estimated activity rate constitutes an independent random variable when compared to the variables included in the duration model, because it consists of (a nonlinear function of) an interaction between a linear function of the observed explanatory variables and a function of unemployment duration. Hence, to include the estimated activity rate into the duration model does not lead to identification problems, the model is still non-parametrically identified.[9]

In Table 5, we report the results from the estimation of a duration model, where the estimated activity rate is included in the model in weeks when the individual is not participating in some activity. When the individual is participating in an activity, this is captured by time-varying indicators as in Model 3, Table 4 above. Interestingly, the parameter on the estimated activity rate is positive and statistically significant in both regions, although it is much larger in Southern Jutland county than in Storstrøm. Moreover, inclusion of the estimated activity rate renders the treatment regime effect insignificant in all time periods in both regions. In Southern Jutland, the positive coefficients on the indicator for belonging to the treatment group disappear entirely, suggesting that we have indeed identified an important mechanism

[9] The model is similar in spirit to that applied by Rosholm & Svarer (2008) to estimate the threat effect of active labour market programmes. They estimated the rate of entry into programmes as a measure of the 'threat'. In the present context with frequent meetings and repeated events, the activity rate seems a more appropriate measure of the 'risk' of an activity.

triggering the difference between the treatment and control group, at least in Southern Jutland. In Storstrøm, the conclusion is qualitatively in the same direction, but the evidence is not quite as compelling as it is in Southern Jutland.

< Table 5 about here >

Finally, in Table 6, we include an estimated meetings intensity and programme participation rate separately, and include both into the duration model. These are also estimated in logit models, the results of which are available on request. The main parameters from the estimation of this model are reported in Table 6.

< Table 6 about here >

These results indicate that the experiment has indeed been carried out quite differently in the two regions, with threat effects being much more pronounced in Southern Jutland than in Storstrøm county. In Storstrøm county, only the estimated meetings intensity has a significantly positive effect on the exit rate from unemployment rate, while the estimated programme participation rate is negative but not statistically significant. In Southern Jutland county, both meetings intensities and programme participation rates have large and statistically significant positive effects on the exit rate from unemployment. Hence, it appears in general as if meetings have larger threat effects than programmes, which is quite interesting, since it indicates that the monitoring part of counseling and monitoring programmes is the most important one. Moreover, in Southern Jutland, this model implies a significantly negative difference between treatment and control groups in weeks 19-22 after the taking into account actual programme and meeting as well as measures of the treatment regime intensity. This is probably because some individuals have a large probability of participating in both meetings and programmes in a given week. Hence, to include both risk measures may lead to a correction which in some sense is too large if only the risk of participating in 'some activity' is relevant.

To sum up, we find that the intensification of the labour market regime has led to a fairly large reduction in unemployment duration of around 2.2 weeks (15%). The actual meetings held and programmes administered to the unemployed workers do no not explain away the difference between the

treatment and control groups, but upon including a measure of the intensity of the treatment regime, the difference between treatment and control groups tend to disappear. Finally, meetings appear to be a more than perfect substitute for programme participation in terms of generating threat effects.

8 Conclusion

This paper uses a social experiment in labour market policy - providing early and intensive monitoring and programme participation in unemployment spells - to assess the nature of labour market policy effectiveness.

The experiment was conducted in two counties in Denmark during the winter of 2005-6. The treatment consisted of a dramatic intensification of labour market policies, involving information, very early mandatory participation in job search assistance programmes, frequent meetings with employment officers, and full time programme participation for at least three months if they had not found employment before 18 weeks of unemployment.

Inspection of the survival functions shows a dramatic treatment effect in both counties. However, the fact that the treatment consists of complex combinations of meetings and different types of programmes placed differently in time means that it is difficult to disentangle the impacts of each treatment separately, but conditioning on a large set of explanatory variables and exploiting the variation in timing of specific activities allows us to identify separate treatment effects in a duration model framework.

We find interesting results; first, the intensification of labour market policy is highly effective, leading to increases in the exit rate from unemployment ranging from 20-40%, varying by region and elapsed unemployment duration.

Second, when introducing time-varying indicators for the various specific treatments actually prescribed to the unemployed workers - job search assistance, various types of meetings, and various programmes - none of those treatments have a positive effect on the exit rate from unemployment, neither during the week in which the activity takes place, nor after the activity is completed; indeed, some of them have large negative lock-in effects, and some even have large negative post-programme effects.

However, the estimated risk of participating in an activity in a given week has a strong positive effect on the job-finding rate. When the estimated risk of participating in an activity is included, it explains away the difference in job-finding rates between treatment and control groups completely in one of the counties, and reduces it dramatically and renders it insignificant in the other county.

The interpretation we attach to these results is the following; since indi-

vidual treatments do not appear to be effective *per se*, but the risk of treatment is, it must be that the intensification of the policy *regime* increases the job-finding rate of unemployed workers. It is not a composition effect, since the workers were not aware of the policy intensification before becoming unemployed. Hence, the prospects of being constantly monitored as a consequence of attending frequent meetings, and of having to participate in programmes after 5 weeks of unemployment and again after 18 weeks, acts by way of increasing the search activity or lowering the reservation wage of unemployed workers, and this indirect effect strongly dominates the negative effects of actually participating in the activities. This effect we label a threat effect; namely, the threat effect is driven by the perceived risk of having to attend some activity, and the treatment effect we found in this study is precisely generated by the perceived intensification of monitoring and programme participation prospects.

The experiment points to a more general observation on the effectiveness of active labour market policies; despite the variation in the implementation of them throughout the world, and despite their verbalized effectiveness and political popularity in e.g. the Danish Flexicurity model, econometricians have generally failed to estimate large positive programme effects. However, the threat effect, as documented by several studies as well as the present one, suggests that meetings and programme participation act as a 'tax' on unemployed individuals' leisure time. By taxing away some of the unemployed workers' time - either directly through the requirement to participate in programmes, or indirectly by monitoring the individuals' job search effort at frequent meetings - the marginal utility of spending time in unemployment falls, and hence, unemployment spells are shortened due to increased search activities or lowered reservation wages. The threat effect materializes the moment the individual realizes that there is a positive risk of having to attend more meetings and participate in programmes in the future, and when that risk becomes sufficiently large, it may significantly affect the search behaviour of the individual.

It thus seems that an intensive labour market policy regime, such as that which is already in place in Denmark - the experiment is an intensification of a policy regime which was already quite intensive by international standards - consisting of early monitoring and job search assistance combined with mandatory programme participation after some elapsed time in unemployment - can reduce average unemployment duration significantly, and thus contributes to alleviating one of the perceived weaknesses of the Flexicurity model; namely the reduced incentive to conduct job search, which is generated by the generous unemployment insurance benefits. This confirms the general impression that active labour market policies constitute an impor-

tant part of the Flexicurity model, not because they 'ensure the availability and the qualificational level of the work force' (the stated intention in the law), but rather because they reduce the utility derived from having leisure as well as generous income transfers.

The Danish Economic Council (2007) has conducted a cost-benefit analysis of the intensification of the labour market policy regime conducted in the experiment, using detailed cost information, and calculating as benefits the reduction in UI benefit payments multiplied by the price of providing public funds (tax distortion) and the increased value of production rsulting from reduced unemployment duration. They ignore general equilibrium effects, but find nevertheless that the programme has a value to society of €2000 *per unemployed individual exposed to the intensive policy regime.* This suggests that an even more intensive programme than the one employed in the Flexicurity model today might be beneficial. However, to arrive at such a recommendation more forcefully, one would have to investigate likely general equilibrium effects of such an intensified regime. This is beyond the scope of the present paper.

The present study only looks at the effect of the labour market policy regime on unemployment duration, but subsequent employment duration may also be affected. At the moment, we do not have data for employment duration, since such data come from several other administrative data sets, which are not available without a considerable time delay. We plan to carry out such an analysis in the future.

