ESTIMATING EQUILIBRIUM EFFECTS OF JOB SEARCH ASSISTANCE

-PRELIMINARY VERSION-

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Abstract

Randomized experiments provide the policy-relevant-treatment effect of active labor market programs if there are no spillovers between workers in the treatment and control group. This assumption is likely to be violated if workers in the treatment and control group compete for the same jobs. We exploit data from a randomized experiment in two Danish counties and compare the outcomes for the workers in the control group to the outcomes for workers in comparison regions. Our results suggest that treatment externalities exist. We then construct a theoretical matching model, which we calibrate to match the results from the empirical analyses given the observed treatment intensity. Using this model, we compute the labor market outcomes for the case that all workers are treated.

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1 Introduction

In this paper we estimate the effects of a Danish activation program for unemployed workers taking into account congestion and equilibrium effects. The program starts quickly after entering unemployment. The goal is to provide intensive guidance towards finding work.[1] To evaluate the effectiveness of the activation program, a randomized experiment was setup in two Danish counties. Graversen and Van Ours (2008), Rosholm (2008) and Vikstrom et al. (2011) show that participants found work significantly faster than nonparticipants, and the differences are quite substantial. To investigate the presence of congestion and general equilibrium effects, we compare job finding rates of non-treated workers in the treatment counties with unemployed workers in comparison counties (using the same administrative data). Since both experiment counties were not selected randomly, we use pre-experiment data from all counties to control in a difference-in-difference setting for existing differences between counties. This allows us to estimate the treatment effect on the non treated.

We also focus on how the experiment affects vacancy supply. Our estimation results show that in the experiment period the supply of vacancies increased significantly faster in the experiment regions than in the comparison regions. Next, we develop an equilibrium search model that incorporates the activation program, and can describe both the negative congestion effects (it takes more time for non treated workers in the treatment region to find jobs) and positive vacancy-supply effects. We use the results from the empirical analyses to estimate the parameters from the equilibrium search model using indirect inference. With the estimated model we study the effects of a large scale role out of the activation program and compute the effects on labor market behavior and outcomes. We find that despite the negative congestion effects, the overall effects of the program are still beneficial in case of a large scale role out. A cost-benefit analysis indicates ....

A growing number of papers have stressed the importance of dealing with selective participation when evaluating the effectiveness of employment programs for disadvantaged workers. In particular, LaLonde (1986) showed that the results from a randomized experiment do not concur with a series of non-experimental estimates. Since then, the use of randomized experiments has become increasingly popular when evaluating active labor market programs, see for example Johnson and Klepinger (1994), Meyer (1995), Dolton and O'Neill (1996), Gorter and Kalb (1996), Ashenfelter et al. (2005), Card and Hyslop (2005), Van den Berg and Van der

[1] The program includes job search assistance and meetings with caseworkers during which, for example, job search effort is monitored and vacancies are offered. If this was not successful, the caseworker has some discretion in choosing an appropriate follow-up program.

Klaauw (2006), and Graversen and Van Ours (2008). The evaluation of active labor market programs is typically based on comparing the outcomes of participants with nonparticipants. This is not only the case in experimental evaluations, but also in non-experimental evaluations (after correcting for selection). It implies that congestion and spillovers between participants and nonparticipants in a program are ignored (e.g. DiNardo and Lee (2011)).

In case of active labor markt programs, equilibrium effects are likely to matter. Moreover, the goal of an empirical evaluation is to collect information that helps to decide whether or not a program should be implemented on a large scale. Therefore, taking account of equilibrium effects is extremely important. If there are equilibrium effects, changing the treatment intensity affects the labor market outcomes of both participants and nonparticipants. This implies that the results from the empirical evaluation are only relevant at the observed treatment intensity. Cahuc and Le Barbanchon (2010) shows within a theoretical equilibrium search model that neglecting such equilibrium effects can lead to wrong conclusions regarding the effectiveness of the program. Blundell et al. (2004) and Ferracci et al. (2010) show empirically that spillover effects can be quite sizable and Lise et al. (2004) show that the conclusion from a costs-benefits evaluation is reversed when taking account of equilibrium effects.

The remainder of the paper is organized as follows. Section 2 discusses the background of the Danish randomized experiment, as well as literature on treatment externalities. Section 3 provides a description of the data and section 4 presents the empirical analyses and the estimation results. In section 5 we develop an equilibrium search model including the activation program. We calibration this model in section 6 and we use the calibrated model for policy simulations. Section 8 concludes.

2 Background

2.1 The Danish experiment

In this subsection, we provide some details about the activation policy for unemployed workers considered in this paper. We also discuss the randomized experiment used to evaluate the effectiveness of the policy and review earlier studies on this experiment. More details on the institutional background can be found in Graversen and Van Ours (2008) and Rosholm (2008).

The goal of the activation policy is to provide intensive guidance towards finding work. The relevant population consists of newly unemployed workers. After approximately 1.5 weeks of unemployment, those selected for the program received a letter explaining the content of the program. The program consists of three parts.

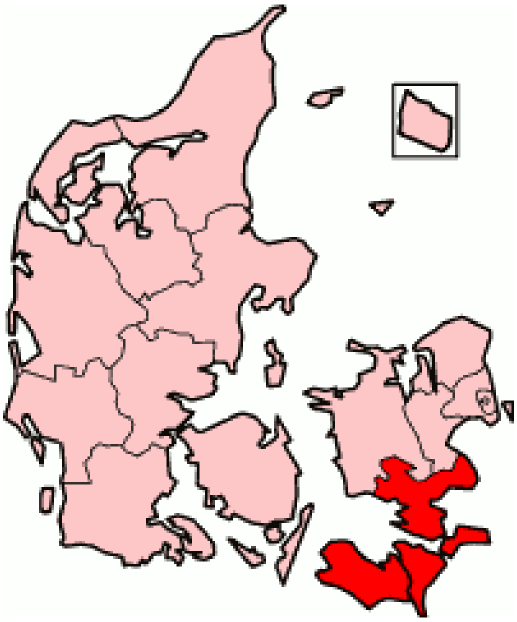
First, after five to six weeks of unemployment workers had to participate in a two-week job search assistance program. Next, the unemployed worker had to meet a caseworker either weekly or biweekly. During these meetings a job search plan was developed, search effort was monitored and vacancies were provided. Finally, if after four months the worker still did not find work, a new program started for at least three months. At this stage the caseworker had some discretion in choosing the appropriate program, which could either be more job search assistance, a temporary subsidized job in either the private sector or the public sector, classroom training, or vocational training. The total costs of the program were 2122 DKK per entitled worker.

To evaluate the effectiveness of the activation policy, a randomized experiment was conducted in two Danish counties, Storstrøm and South Jutland. These counties are shown in Figure 1. Both regions are characterized by a small public sector relative to other Danish counties. The key economic sectors are industry, agriculture, and to some extent transportation. All individuals starting collecting unemployment benefits from November 2005 to February 2006 participated in the experiment. Individuals born on the first to the 15^{th} of the month participated in the activation policy, while individuals born on the 16^{th} to the 31^{st} did not receive this treatment. The control group received the usual assistance, consisting of meetings with a caseworker every three months and more intensive assistance after one year of unemployment.

During the experiment Denmark had about 5.5 million inhabitants and consisted of 15 counties. Storstrøm and South Jutland each contained about 250,000 inhabitants. Both counties volunteered to run the experiment. At the time of the experiment the unemployment rate in Denmark was about 4.2 percent. Denmark provides relatively high unemployment benefits. The average UI benefits level is about 16033 DKK per month and the average replacement rate is between 65 and 70 percent. It is often argued that the success of Danish active labor market programs explains the low unemployment rate (e.g. Rosholm (2008)). The medium unemployment duration at the time of the experiment was about 13 weeks.

Graversen and Van Ours (2008) use duration models to estimate the effect of the activation program on exit rates to work. They find strong effects, due to the program the re-employment rate increases about 30 percent, and this effect is constant across age and gender. Rosholm (2008) finds similar results when estimating the effect of the activation separately for both counties. Graversen and Van Ours (2008), Rosholm (2008) and Vikstrom et al. (2011) all investigate which elements of the activation program are most effective. Graversen and Van Ours (2008) find that the threat effect and job search assistance are most effective. A similar conclusion is drawn by Vikstrom et al. (2011), who construct nonparametric bounds.

Figure 1: Location of the experiment counties.



[Storstrøm.]

(b) South Jutland.

Also Rosholm (2008) finds substantial threat effects. Additional evidence for threat effects is provided by Graversen and Van Ours (2009). They show that the effect of the activation program was most substantial for individuals with the largest travel time to the program location.

All studies on the effect of the Danish activation program ignore possible spillover effects between participants and nonparticipants. Graversen and Van Ours (2008) argue that spillovers should be small because the share of the participants in the total population of unemployed workers never exceeds eight percent. If this share is indeed small, substantial spillover effects are unlikely. However, we estimate that within an experiment county the share of participants in the stock of unemployed workers is much larger towards the end of the experiment period. Approximately five percent of all unemployed workers find work each week, such that after four months in which half of all newly unemployed workers are participants, about 30 percent of the stock of unemployed workers is assigned to the activation program. Moreover, the outflow of long-term unemployed workers is considerably lower than the outflow of short-term unemployed workers, so that competition for jobs occurs mostly between short-term unemployed workers. Among short-term unemployed workers the share participating in the activation program is even higher than 30 percent, which suggests that externalities can actually be substantial.

2.2 Treatment externalities

In this subsection we briefly illustrate the definition of treatment effects in the presence of possible treatment externalities. We also discuss some recent empirical literature dealing with treatment externalities. We mainly focus on labor market applications, but also briefly address some empirical studies on treatment externalities in other fields.

Within a population of N individuals, the treatment effect for individual i equals

$$\Delta_i(D_1, .., D_N) \equiv E[Y_{1i}^*|D_1, .., D_N] - E[Y_{0i}^*|D_1, .., D_N] \tag{1}$$

Where Y_{0i}^* and Y_{1i}^* denote the potential outcomes without treatment and with treatment, respectively. D_i equals one if individual i participates in the program and zero otherwise. A standard assumption in the treatment evaluation literature is that each individual's behavior or outcomes do not directly affect the behavior of other individuals (e.g. DiNardo and Lee (2011)). This assumption is formalized in the stable unit treatment value assumption (SUTVA), which states that the potential outcomes of each individual are independent of the treatment status of other individuals in the population (Cox (1958)),

$$(Y_{1i}^*, Y_{0i}^*) \perp D_j \qquad \forall j \neq i$$

If indeed SUTVA holds, then the treatment effect for individual i equals $\Delta_i = E[Y^*_{1i}] - E[Y^*_{0i}]$. When data from a randomized experiment are available such as from the Danish experiment discussed in the previous subsection, the difference-in-means estimator estimates the average treatment effect in the population $\Delta = \frac{1}{N}\sum_i^N \Delta_i$.