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Table 1. Sample selection

	Storstrøm County		Southern Jutland County		Full sample
	Treatment	Control	Treatment	Control	
Total inflow	1313	1374	1229	1264	5180
Step 1	1306	1371	1226	1252	5155
Step 2	1293	1360	1219	1240	5112
Step 3	1283	1347	1206	1225	5061
Step 4	1174	1219	1055	1065	4513

Table 2. Descriptive statistics

	Storstrøm county		Southern Jutland County	
	Treatment	Control	Treatment	Control
Unemployment duration	14.80	16.72	15.63	17.52
Fraction uncensored	0.86	0.82	0.85	0.79
Starting week:				
43, 2005	0.03	0.02	0.05	0.03
44, 2005	0.10	0.09	0.10	0.11
45, 2005	0.03	0.03	0.02	0.04
46, 2005	0.03	0.03	0.03	0.03
47, 2005	0.02	0.03	0.03	0.03
48, 2005	0.06	0.08	0.09	0.09
49, 2005	0.07	0.04	0.04	0.04
50, 2005	0.05	0.04	0.05	0.03
52, 2005	0.11	0.11	0.11	0.11
1, 2006	0.16	0.17	0.15	0.15
2, 2006	0.04	0.04	0.04	0.05
3, 2006	0.04	0.03	0.03	0.04
4, 2006	0.08	0.08	0.06	0.06
5, 2006	0.10	0.11	0.10	0.10
6, 2006	0.03	0.04	0.04	0.03
7, 2006	0.04	0.03	0.01	0.03
8, 2006	0.02	0.03	0.03	0.03
Gender				
Male	0.62	0.60	0.54	0.54
Female	0.38	0.40	0.46	0.46
Age group				
Less than 24	0.09	0.09	0.11	0.13
25-29	0.13	0.12	0.14	0.14
30-39	0.24	0.27	0.26	0.24
40-49	0.26	0.24	0.26	0.25
50-59	0.25	0.25	0.20	0.22
60 and older	0.03	0.03	0.03	0.04
Ethnicity				
Danish	0.92	0.95	0.92	0.93
Western Immigrant	0.02	0.02	0.05	0.03
Non-western Immigrant	0.05	0.03	0.04	0.04
Unemployment Insurance Fund				
Construction workers	0.07	0.07	0.08	0.07
Metal Industry	0.06	0.05	0.05	0.05
Manufacturing	0.42	0.41	0.34	0.36
Technicians	0.03	0.05	0.03	0.03
Trade	0.11	0.11	0.08	0.10
White collar	0.10	0.10	0.10	0.10
Academics	0.03	0.02	0.03	0.03
Others	0.16	0.15	0.22	0.20
Self-employed	0.03	0.04	0.07	0.05
Past Public Income Transfer Rate				
Last year	0.25	0.24	0.23	0.24
1-2 years ago	0.31	0.29	0.30	0.29
2-3 years ago	0.32	0.30	0.32	0.31

Table 3. Treatment *regime* effects

	Storstrøm		Southern Jutland	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Model 1				
	0.18	**0.05**	**0.20**	**0.05**
Model 2				
Weeks 1-2	0.09	0.14	0.10	0.15
Weeks 3-4	0.20	0.13	-0.08	0.15
Weeks 5-9	**0.23**	**0.10**	**0.36**	**0.10**
Weeks 10-15	*0.18*	*0.10*	**0.27**	**0.11**
Weeks 16-18	**0.38**	**0.15**	0.19	0.16
Weeks 19-22	*0.29*	*0.17*	0.09	0.18
Weeks 23-30	-0.08	0.17	0.12	0.17
Weeks 31+	-0.37	0.28	0.39	0.27

Note: Numbers in bold are statistically significant at the 5% level, and numbers in italics are statistically significant at the 10% level.

Table 4. Treatment *regime* effects and specific treatment effects

	Storstrøm				Southern Jutland			
	Treatments		Controls		Treatments		Controls	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Treatment Group Indicators								
Weeks 1-2	0.07	0.15			0.08	0.15		
Weeks 3-4	0.13	0.15			-0.10	0.15		
Weeks 5-9	*0.20*	*0.12*			**0.42**	**0.14**		
Weeks 10-15	0.17	0.14			**0.38**	**0.14**		
Weeks 16-18	**0.45**	**0.18**			0.31	0.20		
Weeks 19-22	**0.46**	**0.20**			0.28	0.21		
Weeks 23-30	0.19	0.21			0.31	0.20		
Weeks 31+	-0.01	0.31			*0.53*	*0.31*		
Specific Treatment Effects								
Meetings								
CV Meeting during past 2 weeks	-0.14	0.13	-0.28	0.17	**-0.66**	**0.16**	**-0.42**	**0.18**
Job Plan Meeting during past 2 weeks	0.09	0.10	-0.05	0.12	-0.13	0.13	-0.04	0.16
Contact Meeting during past 2 weeks	-0.15	0.25	-0.16	0.30	**-0.99**	**0.47**	0.08	0.59
Job Assignment Meting past 2 weeks	-0.17	0.27	**0.48**	**0.27**	0.18	0.27	0.49	0.36
Cumulative number of meetings past 4 weeks	0.04	0.05	0.05	0.07	0.00	0.08	-0.06	0.10
In Program Indicators								
JSA	-0.02	0.13	-0.10	0.22	**-0.66**	**0.21**		
Education	**-0.85**	**0.31**	**-1.20**	**0.41**	**-1.15**	**0.39**	**-1.07**	**0.46**
Private Sector Empl. Subs	**-1.15**	**0.38**	-0.80	0.55	-1.10	0.90	-1.22	1.17
Public Sector Temp. Job	**-0.96**	**0.34**	-0.83	0.72	**-1.07**	**0.36**	**-1.61**	**0.75**
Vocational Training Program	**-0.94**	**0.43**	-0.49	0.48	-0.20	0.26	*-1.46*	*0.84*
Completed Program Indicators								
JSA	-0.10	0.10	0.01	0.10	0.04	0.11	0.72	1.45
Education	-0.13	0.32	-0.20	0.32	0.23	0.29	0.32	0.37
Private Sector Empl. Subs	-1.44	0.97						
Public Sector Temp. Job	-3.88	44.20	0.48	0.92	-0.37	0.65	-0.10	0.71
Vocational Training Program	0.04	0.26	0.24	0.42	0.28	0.22	0.47	0.39
Other Activities								
Assignment Meeting	0.09	0.46	0.75	0.46	0.15	0.56	0.50	0.46
Job Interview	**0.59**	**0.35**	0.38	0.34	-0.83	0.62	0.25	0.46
Assignment to Private Agency	-0.08	0.11	-0.09	0.10	*-0.21*	*0.12*	**-0.25**	**0.10**

Note: Numbers in bold are statistically significant at the 5% level, and numbers in italics are statistically significant at the 10% level.

Table 5. Treatment *regime* effects, and the effect of the activity rate

	Storstrøm		Southern Jutland	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Treatment Group Indicators				
Weeks 1-2	0.06	0.14	0.07	0.15
Weeks 3-4	0.08	0.15	-0.08	0.15
Weeks 5-9	0.05	0.13	0.18	0.14
Weeks 10-15	0.03	0.14	-0.03	0.15
Weeks 16-18	0.30	0.18	-0.09	0.21
Weeks 19-22	0.29	0.19	-0.23	0.22
Weeks 23-30	-0.01	0.21	-0.14	0.21
Weeks 31+	-0.18	0.32	0.06	0.31
Estimated activity rate	**0.44**	**0.17**	**1.62**	**0.25**

Note: Numbers in bold are statistically significant at the 5% level, and numbers in italics are statistically significant at the 10% level.

Table 6. Treatment *regime* effects and the effects of program participation rates and meetings intensitites

	Storstrøm		Southern Jutland	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Treatment Group Indicators				
Weeks 1-2	0.07	0.14	0.06	0.15
Weeks 3-4	0.10	0.15	-0.08	0.15
Weeks 5-9	0.23	0.21	0.11	0.17
Weeks 10-15	0.08	0.16	-0.21	0.16
Weeks 16-18	*0.38*	*0.20*	-0.32	0.22
Weeks 19-22	*0.41*	*0.23*	**-0.60**	**0.25**
Weeks 22-30	0.16	0.25	*-0.44*	*0.24*
Weeks 31+	0.00	0.34	-0.21	0.34
Estimated meeting intensity	**0.47**	**0.23**	**2.30**	**0.32**
Estimated program participation rate	-0.51	0.73	**1.73**	**0.66**

Note: Numbers in bold are statistically significant at the 5% level, and numbers in italics are statistically significant at the 10% level.

Figure 1. A Duration Timeline for The Social Experiment

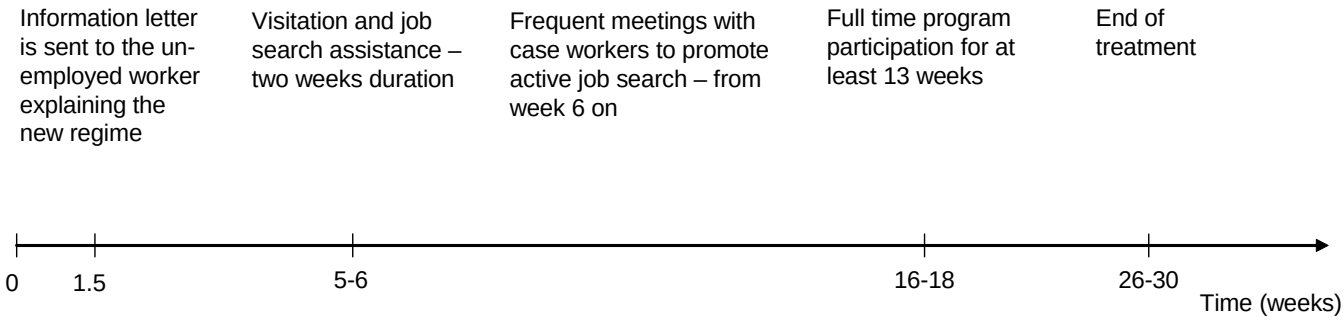


Figure 2. Entry rate into job search assistance/visitation program, Storstrøm county

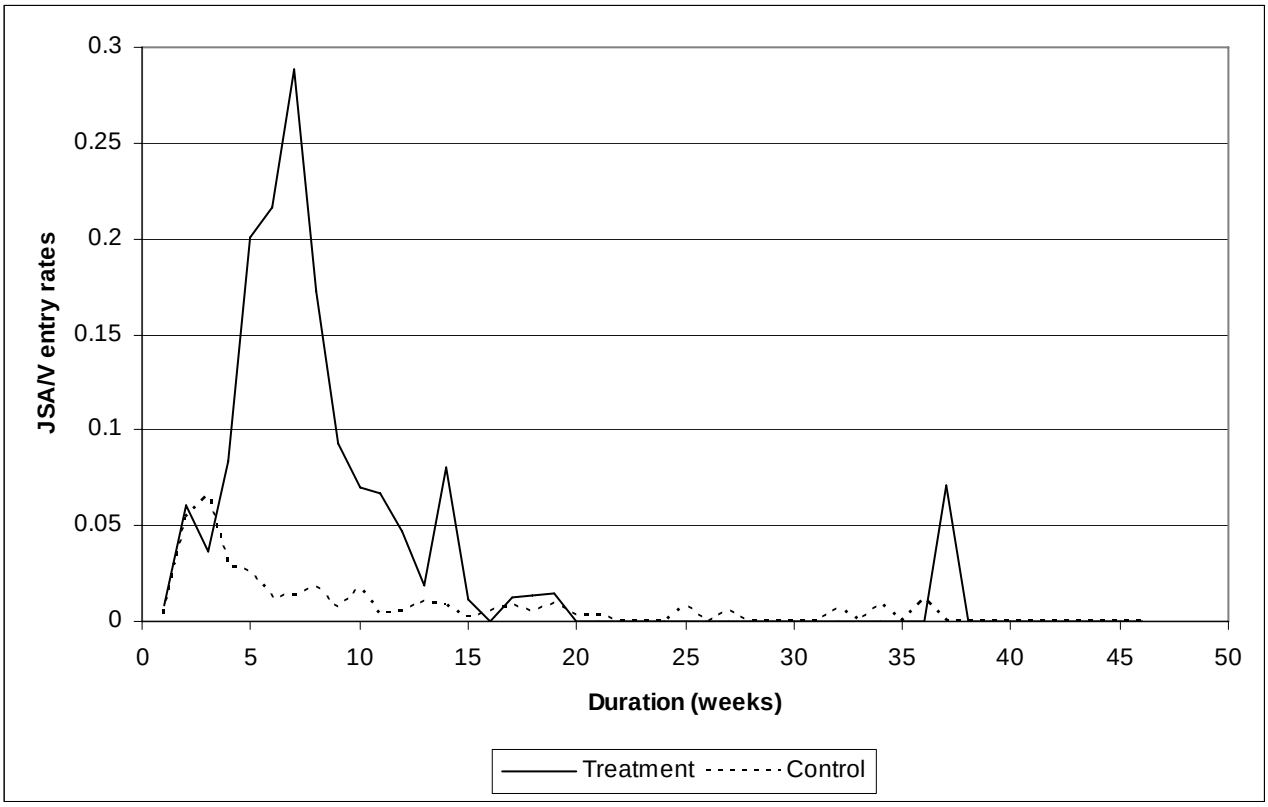


Figure 3. Entry rate into job search assistance/visitation program, Southern Jutland county