However, if SUTVA is violated, the results from a randomized experiment are of limited policy relevance. This is, for example, the case when the ultimate goal is a large scale role out of a program (e.g. DiNardo and Lee (2011), Heckman and Vytlacil (2005)). The treatment effect for individual i in equation (1) depends on which other individuals receive treatment. If all individuals live within the same area, then it might only be relevant which fraction of the population in the same area receive treatment. The latter is defined by $\bar{D}_N = \frac{1}{N}\sum_{i=1}^N D_i$. In the case of the Danish activation program, the area is taken as the county which we assume to act as local labor market. See for a justification of this assumption Van den Berg and Van Vuuren (2010), who discuss local labor markets in Denmark. Also Deding and Filges (2003) report a low geographical mobility in Denmark. When the ultimate goal is the large scale role out of a treatment, the policy relevant treatment effect is

$$\Delta = \frac{1}{N}\sum_i^N E[Y^*_{1i}|\bar{D}_N = 1] - E[Y^*_{0i}|\bar{D}_N = 0]$$

Identification of this treatment effect requires observing similar local labor markets in which sometimes all unemployed workers participate in the program and sometimes no individuals participate. A randomized experiment within a single local labor market does not provide the required variation in $\bar{D}_N$.

Previous literature on the Danish activation program shows that participants have higher re-employment rates than nonparticipants. Because participants and nonparticipants were living in the same local labor market, SUTVA might be violated. Activating some unemployed job seekers can have various spillover effects to other unemployed job seekers. First, if activated unemployed workers search more intensively, this can reduce the job finding rates of non-activated unemployed workers competing for the same jobs. Second, the activation program may affect reservation wages of the participants, and thereby wages. Third, when unemployed workers devote more effort to job search, a specific vacancy is more likely to be filled. Firms may respond to this by opening more vacancies. These equilibrium effects do not only apply to the control group but also to other members of the treatment group. In Section 5 we provide a more formal discussion on possible equilibrium effects due to the activation policy.

As discussed in the previous subsection, the randomized experiment to evaluate the activation program was conducted in two Danish regions. The randomized experiment provides an estimate for $\Delta(\bar{d}_N)$, where $\bar{d}_N$ is the observed fraction of unemployed job seekers participating in the activation program. In addition, we compare the outcomes of the nonparticipants to outcomes of unemployed workers in other regions. This should provide an estimate for $E[Y^*_{0i}|\bar{D}_N = \bar{d}_N] - E[Y^*_{0i}|\bar{D}_N = 0]$. To deal with structural differences between regions, we use outcomes in all regions prior to the experiment and we make a common trend assumption. In Section 4 we provide more details about the empirical analyses. Still the empirical approach only identifies treatment effects and equilibrium effects at a treatment intensity $\bar{d}_N$, while for a large scale role out of the program one should focus on $\bar{D}_N = 1$. Therefore, in Section 5 we develop an equilibrium search model, which we estimate using the estimated treatment effects. Using this model we investigate the case of providing treatment to all unemployed workers $\bar{D}_N = 1$ and get an estimate for the most policy relevant treatment effect Δ.

Treatment externalities have recently received increasing attention in the empirical literature. Blundell et al. (2004) evaluate the impact of an active labor market program (consisting of job search assistance and wage subsidies) targeted at young unemployed. Identification comes from differences in timing of the implementation between regions, as well as from age requirements. The empirical results show that treatment effects can change sign when general equilibrium effects and displacement effects are taken into account. Also Ferracci et al. (2010) find strong evidence for the presence of equilibrium effects of a French training program for unemployed workers. In their empirical analysis, they follow a two-step approach. In a first step, they estimate a treatment effect within each local labor market. In a second step, the estimated treatment effects are related to the fraction of treated individuals in the local labor market. Because of the non-experimental nature of their data, in both steps they rely on the conditional independence assumption to identify treatment effects.

A different approach is taken by Lise et al. (2004). They specify a matching model to quantify equilibrium effects of a wage subsidy program. The model is first tested for 'partial equilibrium implications' using experimental data. This implies that the model is calibrated to the control group, but it can predict treatment group outcomes well. The results show that general equilibrium effects are substantial and may even reverse the cost-benefit conclusion made on the basis of a partial equilibrium analysis.

Crepon et al. (2011) use data from a randomized experiment to identify equilibrium effects of a counseling program. The experiment took place in various French regions and included two levels of randomization. First, for each region the treatment intensity was randomly determined, and second, within each region unemployed workers were randomly assigned to the program according to the local treatment intensity. The target population are high-educated unemployed workers below age 30 who have been unemployed for at least six months. This is only a very small fraction of the total stock of unemployed workers. So one may doubt whether variation in the treatment intensity for this group will have any equilibrium effects. Furthermore, even for individuals assigned to the program, participation is voluntary, and refusal rates turned up to be very high. Indeed, it is not very surprising that no equilibrium effects are found even though the estimated treatment effect is substantial.

Also outside the evaluation of active labor market programs, there is an increasing interest in estimating treatment externalities. Miguel and Kremer (2004) estimates spillover effects of de-worming drugs on schools in Kenya. They find that simple estimates of the treatment effect underestimate the real effect, since there are large positive spillovers to the control group. Duflo et al. (2008) study the effect of tracking on schooling outcomes, allowing for several sources of externalities. Moretti (2004) shows that equilibrium effects of changes in the supply of educated workers can be substantial.

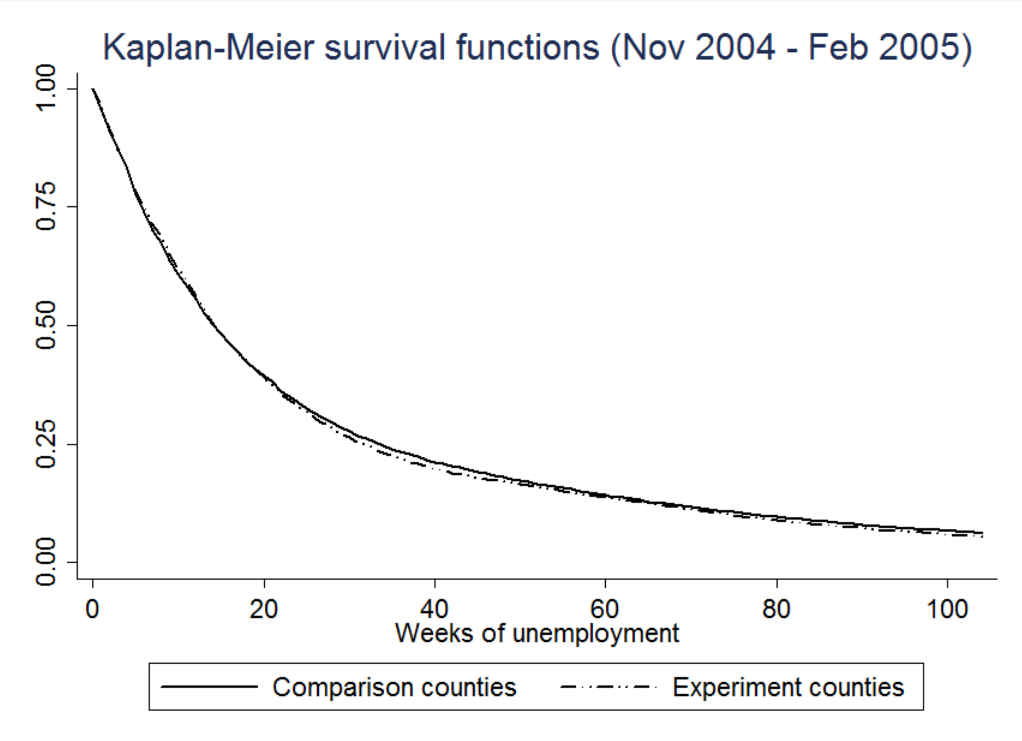
3 Data

For the empirical analyses we use two data sets. The first is an administrative data set describing unemployment spells. Second, we have a data set including the stock of open vacancies. Below we discuss both data sets in detail.

These numbers remain after we removed observations that exhibit inconsistencies due to errors in the data collection. These include observations from the period November 2004 and February 2005 that still have been classified as belonging to either the control or treatment group; observations that are classified as belonging to the control or treatment group but from counties other than Storstrøm or South Jutland; observations from the experimental counties and the experimental period, which are not classified as control or treatment group. In total, 3.2 % of the data was removed.

The randomized experiment discussed in Subsection 2.1 involved all individuals becoming unemployed between November 2005 and February 2006 in Storstrøm and South Jutland. Our data are from the National Labor Market Board and include all 36,652 individuals who applied for benefits in the experiment period in all Danish counties. Of these individuals 3751 lived in either Storstrøm or South Jutland and participated in the experiment. Of the participants in the experiment, 1814 individuals were assigned to the treatment group and 1937 to the control group. The data include also 49,063 individuals who started applying for benefits one year before the experiment period, so between November 2004 and February 2005. We

Figure 2: Survivor functions for the experimental counties and the comparison counties in the year before the experiment.

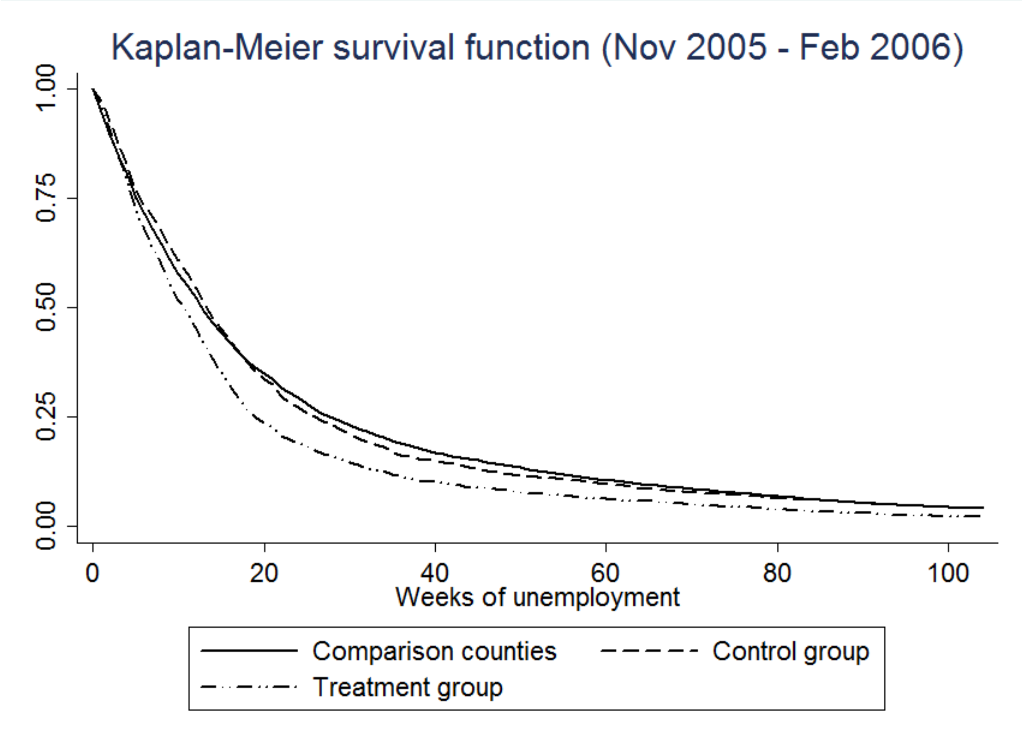


refer to this as the pre-experiment sample.