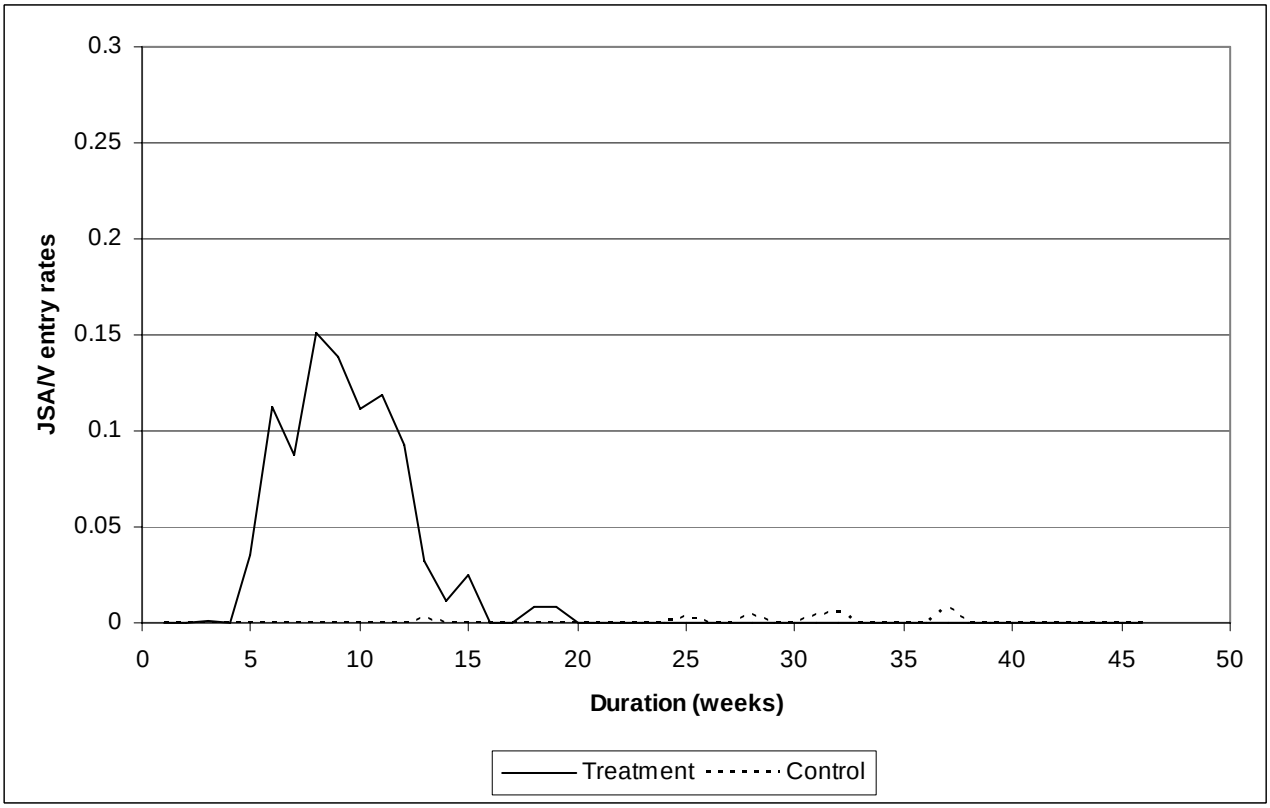


Figure 4. Meetings intensity, Storstrøm county

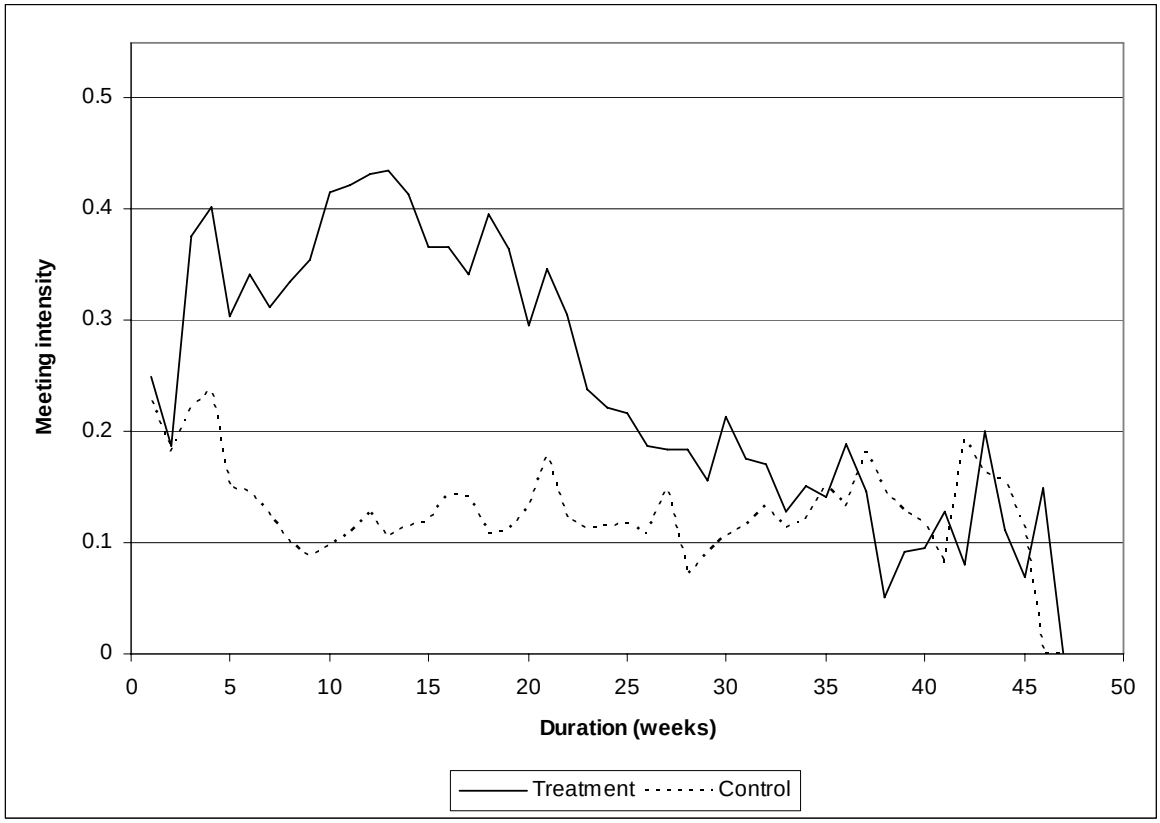


Figure 5. Meetings intensity, Southern Jutland county

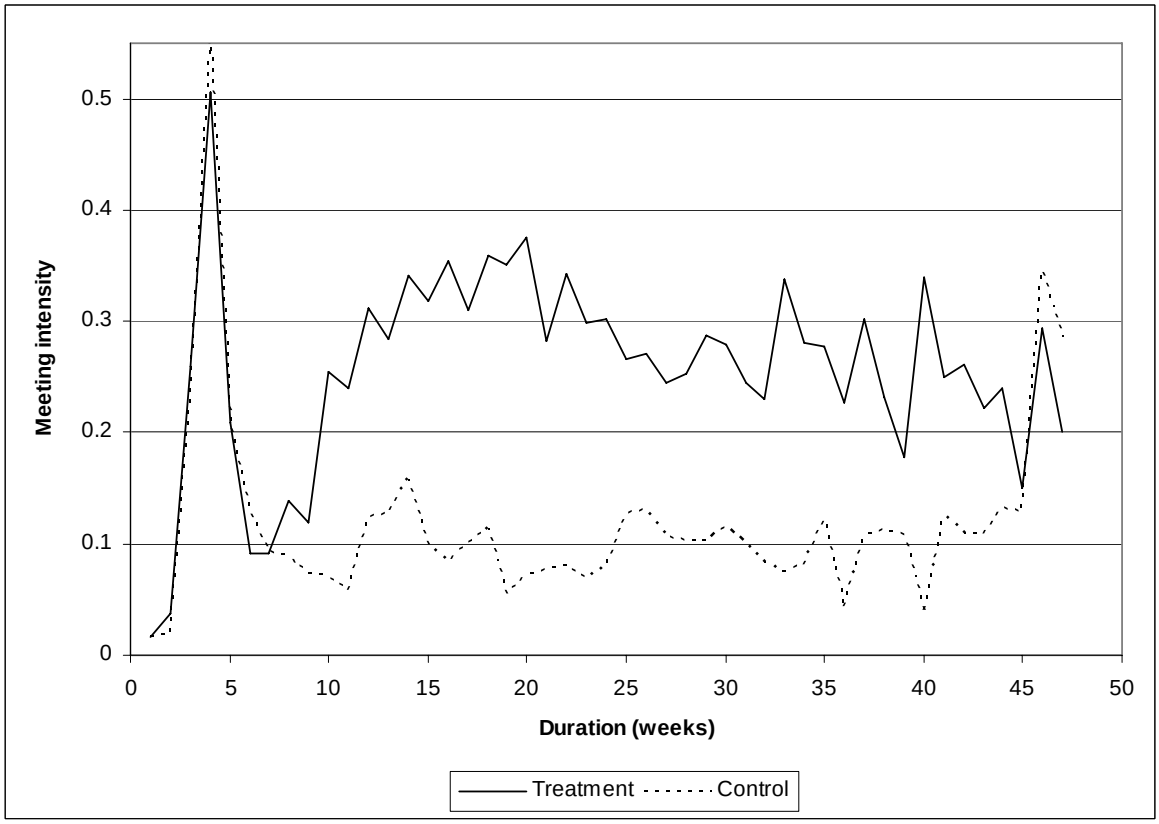


Figure 6. Program participation rate, Storstrøm county