For each individual we observe the week of starting collecting benefits and the duration of collecting benefits measured in weeks. Individuals are followed for at most two years after becoming unemployed. All individuals are entitled to at least four years of collecting benefits. Combining the data on unemployment durations with data on income transfers shows that almost all observed exits in the first two years are to employment. In Figure 2 we show for individuals who started collecting benefits in the pre-experiment period (November 2004 until February 2005) the Kaplan-Meiers estimates for the survivor function. We distinguish between the experiment regions (Storstrøm and South Jutland) and all other regions which we refer to as comparison regions. Recall that Storstrøm and South Jutland volunteered to run the experiment. It is, therefore, interesting to compare these counties to the other Danish counties.

The Kaplan-Meier estimates show that in both the experiment and the comparison regions the median unemployment duration was 15 weeks. After one year, in the experiment regions 84.1 percent of the workers has left unemployment, and this was 83.4 percent in the comparison regions. This shows that in the period prior to the experiment the survivor function in unemployment were very similar. To test this more formally, we have performed a logrank test. This test cannot reject that

Figure 3: Survivor functions for the comparison counties, the control group and the treatment group during the experiment.



the distribution of the unemployment duration is the same in the experiment region as in the comparison region, the p-value for this test is 0.17.

Next, we consider individuals who entered unemployment in the experiment period (November 2005 until February 2006). Figure 3 shows the Kaplan-Meier estimates for the the treatment and control group in the experiment counties and for individuals living in the comparison counties. It is clear that individuals exposed to the activation program have a higher exit rate from unemployment than individuals assigned to the control group in the experiment counties. The Kaplan-Meier estimates show that after 11 weeks about 50 percent of the treated individuals have left unemployment, while this is 13 weeks for individuals in the control group and 14 weeks for individuals living in the comparison counties. Within the treatment group 92.6 percent of the individuals leaves unemployment within a year, compared to 88.8 percent in the control group and 87.3 percent in the comparison regions. A logrank test rejects that the distributions of unemployment durations are the same in the treatment and control group (p-value less than 0.01). But such a test cannot reject that the distributions of unemployment durations are the same in the control group and the comparison counties, the p-value equals 0.77. Finally, it should be noted that over time the unemployment duration distribution changed. In the comparison regions this distribution was substantially different between the pre-experiment

Table 1: Summary statistics.

	Experiment counties			Comparison counties	
	2004–2005	Treatment	Control	2004–2005	2005-2006
Male (%)	57	59	59	55	54
Benefits previous year (in weeks)	9.2	9.2	8.6	8.6	9.3
Benefits past two years (in weeks)	10.9	11.3	10.8	10.6	11.6
Native (%)	93	92	94	93	92
West. Immigrant (%)	4	5	4	3	4
Non-West. Immigrant (%)	3	3	3	4	4
Observations	5970	1814	1937	43,093	36,652
Unemployment rate (%)	6.1	5.0		5.7	4.8
Participation rate (%)	76.3	76.3		79.2	79.1
GDP/Capita (1000 DK)	197.5	201.3		219.8	225.1

period and the experiment period (p-value for similarity equals 0.01).

The data only include a limited set of individual characteristics. Table 1 shows summary statistics within each of the five groups. In the pre-experiment period the unemployed workers in the experiment regions had slightly more weeks of previous benefits receipt than in the comparison regions. The gender composition and nationality distribution were roughly similar. In the comparison regions in the experiment period the unemployed workers had a longer history of benefits receipt than in the pre-experiment period. This increase in not observed in the experiment regions. In the experiment period there was a higher fraction of males among those becoming unemployed in the experiment regions than in the comparison regions.

The lower panel of the table shows some county level statistics. In both the experiment counties and the comparison counties the local unemployment rate declined and GDP per capita increased between the pre-experiment and the experiment period. The labor force participation rate remained virtually unchanged. One can interpret this as evidence that the experiment counties and the comparison counties were subject to similar calendar time trends. However, in both time periods the labor market conditions were more favorable in the comparison counties than in the control counties, i.e. lower unemployment rate, higher labor force participation and higher GDP per capita. The level of unemployment was low compared to the

Our second data set describes monthly information on the average number of open vacancies per day in all Danish counties between January 2004 and November 2007. These data are collected by the National Labor Market Board on the basis of information from the local job centers. To take account of differences in sizes of the labor force between counties we consider the logarithm of the stock of vacancies.

Figure 4: Logarithm of stock of vacancies per month (experiment period between the vertical lines).

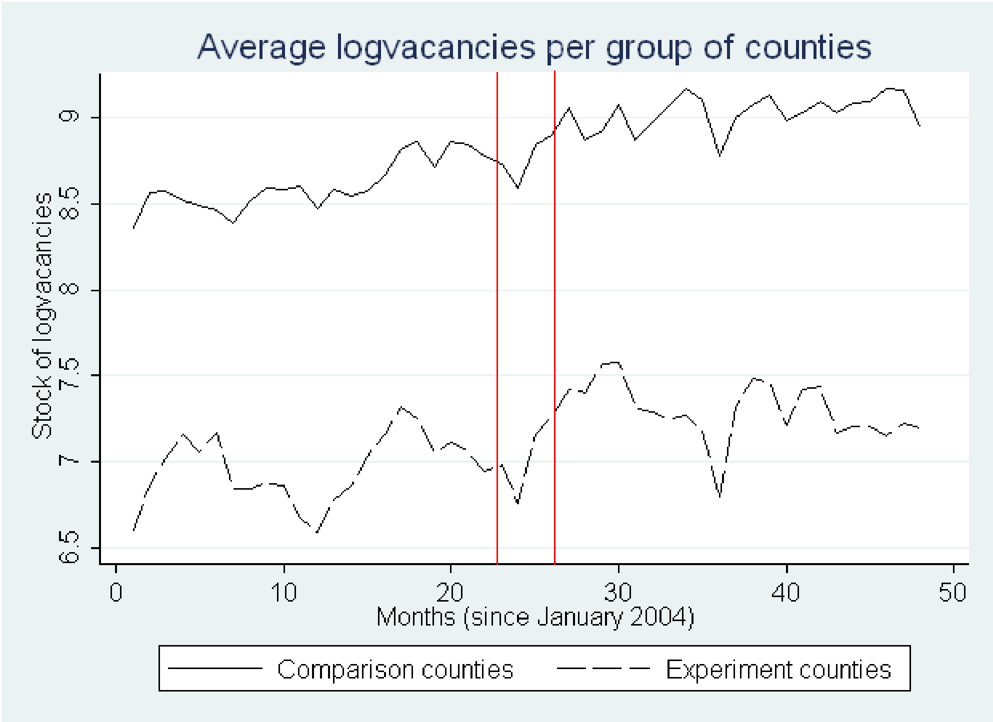


Figure 4 shows how both in the experiment counties and the comparison counties the average number of open vacancies changes over time. Both lines seem to follow the same business cycle pattern. However, during the experiment period and just afterwards, the increase in the vacancy stock was larger in the experiment regions than in the comparison regions.

4 Estimations

The previous section discussed some descriptive evidence on the impact of the activation program. In this section we provide more empirical evidence. We focus both on exit rates from unemployment and the stock of vacancies. The goal is not only to estimate the impact of the program, but also to investigate the presence of possible spillover effects.

4.1 Unemployment duration

The aim of the activation program was to stimulate participants to find work faster. In previous studies on the randomized experiment participants were compared to nonparticipants (see Graversen and Van Ours (2008), Rosholm (2008) and Vikstrom et al. (2011)). As discussed earlier there might be treatment spillovers, i.e. the workers randomized out of the experiment might face changed labor market prospects (more competition, more vacancies, etc.). The implication is that a simple comparison of participants and nonparticipants does not provide a proper estimate for the effect of the activation program. To identify possible spillover effects we use the comparison counties in which the activation program was not introduced. Furthermore, we use the pre-experiment period to control for structural differences between counties.

4.1.1 Duration model

We first focus on the unemployment duration. Consider individuals who are receiving benefits for t units of time (weeks). We assume that differences in exit rates from unemployment can be characterized by observed individual characteristics x_i, the county r_i in which the individual lives, the calendar time moment τ_i of becoming unemployed (experiment or pre-experiment period), and whether or not the individual was assigned to the treatment group d_i or control group c_i of the experiment. In our baseline specification, the exit rate from unemployment is assumed to have the following proportional hazard specification,

$$\theta(t|\tau_i, r_i, x_i, d_i, c_i) = \lambda_{\tau_i}(t) \exp(\alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i)$$

where $\lambda_{\tau_i}(t)$ describes duration dependence, which we allow to be different for individuals who entered unemployment in the experiment period (November 2005 until February 2006) and in the pre-experiment period (November 2004 until February 2005). The parameters α_{r_i} are county fixed effects and β are covariate effects. In the vector of covariates we include gender, nationality and history of benefit receipt, but we also include an indicator for becoming unemployed in November or December to capture possible differences in labor market conditions between the end (Q4) and the beginning (Q1) of a year.

Our parameters of interest are δ and γ, which describe the effect of the activation program on treated and non-treated individuals, respectively. The parameter γ thus describes possible spillover effects. The key identifying assumption for the spillover effects is a common trend in exit rates between the experiment counties and the comparison counties. This assumption is similar to the identifying assumption in difference-in-differences analyses and the common trend is captured in the duration dependence pattern $\lambda_{\tau_i}(t)$. The randomized experiment identifies the difference in exit rates between treated and non-treated individuals in the experiment regions, so $\delta - \gamma$.

To estimate the parameters of interest we use stratified partial likelihood estimation (e.g. Ridder and Tunalı (1999)). The key advantage of stratified partial likelihood estimation is that it does not require any functional form restriction on the duration dependence pattern $\lambda_{\tau_i}(t)$. Let t_i describe the observed duration of unemployment of individual $i = 1, \ldots, n$ and the indicator variable e_i takes the value 1 if an actual exit from unemployment was observed and value 0 if the unemployment duration has been censored. Stratified partial likelihood estimation optimizes the likelihood function

$$\mathcal{L} = \sum_{\tau} \sum_{i \in \mathcal{I}_\tau} e_i \log \left(\frac{\exp(\alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i)}{\sum_{j \in \mathcal{I}_\tau} I(t_j \geq t_i) \exp(\alpha_{r_j} + x_j\beta + \delta d_j + \gamma c_j)} \right)$$

The set $\mathcal{I}_\tau$ includes all individuals who entered unemployment in the same calendar time period (experiment or pre-experiment period) and share the same duration dependence pattern.

The parameter estimates for the specification without any individuals characteristics are shown in column (1) of Table 2. Column (2) shows the estimates from a specification including individual characteristics. Participating in the activation program increases the exit rate from unemployment with $100\% \times (\exp(0.179) - 1) \approx 20\%$ compared to not having any activation program. The effect of the presence of the activation program on the nonparticipants in the program is a reduction in the exit rate of about five percent. The effect on the participants in the program is significant at the one percent level, while the effect on the nonparticipants is only significant at the ten percent level. Our estimate for the difference in exit rates between participants and nonparticipants in the activation program is in line with what has been found before, e.g. Graversen and Van Ours (2008) and Rosholm (2008). The activation program is thus effective in stimulating participants in leaving unemployment, but there is some evidence the program is associated with negative externalities to the nonparticipants. A simple comparison of the participants and nonparticipants would thus overestimate the effectiveness of the activation program.

Next, in column (3) we allow the treatment effects to be different for workers who entered unemployment in the fourth quarter (of 2005) and the first quarter (of 2006). The estimation results show that the estimated effects are very similar. In column (4) we estimate separate treatment effects for South Jutland and Storstrøm. In both counties participation in the activation program increases exit from unemployment. Also in both counties, the activation program reduces the exit rate of the nonparticipants, but only in South Jutland the effect is significant at the five percent level. Rosholm (2008) stressed that the implementation of the activation programs differed between both experiment counties which can explain the different treatment effects in both counties.

In our specification we allowed the duration dependence pattern to be different in both calendar time periods and we included fixed effects for all counties. Alter-

Table 2: Estimated effects of the activation program on exit rate of participants and nonparticipants.

	(1)		(2)		(3)		(4)	
Treated	0.197	(0.028)***	0.179	(0.028)***				
Control	−0.014	(0.028)	−0.048	(0.028)*				
Treated Q4					0.171	(0.037)***		
Treated Q1					0.188	(0.037)***		
Control Q4					−0.047	(0.037)		
Control Q1					−0.049	(0.036)		
Treated SJutland							0.162	(0.040)***
Treated Storstrøm							0.194	(0.038)***
Control SJutland							−0.079	(0.040)**
Control Storstrøm							−0.022	(0.037)
Individual characteristics	no		yes		yes		yes	
County fixed effects	yes		yes		yes		yes	
Observations	89,466		89,466		89,466		89,466	

Note: Standard errors in parentheses. * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Individual characteristics include gender, nationality, labor market history, and quarter of entering unemployment.

natively, we could have included fixed effects for the calender time period and have the duration dependence pattern differ between counties. Repeating the analyses above, shows that the estimated effects of the activation program are not sensitive to the choice of the specification. We also tried restricting the group of comparison counties. We have included only counties closely located to the experiment regions, or located as far away as possible, or counties which are most similar in aggregate labor market characteristics. Our estimation results are very robust to the choice of comparison counties (see Appendix A). This confirms the findings of the Danish economic council (2002) **PAUL, SVP IN REFERENCES:** Danish Economy - Autumn 2002, report by the Danish Economic Council. that only 1% of the unemployed workers and 1.4% of the newly employed workers moved.

Finally, if there would be substantial worker mobility, our estimate of the spillover effect would be an underestimate of the true spillover effect at the given treatment intensity.

4.1.2 Binary outcomes

Above, we used a duration model to investigate the size of the effect of the activation program and the presence of possible spillover effects on nonparticipants in the program. The advantage of a duration analysis is that it uses all information on observed exits. The disadvantage is that some functional form is assumed on the hazard rate. For example, the effect of the activation program on the exit rate from unemployment is assumed to be the constant during the period of unemployment. Therefore, in this subsubsection we consider binary outcomes for finding work.

Let E_i be an indicator for exiting unemployment within a fixed time period. In the estimation, we consider exit within three months, one year and two years. So in the first case, the variable E_i takes value one if individual i is observed to leave unemployment within three months and zero otherwise. The estimate the effect of the activation program on the participants and the nonparticipants, we estimate the linear probability model

$$E_i = \alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i + \eta_{\tau_i} + U_i$$

The parameters α_{r_i} are fixed effects for the different counties and η_{τ_i} are time fixed effect. The framework is thus a difference-in-difference model and the parameters of interest are δ and γ, which are the effects of the activation program on the participants and the nonparticipants, respectively. In the vector of observed individual characteristics x_i, we include the same covariates as in the hazard rates used above. The parametrization of this linear probability model has strong similarities with the duration model.

Table 3: Estimated effects of the activation program on exit probabilities of participants and nonparticipants.

	three months (1)		one year (2)		two years (3)	
Treated	0.070	(0.011)***	0.043	(0.006)***	0.011	(0.004)***
Control	−0.027	(0.011)**	0.002	(0.005)	−0.009	(0.002)***
Individual characteristics	yes		yes		yes	
County fixed effects	yes		yes		yes	
Observations	89,466		89,466		89,466	

Note: Clustered standard errors in parentheses. * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Individual characteristics include gender, nationality, labor market history, and quarter of entering unemployment.

Table 3 shows the parameter estimates for the linear probability model, the standard errors are clustered within counties interacted with the two calendar time periods. First, the size of the treatment effect on the treated becomes smaller for longer unemployment durations, but is always highly significant. The decrease in the size is not surprising. After longer periods the fraction is survivors is reduced substantially and the parameter estimates describe absolute changes in survival probabilities. However, also Graversen and Van Ours (2008), Rosholm (2008) and Vikstrom et al. (2011) describe that the effect of the activation program was largest early during unemployment.

After three months, participants in the program are almost ten percentage point $(0.070 + 0.027)$ more likely to have found work than the nonparticipants, but over one quarter of this difference is due to reduced job finding of the nonparticipants. The effect of the activation program on those randomized out during the experiment is substantial and significant after three months. This describes the period in which the activation program was intense, containing a job search assistance program and frequent meeting with caseworkers. At this period the competition for vacancies was most intense and treatment externalities thus largest. Early in the unemployment spell also relatively many participant in the activation program exit unemployment, which the treatment externalities for the nonparticipants. Indeed, we find that after one year, the effect on the nonparticipants is negligible. After two years, the effect on the nonparticipants is almost as large as the effect on the participants. Both effects are significant, but relatively small. After two years, only slightly of three percent of the participants in the experiment are still unemployed.

4.2 Vacancies

The results in the previous subsection provide some evidence for treatment externalities. A likely channel is that unemployed job seekers compete for the same vacancies, and that an increase in search effort of participants affects the exit to work of other unemployed job seekers in the same local labor market. A more indirect effect may be that when firms realize that unemployed workers make more applications, they will open more vacancies. Both participants and nonparticipants benefit from an increased stock of vacancies. In this subsection we investigate to which extent the stock of vacancies is affected by the experiment.

To investigate empirically whether the experiment affected the demand for labor we consider the stock of vacancies in county r in month t, which is denoted by V_{rt}. We regress the logarithm of the stock on vacancies on time dummies α_t, an indicator for the experiment D_{rt}, and we allow for county fixed effects θ_r,

$$\log\left(V_{rt}\right) = \alpha_t + \delta D_{rt} + \theta_r + U_{rt}$$

Because the dummy variable D_{rt} only takes value one during the experiment, this framework is a difference-in-differences model. The parameter of interest is δ, which describes with which fraction the stock of vacancies changes during the experiment. The key identifying assumption is that the experiment regions and the comparison regions have a common trend, described by α_t, in the changes in the stock of vacancies. Furthermore, the experiment should only affect the local labor market in the experiment counties. If there would be spillovers between counties, δ would underestimate the effect of the experiment on vacancy creation. Finally, since the unit of time is a month, there is likely to be autocorrelation in the error terms U_{rt}. Because the total number of counties equals 14, we report cluster-robust standard errors to account for the autocorrelation (Bertrand et al. (2004)).[2]

Table 4 report the estimation results. Column (1) shows that during the four months of the experiment (November 2005 until February 2006), the stock of vacancies was about 5 percent increased in the experiment counties. But this effect is not significant. The results in column (2) show that the increase in vacancies during the experiment only occurred in South Jutland, and that there was no increase in vacancies in Storstrøm. However, recall that the activation program does not start immediately after entering unemployment, but the workers start the two-week job search assistance program five to six weeks after entering unemployment. Furthermore, it may take time before the stock of vacancies adjust. in the beginning of

[2]The standard errors are based on a generalized version of the White-heteroskedasticity consistent standard errors formula that allows for an arbitrary variance-covariance matrix (White (1980)).

Table 4: Estimated effect of the experiment on logarithm of vacancies.

	(1)		(2)		(3)		(4)	
Experiment	0.047	(0.050)						
Experiment SJutland			0.103	(0.027)***				
Experiment Storstrøm			−0.009	(0.027)				
Experiment nov/dec 2005					0.057	(0.084)	0.007	(0.055)
Experiment jan/feb 2006					0.067	(0.032)*	0.016	(0.032)
Experiment mar/apr 2006					0.081	(0.033)**	0.031	(0.041)
Experiment may/june 2006					0.182	(0.046)***	0.132	(0.034)***
Experiment july/aug 2006					0.114	(0.027)***	0.064	(0.031)*
Experiment sept/oct 2006					−0.049	(0.046)	−0.099	(0.068)
County fixed effects	yes		yes		yes		yes	
Month fixed effects	yes		yes		yes		yes	
Observation period	Jan 04–Dec 07		Jan 04–Dec 07		Jan 04–Dec 07		Jan 05–Dec 06	

Note: Robust standard errors in parenthesis, * indicates significant at 10% level, ** at the 5% level and *** at the 1% level.

the experiment, there are relatively few participants in the experiment among the stock of unemployed job seekers. Also it may take time before firms acknowledge that unemployed workers devote more effort to job search and that it is has become easier to fill a vacancy. And finally, it takes some time to fill a vacancy, so at the start of the experiment that are only very few vacancies, which are actually opened during the experiment, while later the stock of vacancies contain a much higher fraction of vacancies, which are opened during the experiment. Therefore, we allow the effect of the experiment to change over time. The parameter estimates reported in column (3) show that indeed during the experiment the stock of vacancies started to increase in the experiment regions compared to other regions. This effect peaked in May/June, so three to four months after the random assignment stopped and decreased afterwards again. The pattern coincides with the mechanism described above.