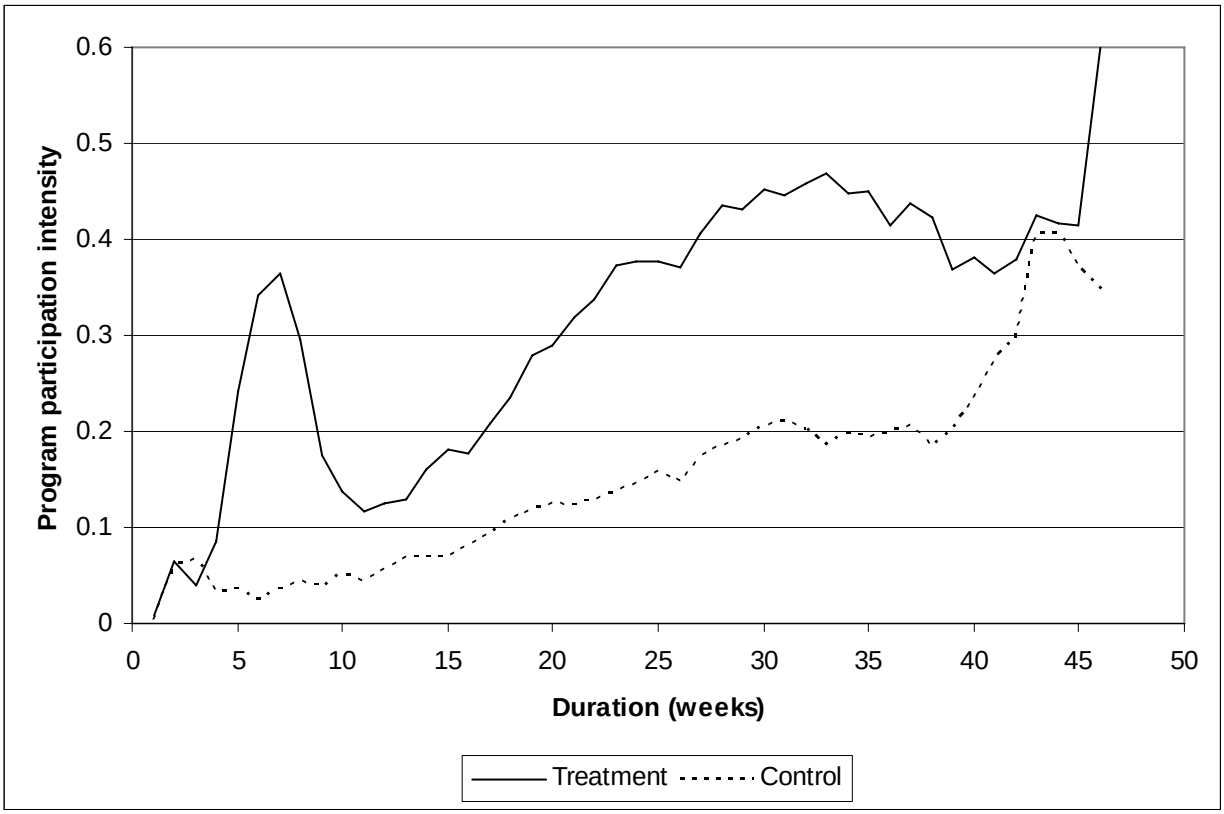


Figure 7. Program participation rate, Southern Jutland county

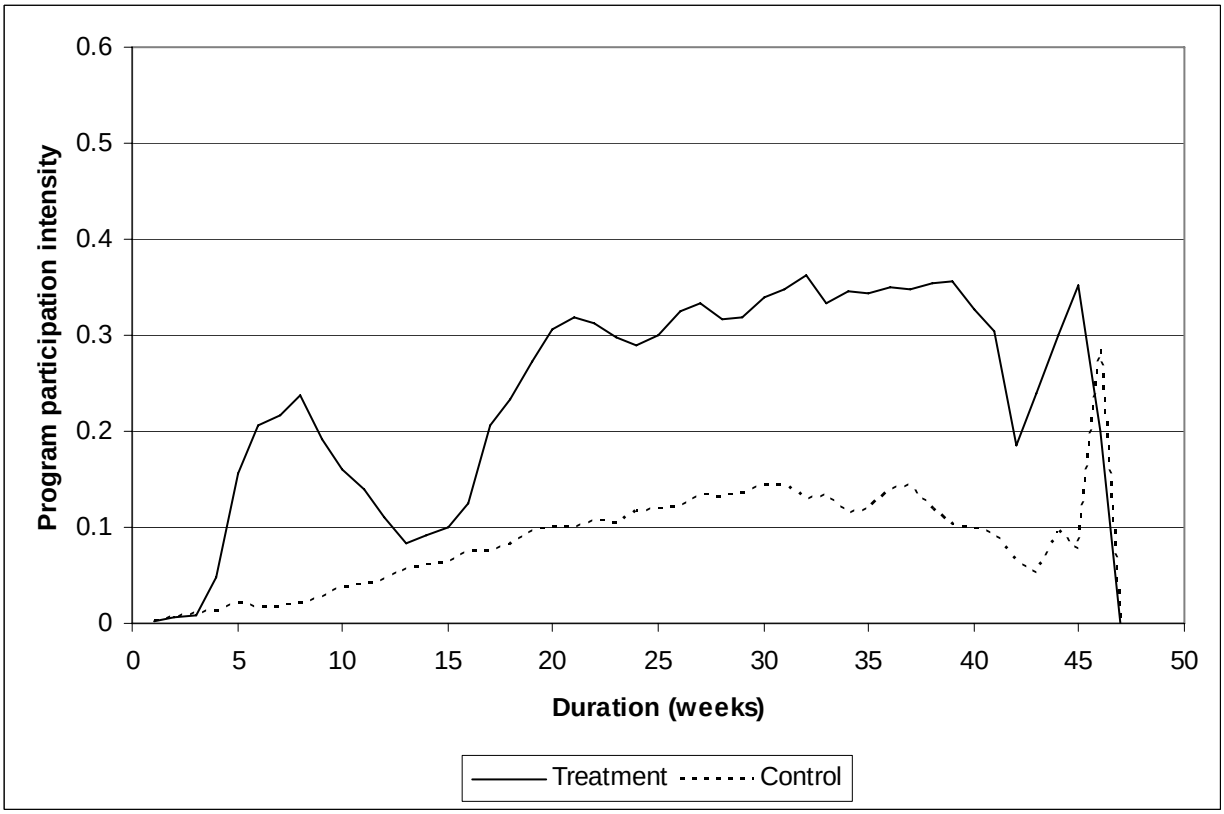


Figure 8. Difference in activity rates between treatment and control groups

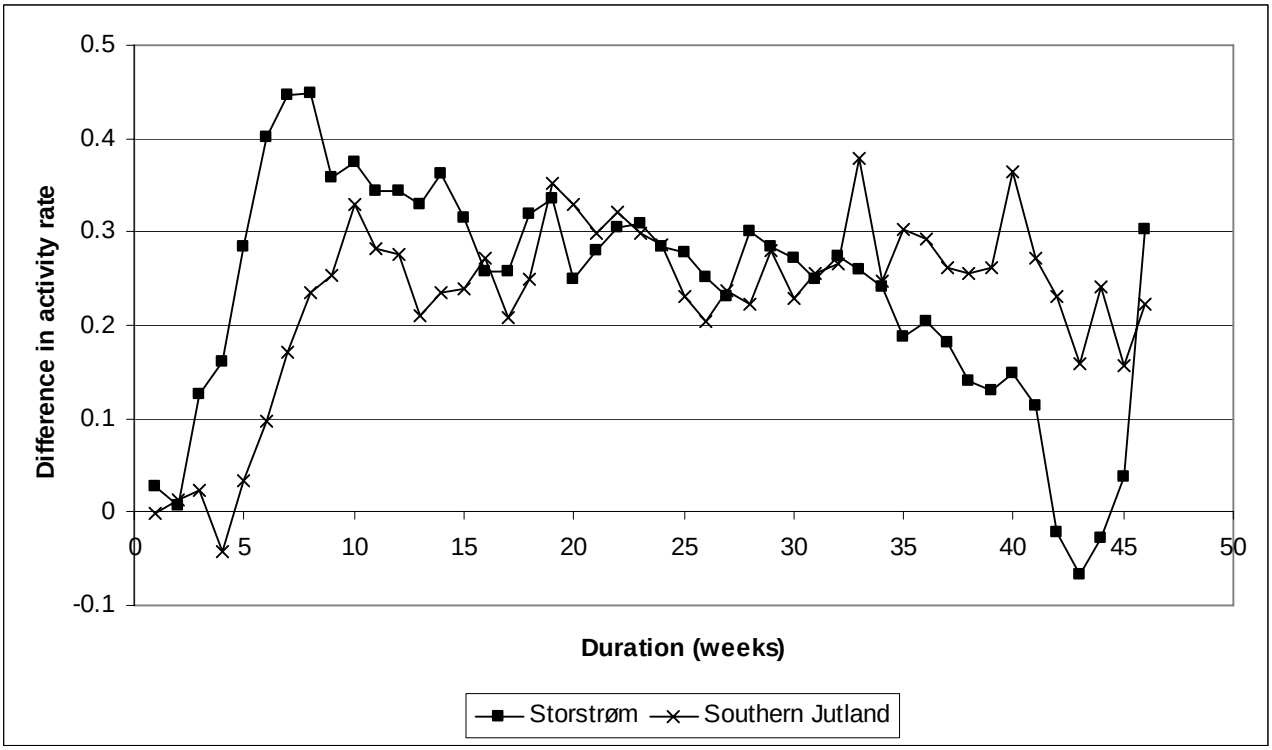


Figure 9. Intensity of assignment to private contractors, Storstrøm county

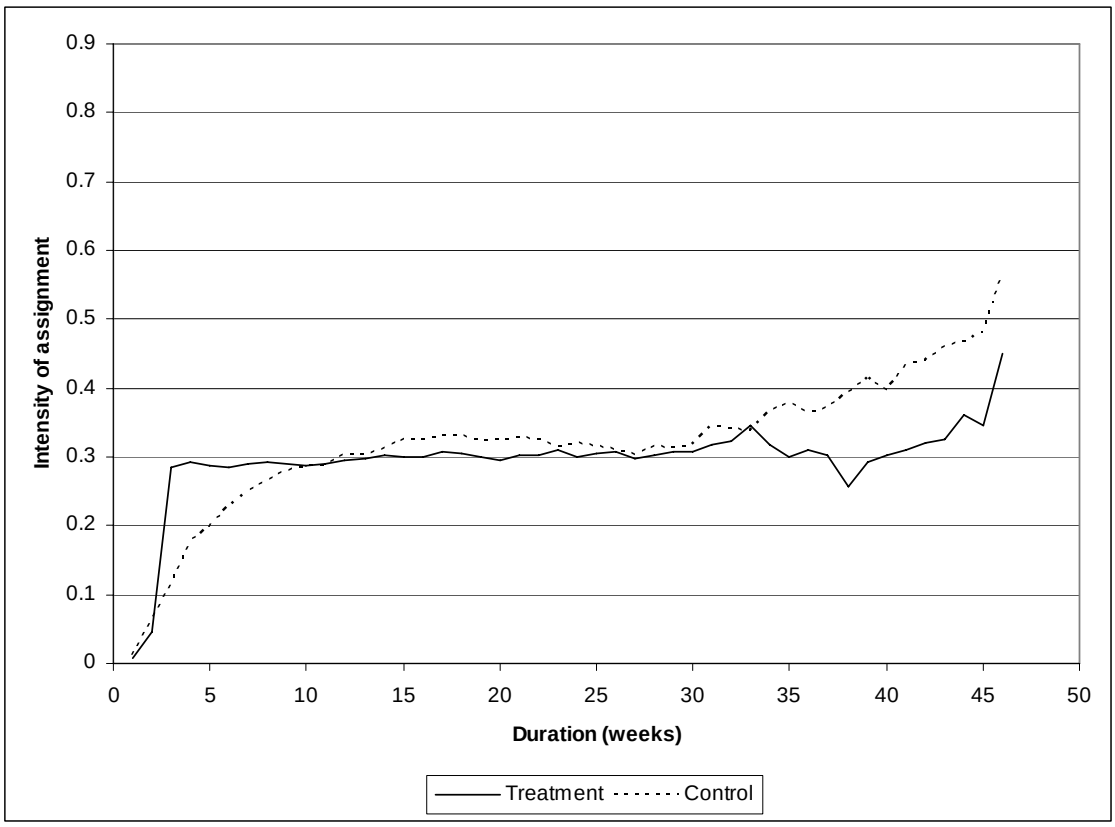


Figure 10. Intensity of assignment to private contractors, Southern Jutland county

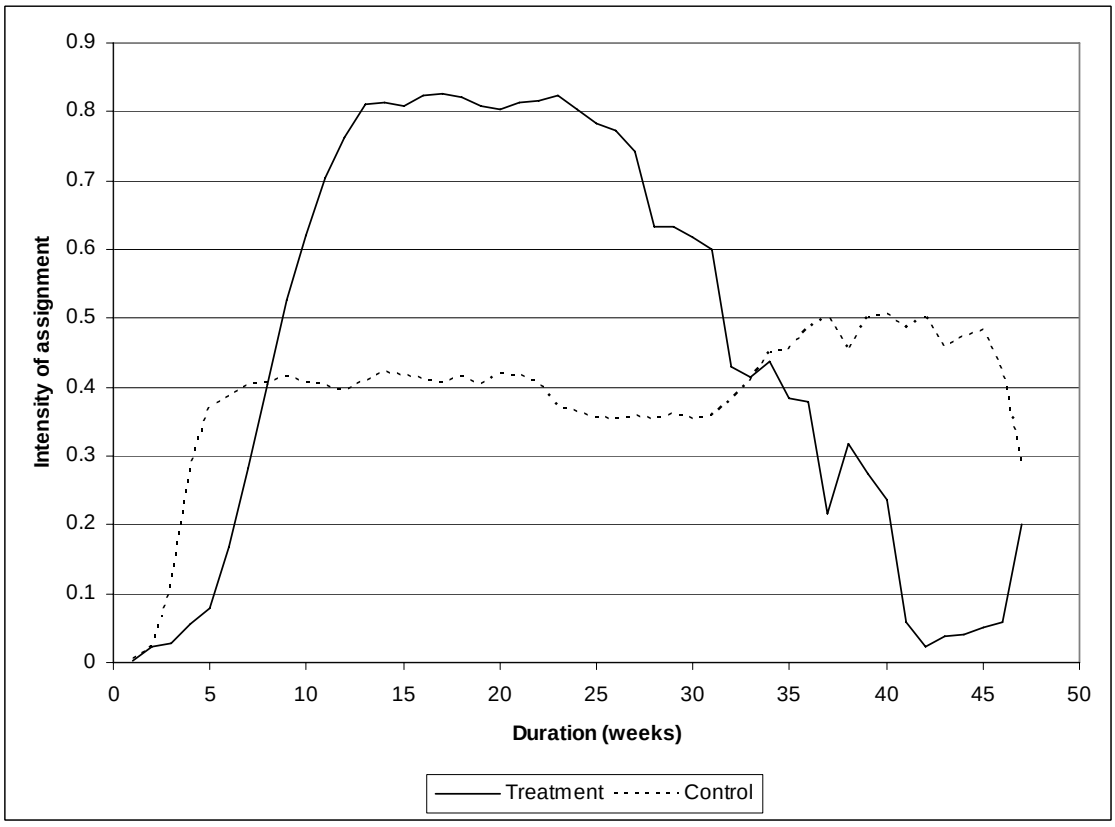
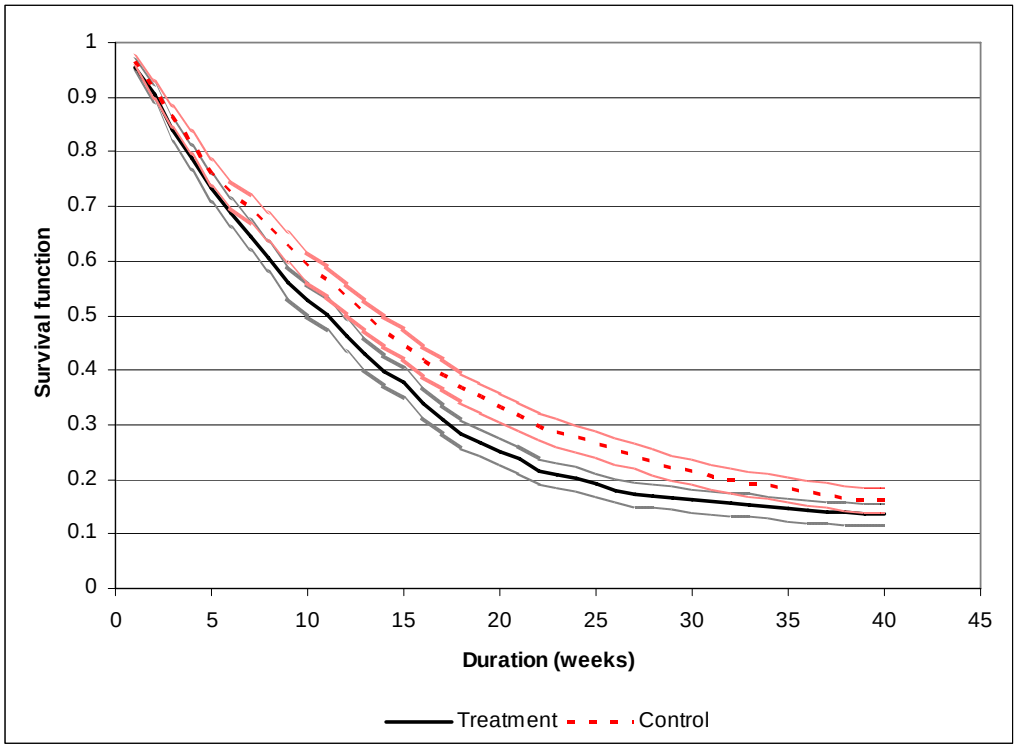
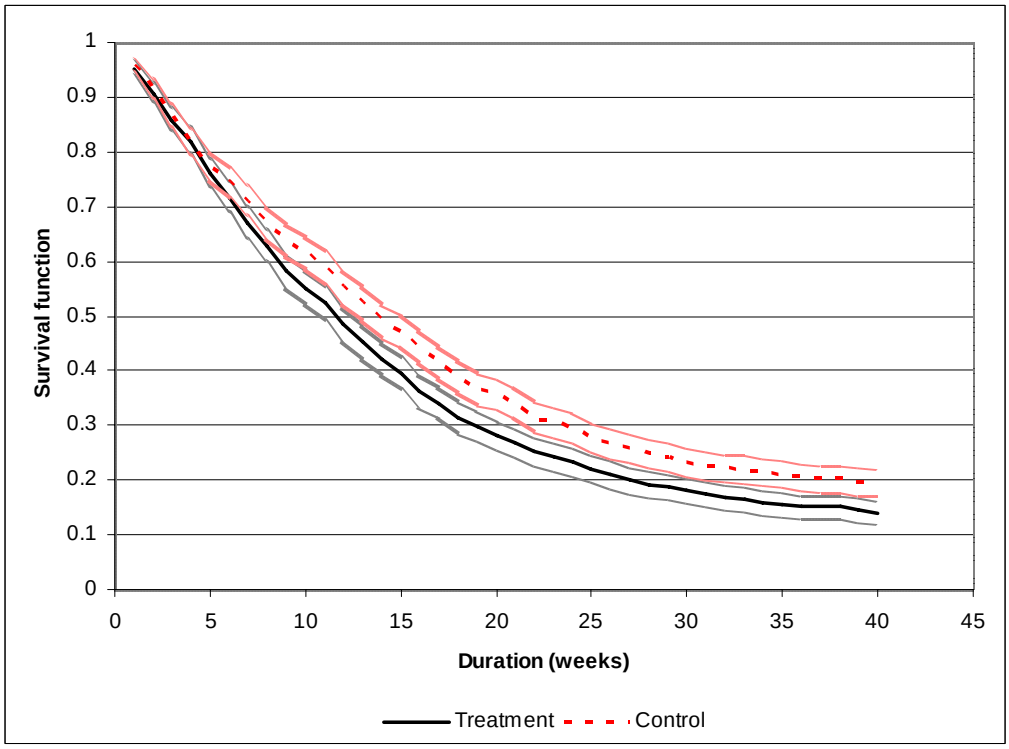