The results in column (4) show the same analysis as presented in column (3), but restrict the observation period from January 2005 until December 2006. The pattern in the effects of the experiments on the stock of vacancies remains similar, although fewer parameter estimates are significant. The latter is not only because standard errors are larger, but also estimated effects are slightly smaller. Finally, like in the empirical analyses on unemployment durations, we also restricted the set of comparison counties. The estimated effects vary somewhat depending on the choice of the set of comparison counties. But in general the estimated effects of the experiment increased somewhat as well as the standard errors (the estimation results are provided in Appendix A).

5 Equilibrium analysis of the activation program

The empirical results on the unemployment durations and the stock of vacancies indicate the presence of externalities. Nonparticipants in the experiment have somewhat reduced exit rates from unemployment, and the stock of vacancies increased due to the experiment. In Subsection 2.2, we argued that in the presence of treatment externalities a simple comparison between participants and nonparticipants does not estimate a policy relevant treatment effect. In particular, a large scale role out of the program will change the treatment intensity in the population and thereby the effect of the activation program. In this section we extend the Diamond-Mortensen-Pissarides (DMP) equilibrium search model (see Diamond (1982), Mortensen (1982) and Pissarides (2000)) to analyze how externalities vary with the treatment intensity of the activation program. We calibrate the model such that it matches the estimates we obtained in the previous section given a treatment rate of 30%. We use the calibrated model to estimate the treatment effects for higher treatment rates including the case where the program would be implemented in Denmark as a whole.

5.1 The labor market

Point of departure is a discrete-time matching model in the spirit of Pissarides (2000). We extend the model with an endogenous matching function that allows for search effort. As in Albrecht et al. (2006), we allow unemployed workers to send multiple job applications in each period. Workers are risk neutral and ex-ante identical, they all have the same productivity. They only differ in whether or not they participate in the activation program, which reduces the costs of making a job application. Recall that the goal of the activation program was to stimulate job search effort. The regular meetings did not include elements that could increase human capital or productivity (e.g. Graversen and Van Ours (2008)). Firms are also identical. We focus on symmetric equilibria where identical workers play identical strategies.

When a worker becomes unemployed, the worker receives benefits b and value of non-market time, h, and must decide how many job applications to make. The choice variable a describes the number of applications, which workers make simultaneously within a time period. The length of a period is determined by the time it takes firms to collect and process applications. A worker becomes employed in the next period if one of the job applications was successful, otherwise the worker remains unemployed and has to apply again in the next period. Making job applications is costly, and we assume these costs to be quadratic in the number of applications, i.e. $\gamma_0 a^2$. Wages are determined by Nash bargaining .

An important feature of our model is that we allow the success of an application to depend on the search behavior of the other unemployed workers and the number of posted vacancies. Let $\bar{a}$ describe the average number of applications made by other unemployed workers, u is the unemployment rate and v the vacancy rate (number of open vacancies divided by the size of the labor force). In Subsection 5.2 we derive our matching function and find that it exhibits constant returns to scale. The matching rate for a worker who sends out a applications, $m(a; \bar{a}, \theta)$ is increasing in labor-market tightness $\theta = v/u$ and decreasing in the average search intensity of other workers $\bar{a}$. In Sections, 7.1 and 7.2, we consider a different matching function (Cobb Douglas) and wage mechanism (ex post Bertrand competiotion as in Albrecht et al., 2006) to see how robust our results are for particular modeling assumptions.

Let r be the discount rate and $E(w)$ be the flow value of being employed at a job that pays w. Then the following Bellman equation summarizes the value of the state of unemployment (absent any treatment), Let r be the discount rate. Then the following Bellman equation summarizes the value of the state of unemployment (absent any treatment),

$$U_0 = \max_{a \geq 0} \left[\frac{b}{1+r} + h' - \gamma_0' a^2 + \frac{1}{1+r} (M_u(a; \bar{a}, \theta) E(w) - (1 - M_u(a; \bar{a}, \theta)) U_0) \right]$$

Let $\gamma_0 = (1+r)\gamma_0'$ and $h = (1+r)h'$. Then we can rewrite the value of unemployment as,

$$rU_0 = \max_{a \geq 0} \left[b + h - \gamma_0 a^2 + m(a; \bar{a}, \theta) \left[E(w) - U_0 \right] \right] \tag{2}$$

The optimal number of applications that a worker who is not in the program sends out (a_0^*) follows from choosing the a that maximizes 2 which implies the following first-order condition

$$a_0^* = \frac{E(w) - U_0}{2\gamma_0} \frac{\partial m(a_0^*; \bar{a}, \theta)}{\partial a} \tag{3}$$

The activation program consists of meetings with caseworkers and a job search assistance program which are time-consuming for participants. Therefore, the participants do not have the extra non market time, h that the workers in the control group have. The benefit of the program is that it reduces the costs of making job applications to $\gamma_1 < \gamma_0$. The program did not increase the worker's productivity, see Rosholm (2008). This implies that for participants in the activation program the value of unemployment follows from

$$rU_1 = \max_{a \geq 0} \left[b - \gamma_1 a^2 + m(a; \bar{a}, \theta) \left[E(w) - U_1 \right] \right]$$

Let a_1^* denote the optimal number of applications of a participant in the activation program that follows from

$$a_1^* = \frac{E(w) - U_1}{2\gamma_0} \frac{\partial m(a_1^*; \bar{a}, \theta)}{\partial a} \tag{4}$$

Furthermore, let τ be the fraction of the unemployed workers participating in the activation program. Since we focus on symmetric equilibria, the average number of applications of all unemployed workers within the population equals $\bar{a} = \tau a_1^* + (1 - \tau) a_0^*$.

The aim of our model is to describe the behavior of unemployed workers. Therefore, we keep the model for employed workers as simple as possible, and we ignore on-the-job search. This is also motivated by data restrictions, our data do not contain any information on post-unemployment outcomes, such as wages and job-to-job transitions.

Next, consider the value of the state of employment. With probability δ a job is destroyed and the employed worker becomes unemployed again. When being employed, the worker does not know whether or not she will enter the activation program once she becomes unemployed. This implies that when wages are determined, the employers consider $\bar{U} = \tau U_1 + (1+\tau)U_0$ as the relevant outside option. Since we assumed that wages are paid at the end of the period, the Bellman equation for the state of employment at wage w can be written as,

$$rE(w) = w - \delta\left[(E(w) - \bar{U})\right] \tag{5}$$

Vacancies are opened by firms but this is costly. For a firm, the costs of having an open vacancy are c_v per period. The probability of filling a vacancy depends on the average job application behavior $\bar{a}$ of the unemployed workers and on labor market tightness θ. The probability of filling a vacancy is (given that the matching function exhibits constant returns to scale), $\frac{m(\bar{a},\theta)}{\theta}$, which we derive below. Once a vacancy is filled, the flow value to the firm equals J depends on the productivity p of the worker, the wage w and the job destruction probability δ,

$$rJ = p - w - \delta(J - V) \tag{6}$$

Each period that a job exists, the firm receives the value of output p minus wage cost w. With probability δ the job is destroyed and the job switches from filled to vacant. Of course, firms can also choose to not open a vacancy after a job is destroyed. But as we discuss below, due to free entry the value of a vacancy will be zero in equilibrium. The Bellman equation for a vacancies is

$$rV = -c_v + \frac{m(\bar{a},\theta)}{\theta}(J - V) \tag{7}$$

5.2 Wages and the matching function

Wages are determined by Nash bargaining. The bargaining takes place after the worker and firm meet. We assume that firms do not observe whether or not the unemployed worker participates in the activation program. Consequently, firms do not observe search intensity nor the worker's disutility of program participation. Therefore, firms assign the same (average) outside option to all workers when bargaining. Note that if wages are continuously renegotiated, all employed workers will have the same outside option and earn the same wage. Let β denote the bargaining power of the workers, then the generalized Nash bargaining outcome implies

$$w^* = \arg\max_w (E(w) - \bar{U})^\beta (J(w) - V)^{1-\beta}.$$

with the following first-order condition,

$$\beta(p - w) = (1-\beta)(w - r\bar{U})$$

Define the per-period payoffs for unemployed individuals by $\pi_0 = b + h - \gamma_0 a_0^2$ and $\pi_1 = b - \gamma_1 a_1^2$ then, equilibrium wages are given by

$$w = \frac{\beta p\Big[(r+\delta)(r+m_0+m_1)+m_0 m_1 - \delta\overline{m}\Big] + (1-\beta)\Big[(1-\tau)m_1\pi_0 + \tau m_0 \pi_1 + r\overline{\pi}\Big]}{(r+\delta)(r+m_0+m_1-\overline{m}) + \beta(r\overline{m}+m_0 m_1)} \tag{8}$$

$\overline{m}$ and $\overline{\pi}$ indicate population averages. Inspection shows that the wage level increases in the productivity of a match (p) and in the (average) net flow income of unemployment (π_0 and π_1), which increases the outside option of the worker.

In Appendix B we discuss the wage mechanism of Albrecht et al. (2006) where workers with multiple offers have their wages bid up by Bertrand competition. But it should be noted that this mechanism for wage determination gives very similar results in terms of labor market flows, vacancy creation and the effect of the treatment.

Finally, we have to specify the matching functions $m(a;\bar{a},\theta)$ for unemployed worker and $\frac{m}{\theta}(a;\bar{a},\theta)$ for firms. Since the treatment reduced the search cost, we want a matching function that allows for different search intensities of participants and nonparticipants in the activation program. Moreover, it should allow for congestion effects between unemployed job seekers. Below we adjust the matching function of Albrecht et al. (2006) to allow for different search intensities. Delays in finding a job are the result of two coordination frictions: (i) workers do not know where other workers apply to and (ii) firms do not know which candidates are considered by other firms.

If a firm receives multiple applications, it selects one of the applicants at random and makes an offer to this worker. The other applications are turned down. A worker who receives only one job offer accepts the offer and matches with the firm. If a worker receives multiple job offers, the worker randomly selects one of the offers and matches with the firm.

The expected number of applications per vacancy is given by

$$\frac{u(\tau a_1^* + (1-\tau)a_0^*)}{v} = \frac{\bar{a}}{\theta}$$

If the number of unemployed workers and the number of firms are sufficiently large, then the distribution of number of applications to a specific vacancy can be approximated by a Poisson distribution with mean $\bar{a}/\theta$. For a worker, an application results in a job offer with probability $\frac{1}{1+i}$, where i is the number of competitors for that job (which is the number of other applications to the vacancy). This implies that the probability that an application results in a job offer equals

$$\psi = \sum_{i=0}^{\infty} \frac{1}{1+i} \frac{\exp(-\bar{a}/\theta)(\bar{a}/\theta)^i}{i!} = \frac{\theta}{\bar{a}}\left(1 - \exp\left(-\frac{\bar{a}}{\theta}\right)\right)$$

The matching probability of a worker who makes a applications is thus given by

$$m(a;\bar{a},\theta) = 1-(1-\psi)^a = 1-\left(\frac{\bar{a}-\theta}{\bar{a}} - \frac{\theta}{\bar{a}}\exp\left(-\frac{\bar{a}}{\theta}\right)\right)^a$$

Once we substitute for a the optimal number of application a_1^* and a_0^* we obtain the matching functions for the participants and the nonparticipants in the activation program, respectively.

The aggregate matching function is first increasing in the number of applications per worker and then decreasing. More applications per worker reduce the first coordination problem but amplify the second one. For a given level of market tightness there is a threshold search intensity above which the negative effects of (ii) dominate the reduction in (i).

5.3 Equilibrium

In this subsection we derive the steady state equilibrium of out search model. In a steady state equilibrium, the inflow into unemployment equals the outflow from unemployment. Furthermore, the asset value of a vacancy is zero under free entry. In steady state, the inflow into unemployment equals the outflow from unemployment, which gives

$$\delta(1-u) = (\tau m(a_1^*;\bar{a},\theta) + (1-\tau)m(a_0^*;\bar{a},\theta))u$$

The equilibrium unemployment rate is, therefore,

$$u^* = \frac{\delta}{\delta + \tau m(a_1^*;\bar{a},\theta) + (1-\tau)m(a_0^*;\bar{a},\theta)}$$

The zero-profit condition implies that the flow value of a filled vacancy equals

$$J = \frac{p-w}{r+\delta}.$$

Substituting this into the Bellman equation for vacancies (7) gives

$$\frac{m}{\theta}(\bar{a},\theta) = \frac{(r+\delta)c_v}{p-w(\theta)} \tag{9}$$

Since the left hand side is decreasing in θ, goes to infinity for $\theta \to 0$ and the right hand side is increasing in θ because wages are increasing in θ, there is a unique θ^* that satisfies (9). We can now define the equilibrium as the tuple $\{a_0^*, a_1^*, w, \theta^*\}$ that satisfies equations, (3), (4), (8) and (9).

5.4 Welfare

Normalize output to one and let c_p be the cost per worker of participating in the program. The net output in this economy (that we use as a welfare measure) is given by

$$\Omega(\tau) = (1-u)\cdot 1 + u\big[(1-\tau)(h' - \gamma_0' a_0^{*2}) + \tau(-\gamma_1' a_1^{*2})\big] - (1-u)\delta\tau c_p - v c_v \quad (10)$$

Note that we take both the time cost and the cost of the program into account and that UI benefits do not enter the welfare function because these are just a redistribution issue. After we have estimated the primitives of the model, we can see whether the experiment increased welfare, i.e. $\Omega(0.3) - \Omega(0) > 0$, what the effects would be of an economy-wide implementation, $\Omega(1) - \Omega(0) > 0$ and what the optimal level of τ is.

6 Evaluation of the equilibrium search model

In this section we first discuss how we estimate our equilibrium search model by indirect inference (see Smith (1993) and Gourieroux et al. (1993)) using the treatment effects estimated in section 4 as our auxilary model. Next, we use the calibrated model to learn what the effects of the program would be in case of an economy wide implementation. Finally, we look at the aggregate welfare effects of the program.

6.1 Parameter values

By the nature of the urn-ball matching function, the equilibrium search model is in discrete time. The length of a time period is determined by the time it takes for firms to collect and process applications which we set equal to one month. Next, we fix the treatment intensity, τ, to 0.3 which is motivated by the fact that at the end of the experiment period about 30% of the unemployed workers in the experiment regions were assigned to the activation program. We set the discount rate equal to 10% annually, which implies that r is 0.008. This is a bit smaller than the discount rates used by, for example, Lise et al. (2004) and estimated by Frijters and Van der Klaauw (2006). Productivity is normalized to one. The upper panel of Table 5 summarizes the values for the model parameters that we fix a priori.

Next, we use indirect inference to estimate the remaining model parameters. These parameters are chosen such that a set of data moments is matched as closely as possible by the model. The moments that we want to match are presented in table 6. The model should capture the unemployment rate and vacancy rate from

Table 5: Parameter values.

Fixed parameter values		
r	0.008	Annual discount rate equals 10%.
p	1	Productivity normalized to 1.
Estimated parameter values		
γ_0	0.20	(cost of sending an application for untreated workers)
γ_1	0.11	(cost of sending an application for treated workers)
δ	0.01	(job destruction rate)
h	0.02	(value non-market time for untreated unemployed))
c_v	0.83	(per period cost of posting a vacancy)
b	0.64	(UI benefits)
β	0.75	(bargaining power)

the data, the estimated treatment effect on the treatment group and on the control group, the estimated effect on vacancies, the average matching rate in the experiment counties and finally the fact that unemployment benefits are approximately 65% of the wage level. Define $\xi = (h, \gamma_0, \gamma_1, \delta, c_v, b, \beta)$ as the vector of parameters to be estimated. Given values for ξ the model can be solved and the set of moments can be computed. To obtain estimates for ξ, we minimize the sum of squared differences between the data and model moments over ξ, where each moment condition is given an appropriate weight based on the variance of the data moment estimate and the economic significance. The latter is justified by the fact that for the question at hand we want the model to particularly be able to match the unemploymet rate, the treatment effect of the treated and the treatment effect of the control group well. The estimates for ξ are presented in the lower panel of table 5. We find that the cost of sending applications is lower for treated individuals than for untreated individuals, job destruction is 1% per month, unemployment benefits are 64% of productivity, bargaining power of workers is 0.75 and the value of non-market time is 2% of productivity.

6.2 Increasing the fraction of treated workers

Recall from section 2 that the costs of the activation program are about 13.2 percent of the average monthly UI benefits level.

We can now use the model to predict how the treatment effect depends on the share of the population receiving treatment. We are interested in the effect of the

Table 6: Moment conditions

	Data moment	Description	Corresponding value model
Unemployment rate	5.0%	Unemployment rate Storstrøm and South Jutland during the experiment (see Table 1)	$u\|\tau = 0.3$
Treatment effect on log vacancies	0.081	Estimated percentage effect on vacancies 5-6 months after the beginning of the experiment (see table 4)	$\frac{v\|\tau=0.3 - v\|\tau=0}{v\|\tau=0}$
Treatment effect on treated	0.07	Estimated effect (see table 3)	$[1-(1-m_1\|\tau=0.3)^3]-[1-(1-m_0\|\tau=0)^3]$
Treatment effect on control	-0.027	Estimated effect (see table 3)	$[1-(1-m_0\|\tau=0.3)^3]-[1-(1-m_0\|\tau=0)^3]$
Outflow rate after 3 months	0.51	Fraction of unemployed in Storstrøm and South Jutland that left unemployment within three months	$1-(\tau(1-m_1\|\tau=0.3)^3+(1-\tau)(1-m_0\|\tau=0.3)^3)$
Vacancy rate	0.01	Approximation of the number of vacancies as a percentage of the labour force in Storstrøm	$v\|\tau = 0.3$
Replacement rate	65%	Unemployment benefits are around 65% of the wage level	$\frac{b}{w}$

treatment on the matching rate of both the treated and the untreated workers, aggregate unemployment, the vacancy rate and the wage level. All parameters that we have either fixed or estimated are assumed to be constant at their values given in table 5. Simulation allows for predictions of the sign and magnitude of externalities of treatment.

We solve the model for a gradually increasing share of treated workers (τ). The results are shown in Figure 5. Unemployment decreases slightly as τ increases up to 0.9, but increases when τ is increased to 1. The difference between no treatment and 90% treatment is about 0.13 percentage points in unemployment. This is a decrease of approximately 2.5 %.[3] The matching rates of both the treated and the untreated decrease monotonically, while treated individuals have a 29% higher matching rate than untreated for $\tau = 0.3$. As a result, the average matching rate increases slightly in τ. Note that both the small decrease in matching rate for the untreated over $\tau = 0 - 0.3$ and the difference in matching rates of the treated and untreated are very close to the empirical estimates from section 4. In table 7 we compare the model estimates for matching rates with the results from the empirical analysis. The matching rates from the data at $\tau = 0.3$ are simply the average matching rates observed in the treatment and control group during the experiment. The matching rate at $\tau = 0$ is the counterfactual which we calculate based on the spillover effect estimate from the linear model (see table 3). The data and simulated matching rates are close, and especially the change in matching rate for the untreated is captured well by the model. For higher values of τ the model predicts that matching rates for both groups continue to decrease, as externalities such as congestion in the matching process become larger.

Next, the vacancy rate increases, as was estimated empirically, but the size of the increase is small. The wage decreases, but also this change is very small. These changes reflect that a high treatment rate leads to a large share of the unemployed searching more intensively and thus matches occur slightly faster on average. The negative congestion externalities occurring in the matching process become larger as τ increases as can be inferred from the decreasing matching rates.

6.3 Treatment effects

From these results, the treatment effects can be inferred. The treatment effect of interest is the change in the matching rate when τ is increased from 0 to 1. As

[3] It is important to notice that the simulation results indicate steady state values of the variables. This implies that the simulated rate of unemployment would be achieved if the corresponding share τ of unemployed would be treated for a long enough period. Therefore, results could be different in the short run as the labor market would be out of equilibrium.