Figure 11. Kaplan-Meier survival functions in unemployment, Storstrøm county



Note: Shaded lines represent 95% confidence intervals for the survival functions

Figure 12. Kaplan-Meier survival functions in unemployment, Southern Jutland county



Note: Shaded lines represent 95% confidence intervals for the survival functions

Figure 13. Kaplan-Meier exit rates from unemployment, Storstrøm county

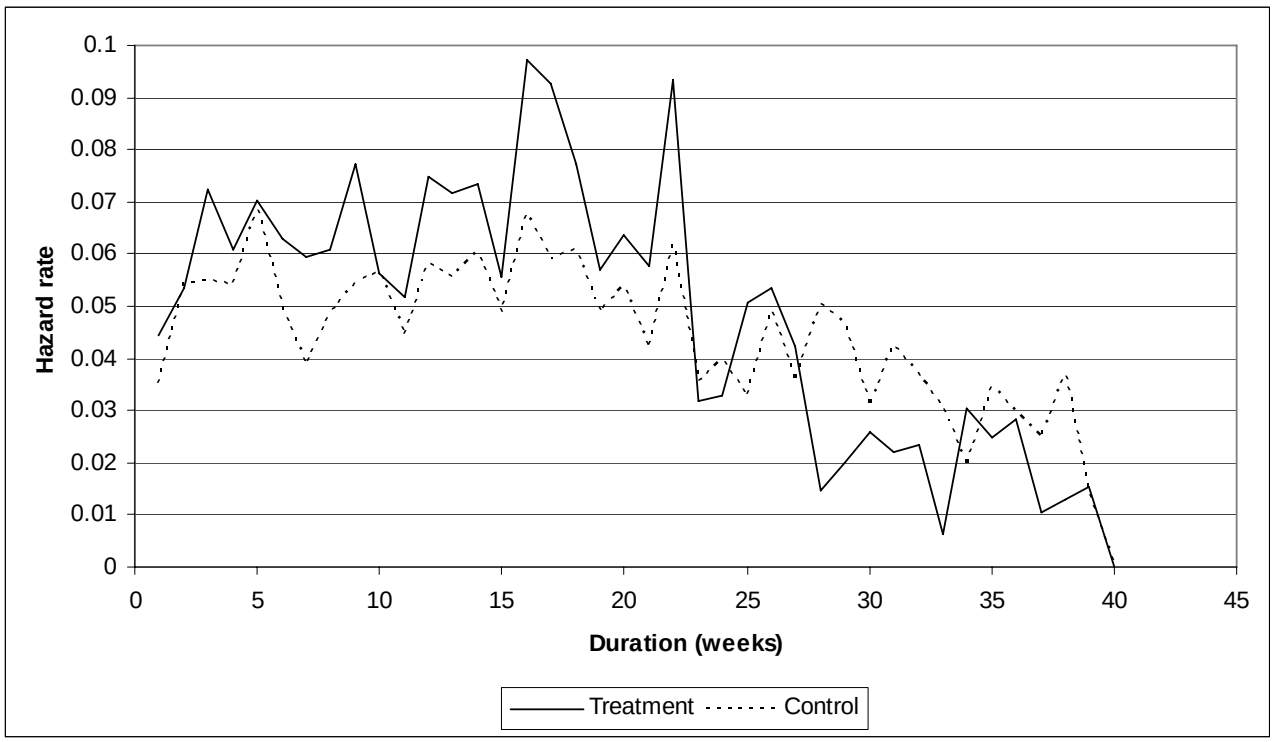


Figure 14. Kaplan-Meier exit rates from unemployment, Southern Jutland county

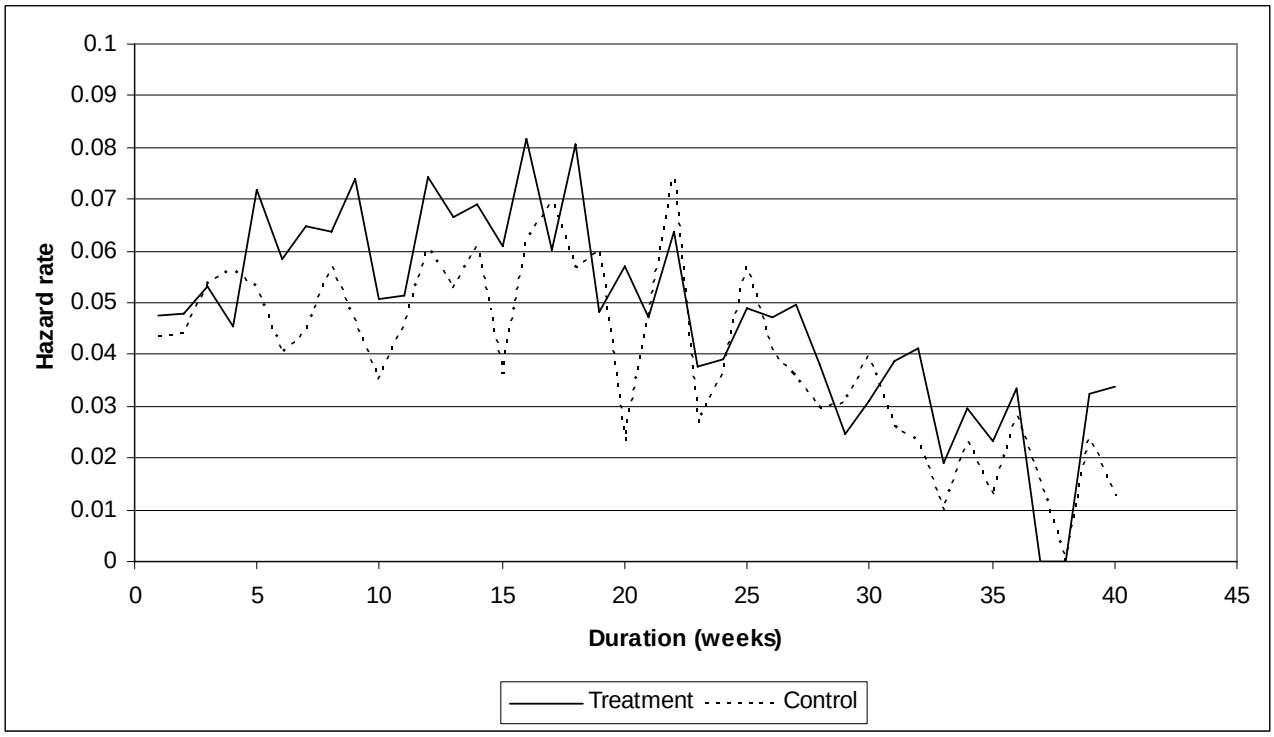
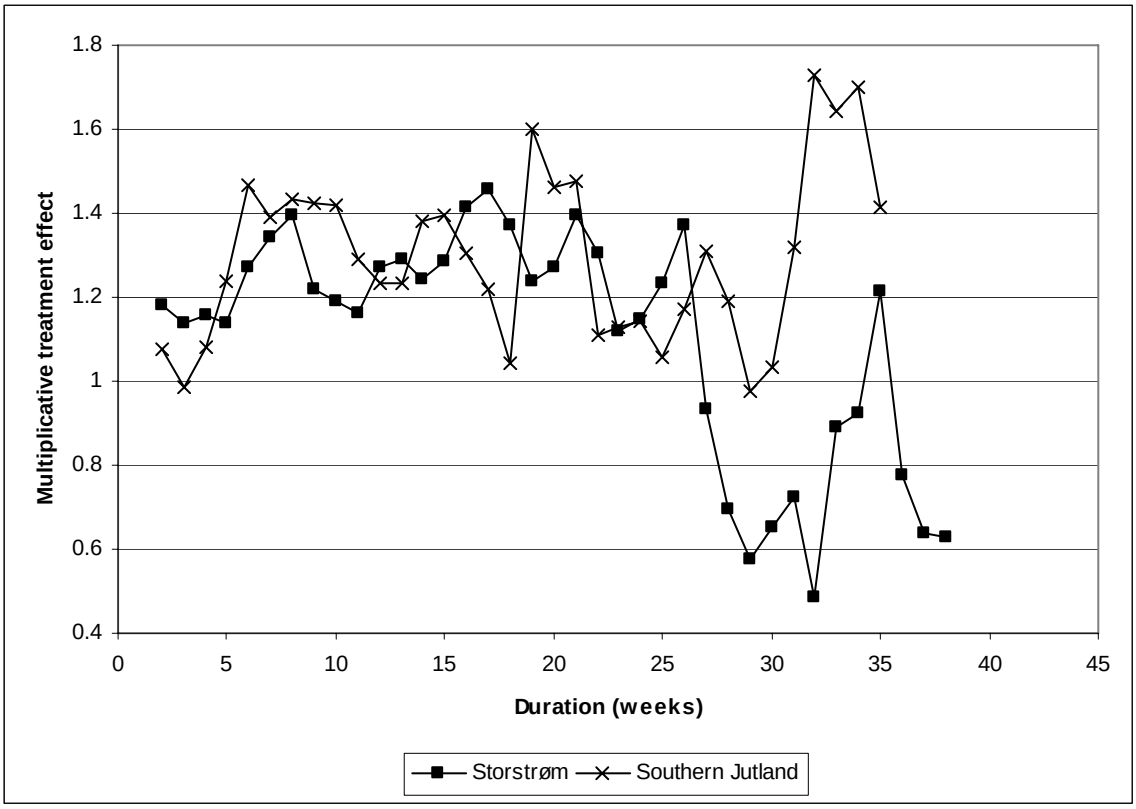


Figure 15. Flexibly estimated treatment effects



Appendix

Table A1. Cofficients to background characteristcs in duration Model 1.

	Storstrøm		Southern Jutland	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Starting week:				
43, 2005	0.25	0.22	0.19	0.20
44, 2005	0.05	0.17	0.04	0.17
45, 2005	0.14	0.21	0.03	0.24
46, 2005	0.01	0.22	0.00	0.22
47, 2005	0.19	0.21	-0.05	0.24
48, 2005	0.07	0.18	-0.15	0.18
49, 2005	*0.35*	*0.19*	0.00	0.21
50, 2005	0.19	0.19	-0.04	0.20
52, 2005	**0.55**	**0.17**	**0.45**	**0.17**
1, 2006	0.25	0.16	0.05	0.17
2, 2006	**0.70**	**0.18**	*0.34*	*0.19*
3, 2006	**0.49**	**0.18**	*0.34*	*0.20*
4, 2006	**1.00**	**0.17**	**0.71**	**0.18**
5, 2006	0.23	0.16	0.05	0.17
6, 2006	0.26	0.19	0.11	0.20
7, 2006	**0.56**	**0.19**	**0.60**	**0.21**
8, 2006				
Gender				
Male	**0.40**	**0.06**	**0.36**	**0.06**
Female				
Age group				
Less than 24	0.12	0.09	**0.25**	**0.10**
25-29	0.11	0.09	0.03	0.09
30-39				
40-49	**-0.14**	**0.07**	0.02	0.08
50-59	**-0.27**	**0.07**	**-0.26**	**0.08**
60 and older	-0.21	0.16	-0.06	0.17
Ethnicity				
Danish				
Western Immigrant	-0.27	0.19	0.05	0.14
Non-western Immigrant	-0.19	0.13	*-0.26*	*0.15*
Unemployment Insurance Fund				
Construction workers	**0.83**	**0.10**	**0.44**	**0.11**
Metal Industry	-0.02	0.11	**-0.27**	**0.12**
Manufacturing				
Technicians	**-0.30**	**0.12**	-0.05	0.14
Trade	**-0.27**	**0.09**	**-0.50**	**0.11**
White collar	-0.11	0.09	-0.10	0.10
Academics	**-0.43**	**0.17**	-0.18	0.17
Others	**-0.52**	**0.15**	**-0.56**	**0.12**
Self-employed	**-0.18**	**0.08**	**-0.27**	**0.08**
Past Public Income Transfer Rate				
Last year	**-0.56**	**0.11**	**-0.47**	**0.12**
1-2 years ago	-0.02	0.12	0.02	0.12
2-3 years ago	-0.16	0.10	*-0.19*	*0.10*

Table A2. Estimation of the weekly activity rate

	Storstrøm		Southern Jutland	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Starting week:				
43, 2005	0.050	0.101	*0.183*	*0.100*
44, 2005	0.067	0.083	**0.336**	**0.090**
45, 2005	*-0.176*	*0.099*	0.127	0.112
46, 2005	-0.002	0.095	0.135	0.110
47, 2005	**0.227**	**0.102**	**0.376**	**0.106**
48, 2005	0.075	0.086	**0.369**	**0.091**
49, 2005	-0.085	0.092	**0.576**	**0.100**
50, 2005	**-0.254**	**0.097**	**0.260**	**0.104**
52, 2005	-0.012	0.085	**0.265**	**0.095**
1, 2006	-0.034	0.081	**0.314**	**0.089**
2, 2006	-0.009	0.098	-0.068	0.111
3, 2006	0.070	0.102	**0.342**	**0.116**
4, 2006	-0.073	0.092	**0.385**	**0.106**
5, 2006	0.050	0.084	**0.376**	**0.093**
6, 2006	-0.086	0.100	**0.263**	**0.107**
7, 2006	*-0.184*	*0.104*	-0.163	0.158
8, 2006				
Gender				
Male	**-0.067**	**0.028**	0.018	0.031
Female				
Age group				
Less than 24	**0.177**	**0.052**	**0.217**	**0.051**
25-29	*0.077*	*0.043*	0.039	0.044
30-39				
40-49	0.047	0.034	**-0.146**	**0.039**
50-59	**0.145**	**0.034**	*-0.067*	*0.040*
60 and older	-0.002	0.068	-0.081	0.085
Ethnicity				
Danish				
Western Immigrant	0.073	0.076	0.052	0.073
Non-western Immigrant	**0.194**	**0.051**	**0.169**	**0.060**
Unemployment Insurance Fund				
Construction workers	**0.206**	**0.069**	0.001	0.077
Metal Industry	0.045	0.062	-0.026	0.073
Manufacturing				
Technicians	**0.314**	**0.062**	0.139	0.086
Trade	**-0.148**	**0.040**	-0.010	0.048
White collar	0.045	0.042	**-0.259**	**0.051**
Academics	-0.080	0.074	**0.443**	**0.079**
Others	-0.097	0.062	0.073	0.055
Self-employed	**-0.102**	**0.036**	0.040	0.038
Past Public Income Transfer Rate				
Last year	*0.082*	*0.048*	**0.251**	**0.050**
1-2 years ago	**-0.190**	**0.052**	**0.145**	**0.058**
2-3 years ago	0.000	0.044	-0.069	0.049
Treatment Group Indicators				
Weeks 1-2	0.117	0.072	0.287	0.231
Weeks 3-4	**0.753**	**0.071**	-0.048	0.071
Weeks 5-9	**1.956**	**0.054**	**0.962**	**0.061**
Weeks 10-15	**1.752**	**0.057**	**1.513**	**0.063**
Weeks 16-18	**1.305**	**0.086**	**1.322**	**0.097**

Weeks 19-22	**1.323**	**0.083**	**1.663**	**0.092**
Weeks 22-30	**1.238**	**0.068**	**1.217**	**0.071**
Weeks 31+	**0.851**	**0.077**	**1.301**	**0.081**

Note: The parameters shown are log odds ratios. The duration parameters are not shown in the table, but are available on request. Numbers in bold are statistically significant at the 5% level, and numbers in italics are statistically significant at the 10% level.

Figure A1. Activity rates