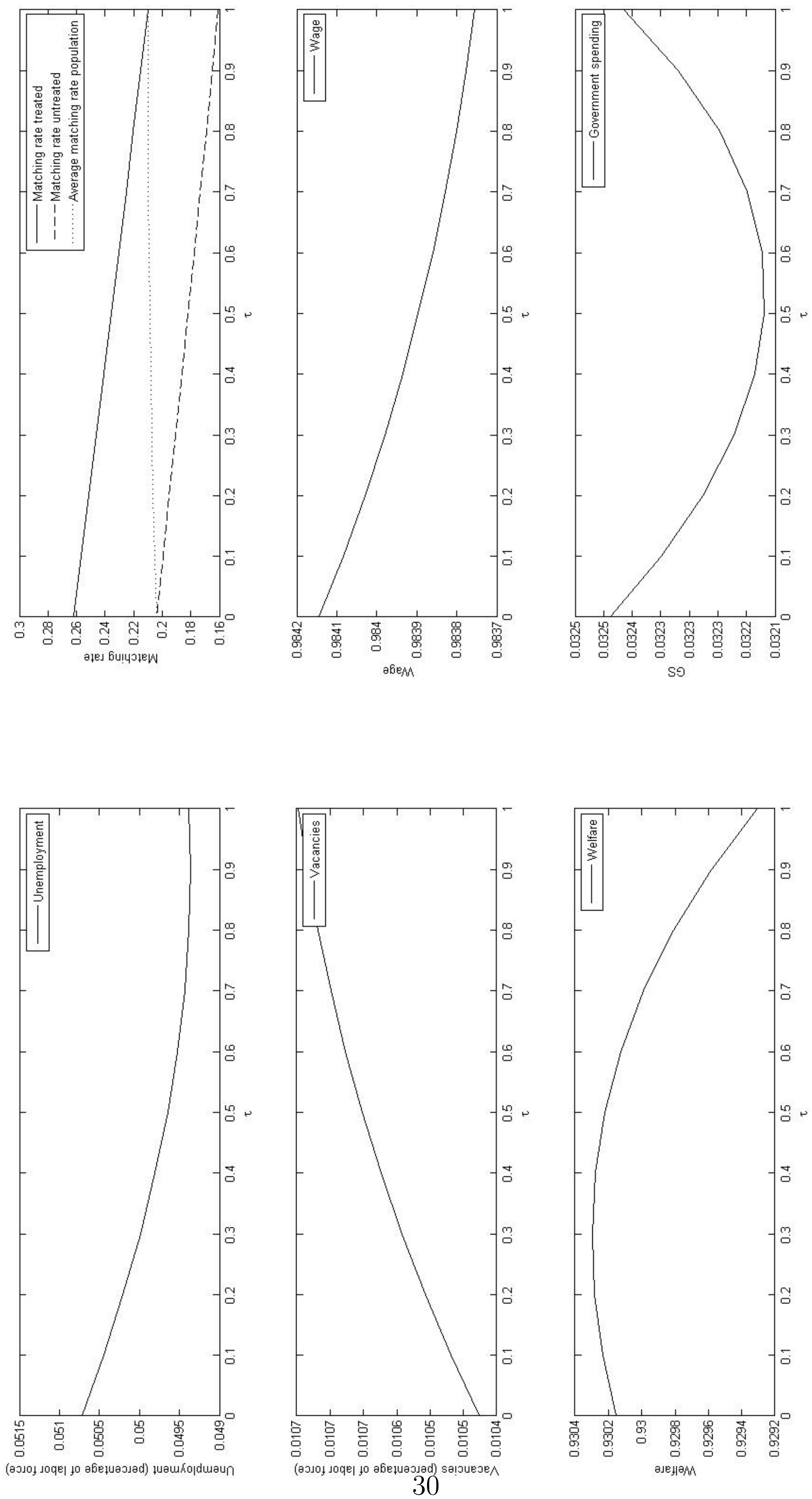

Unemployment
Unemployment (percentage of labor force)
Vacancies
Vacancies (percentage of labor force)
Welfare
Welfare
Matching rate treated
Matching rate untreated
Average matching rate population
Matching rate
Wage
Wage
Government spending
GS
τ

Table 7: Empirical and simulated matching rates

	$\tau = 0$	$\tau = 0.3$	$\tau = 0.5$	$\tau = 1$
m_n (Data)	0.182	0.169	-	-
m_n (Simulated)	0.204	0.191	0.182	0.161
m_t (Data)	-	0.238	-	-
m_t (Simulated)	0.262	0.246	0.235	0.210

Note: $m_n|\tau = 1$ and $m_t|\tau = 0$ do not exist in reality, but the model can still predict these values.

can be seen in the graph, the increase in average matching rate is around 0.006 (this is the total increase in the *average* matching rate in the graph). This is an increase of almost 3%. In the experiment approximately 30 % received treatment. The estimated effect that a simple comparison of treatment and control would give at $\tau = 0.3$ is much larger, around 0.06, which is a 29% increase. This difference is rather stable as τ changes. We draw two main conclusions from this analysis. First of all a comparison of the control and the treatment group overestimates the true treatment effect. Second, the change in the average matching rate is decreasing in the share of the population that receives treatment.

6.4 Cost-benefit analysis and welfare

The model predictions allow for different types of cost-benefit analyses. First of all, a standard cost-benefit analysis from the perspective of the policy maker would be to compare the difference in matching rates (or the corresponding average unemployment durations) of the control and treatment group with the cost of the program. More precisely, one could compute:

$$\Delta \text{GS} = \tau u(c_{program} - \Delta \text{duration}_{treated} \cdot b) \tag{11}$$

Which is, the change in government spending equals the number of treated individuals multiplied by the difference between the cost of the program and the savings in terms of unemployment benefits paid. The cost of the program ($c_{program}$) is 2122 DKK, while the change in average unemployment duration is 0.42 months. Average monthly benefit payments are 14800 DKK. The gain for the government budget is therefore 4094 DKK per treated worker, independent of the share of treated workers. This analysis, however, ignores externalities. Alternatively, a cost benefit analysis should take into account the change in matching rate of the control group. In equilibrium, total government spending equals benefit payments and program payments:

$$\text{GS} = ub + \tau(1-u)\delta c_{program} \tag{12}$$

We find that this expression attains a minimum at $\tau = 0.5$, such that 50% participation in the program would be optimal for the government budget. This is shown in figure 5. Finally, we could compute welfare as defined in section 5.4:

$$\Omega(\tau) = (1-u)\cdot 1 + u\big[(1-\tau)(h' - \gamma_0' a_0^{*2}) + \tau(-\gamma_1' a_1^{*2})\big] - (1-u)\delta\tau c_p - vc_v \tag{13}$$

Also Ω is plotted in figure 5. We find that welfare is maximized for $\tau = 0.3$.

6.5 Summary

To sum up, we find that the treatment leads to negative externalities for both the treatment and the control group. The average matching rate increases slightly, but the matching rate of both the treated and the untreated decreases as the share of treated increases. Unemployment is minimal when 90% of the unemployed participate in the program. Comparing the control group with the treatment group overestimates the treatment effect, and the treatment effect decreases in τ. Vacancies show a small increase. A standard cost-benefit analysis ignoring general equilibrium effects suggests a large positive net-benefit for government spending for each participant in the program, such that $\tau = 1$ is optimal. Inclusion of externalities leads to the conclusion that treatment should be 50% in order to minimize government expenditure. Finally, welfare is maximized at $\tau = 0.3$. In the next section we perform two robustness checks to investigate whether these results are driven by some of the model assumptions.

7 Robustness checks

7.1 A different matching function

All previous results are based on the model with a specific choice for the matching function. This choice may be responsible for some of the outcomes. To investigate the robustness of the results the same analysis is performed with a different matching function. A standard, basic matching function of the form $m(u,v) = u^{\alpha}v^{(1-\alpha)}$ is assumed. This function is of the common Cobb-Douglas form and is homogeneous of degree one. The matching rate for unemployed can be written as $\frac{m(u,v)}{u}$. The form is adapted to incorporate a distinction between treated and untreated workers. This is done by including a search intensity variable (s) which is, as before, chosen optimally by the worker. Again we expect a different search intensity for the treated

(s_t) and untreated (s_n) workers. The population average search intensity is denoted by $\overline{s}$. This leads to:

$$
\begin{aligned}
m(su, v) &= (su)^{\alpha} v^{1-\alpha} \\
m_t &= \frac{s_t}{\overline{s}u} m(\overline{s}u, v) = s_t \left(\frac{\theta}{\overline{s}}\right)^{1-\alpha} \qquad (14) \\
m_n &= \frac{s_n}{\overline{s}u} m(\overline{s}u, v) = s_n \left(\frac{\theta}{\overline{s}}\right)^{1-\alpha} \qquad (15)
\end{aligned}
$$

We are required to fix the elasticity of matches with respect to unemployment (α). An extensive literature exists on the empirical estimation of this matching function. A survey is performed by Petrongolo and Pissarides (2001), showing that estimates of α range from 0.29 to 0.70 depending on the country and the estimation approach. A study by Albaek and Hansen (1995) even estimates $\alpha = 0.74$ for Denmark, however this estimate is based on data from 1974-1988. Since we are not aware of any more recent estimates this value is used in our model. Moreover, we check the importance of the parameter value for the simulation results and find that results are not very sensitive to the choice for α.

Given this alternative matching function, we calibrate the model again and find parameter estimates for ξ. Next we simulate the model to find the effect of increasing τ. The results are shown in Figure 6. As before, we find that unemployment decreases if the scale of treatment is increased. The difference in unemployment between no treatment and full treatment is 0.6 percentage points, which is larger than before. Negative externalities lead to decreasing matching rates for both the treated and untreated workers. The matching rates are again very close to the estimated data moments.

The externalities are smaller than in the urnball-matching function case, and the increase in the average matching rate is larger. This causes the stronger decrease in unemployment. Vacancies increase at a rate very close to the estimations in section 4. The wage level decreases monotonically. Search intensity of both groups of unemployed workers decreases. Overall the main results (unemployment, matching rates and vacancies) are quite similar to previous analysis, only the magnitude of the externalities is somewhat smaller, leading to a stronger decrease in unemployment after an increase in treatment. This is due to the matching function that does nog incorporate both coordination frictions that are present in the multiple applications-urnball matching function.

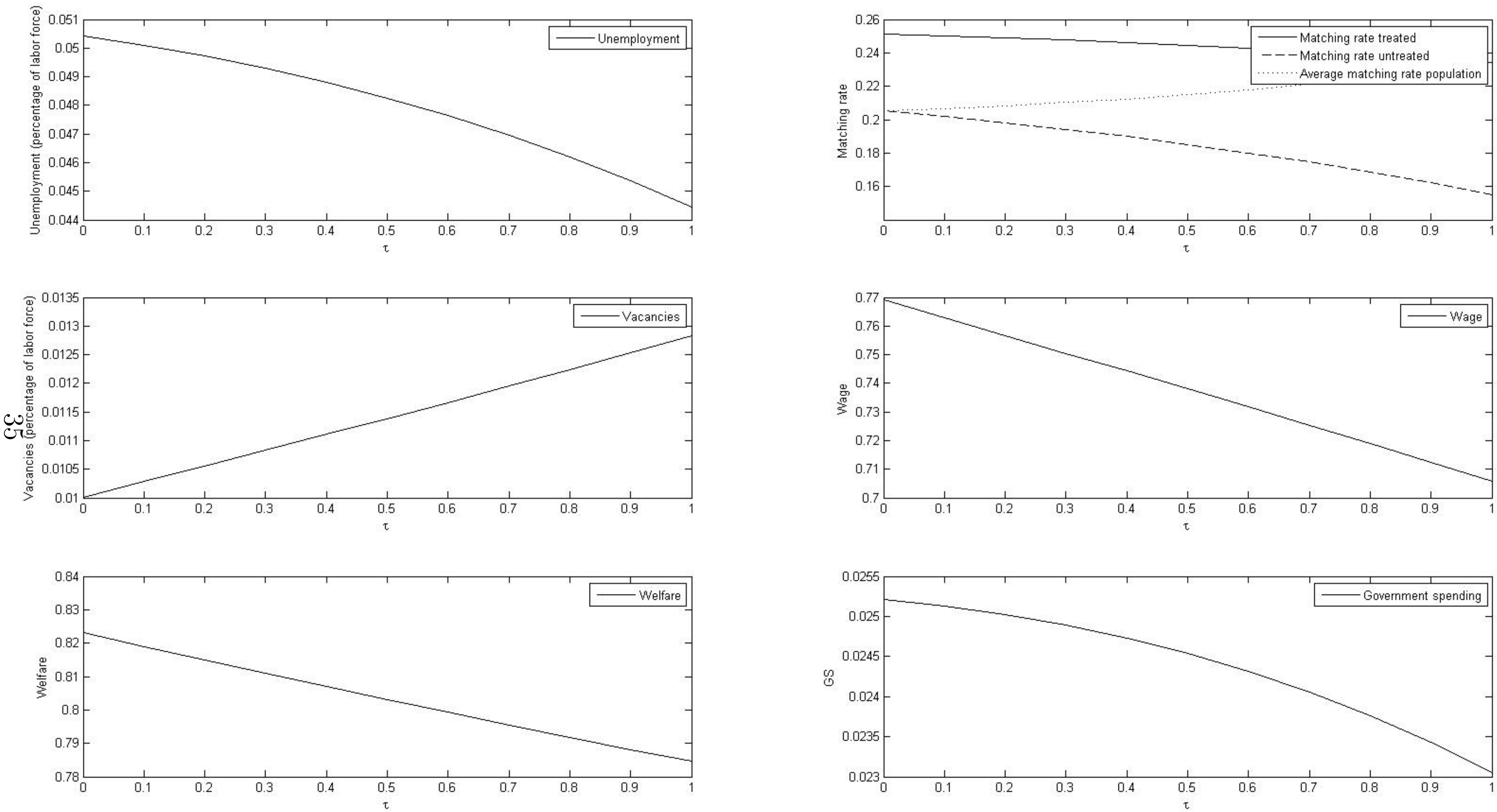
A cost-benefit analysis ignoring spillovers (as defined in equation 11) again leads to the conclusion that full treatment is optimal. The second cost-benefit analysis (see equation 12), which includes spillover effects, is graphed in figure 6 and also suggests that $\tau = 1$ is optimal from the governments perspective. Finally, the welfare analysis (see equation 10) is also graphed in figure 6 and shows that welfare is monotonically decreasing in τ such that no treatment is optimal.

7.2 Bertrand competition for workers

So far the wage has been determined by Nash bargaining. An alternative is to assume Bertrand competition for a worker to determine the wage level. Therefore we modify the model by replacing Nash bargaining with Bertrand competition (similar to for example Albrecht et al. (2006)), to see whether the results are sensitive to the wage mechanism.

Bertrand competition implies the following. If a worker receives only one job offer, he is forced to accept this offer, such that the firm will pay him the minimum wage possible. This wage equals the reservation wage of the worker. If the worker receives multiple offers, firms have the possibility to raise their wage offers in order to attract the worker. The firms continue overbidding each other until the wage equals to the maximum value possible, which is p (productivity). Let j indicate the number of job offers that an worker receives. The probability of receiving a job offer offering the reservation wage equals $P(j = 1)$, while the probability of an offer with wage equal to productivity is $P(j > 1)$. Clearly these probabilities depend on the number of application a worker sends (or, equivalently, the worker's search intensity). A complete derivation of the model under Bertrand competition is provided in the appendix. Here we simply describe the results.

Using the modified model, we can again simulate the model for increasing values of τ, the share of treated unemployed workers. The effect of τ on unemployment, vacancies, the matching rates, the reservation wage, the share of workers receiving the high wage and the search intensity of the unemployed is shown in Figure 7. The results are quite similar to the outcomes under Nash bargaining. We find again that the matching rates decrease when τ is increased. This is partly explained by the decrease in search intensities of both the treated and the untreated. Vacancies show a small increase, while the reservation wage decreases slowly. Most importantly, the decrease in unemployment is similar, though slightly larger than under Nash bargaining. The difference between no treatment and full treatment is 0.8 percentage points in unemployment. Furthermore we plot the share of workers receiving the high wage (equal to productivity) and find that this share is much higher for the treated, but it decreases for both groups as τ increases. We conclude that the results are similar to the results under Nash bargaining, such that the wage process is not driving the outcomes.



Unemployment (percentage of labor force)
Unemployment
Matching rate
Matching rate treated
Matching rate untreated
Average matching rate population
Vacancies (percentage of labor force)
Vacancies
Wage
Welfare
GS
Government spending
τ

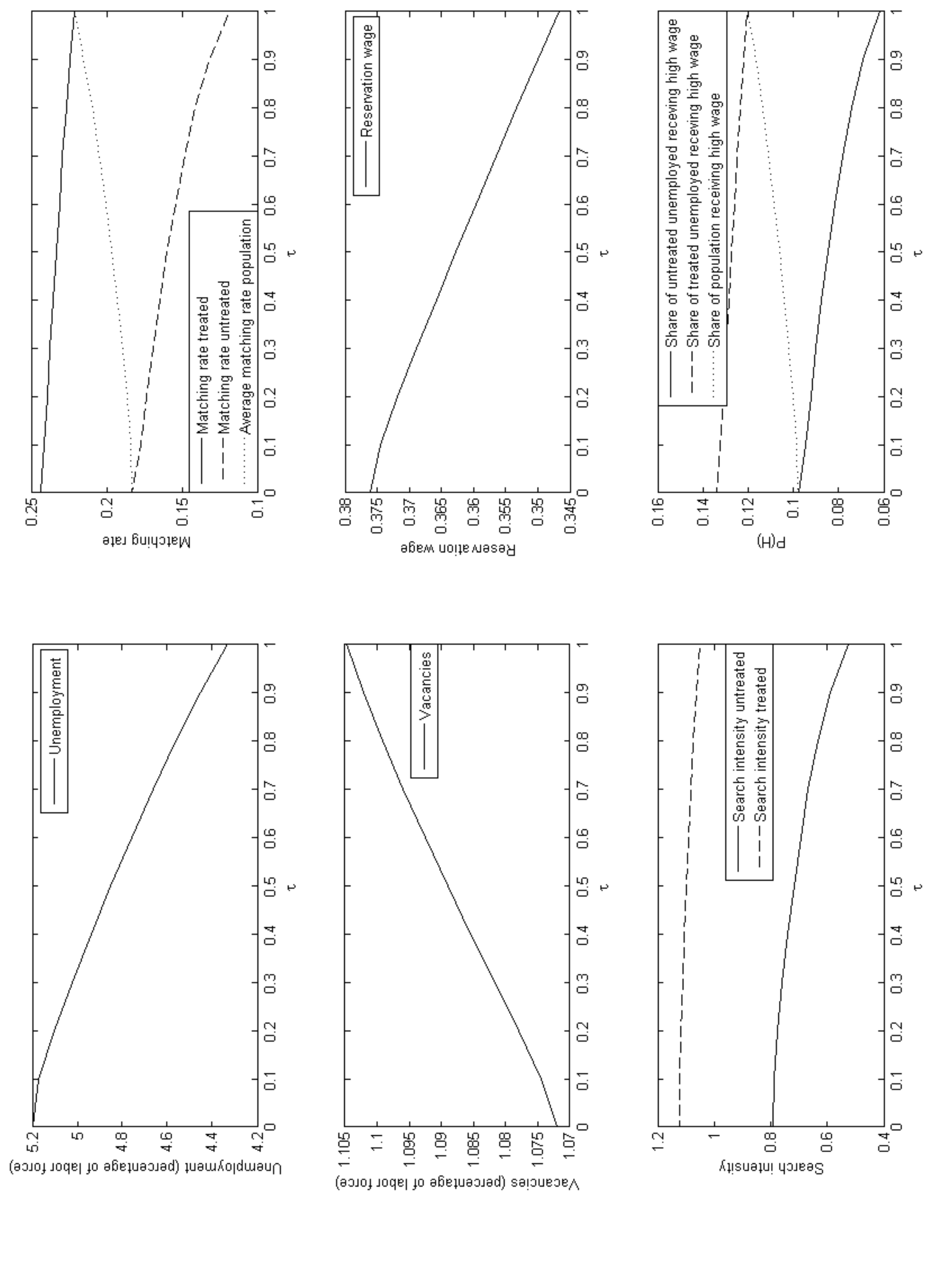


Figure 7: Simulation results due to changes in τ (Bertrand wages)

8 Conclusion

In this paper we investigate the existence and magnitude of externalities of job search assistance. We investigate the treatment effect and the effect on the control group taking into account equilibrium effects. Using data from a randomized experiment we find evidence of negative spillovers, implying that estimates of the treatment effect ignoring equilibrium effects overestimate the real effect. To predict outcomes when, instead of 30 percent, a much larger share of the population would receive treatment we develop a search model incorporating treatment. After calibrating the model to the data, the model predicts a small increase in vacancies and a decreasing matching rate for both the treated and untreated workers, when the share of treated is increased. This is in line with the empirical findings. Simulation shows that these effects are persistent when the share of treated is increased up 100 percent. Unemployment is predicted to decrease with approximately 2.5% percentage when τ is increased to 0.9. These results are robust to a different matching function and to a different wage mechanism. The outcome is interesting because it suggests that the benefits of implementing the program on a larger scale could be smaller than a simple estimate would predict. Still, unemployment decreases, such that determining whether treatment on such a scale is desirable, depends on the per-person cost of the treatment and the social value of reducing unemployment. We perform a simple cost-benefit analysis ignoring spillovers, a cost-benefit analysis for government expenditure which incorporates spillovers and a welfare analysis. We find that ignoring spillovers suggests that full treatment is optimal. Inclusion of spillovers leads to minimium government expenditure for $\tau = 0.5$. Finally, welfare is maximized for $\tau = 0.3$.

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A Empirical analyses with restricted comparison counties

In section 4 we presented our empirical results. These were based on comparing the experiment counties with all other Danish counties. Both the pre-experiment period and the experiment period are characterized by solid economic growth and decreasing unemployment rates. There is no reason to believe that one of the experiment counties experienced an idiosyncratic shock which might have affected labor market outcomes. In this appendix we consider the robustness of our empirical results with respect to the comparison counties. **AANVULLEN**

B Equilibrium search model with Bertrand competition

A different wage mechanism is proposed by Albrecht et al. (2006), based on Bertrand competition. Under Bertrand competition it is assumed that if a worker receives offers from multiple vacancies, the firms compete over his services, thereby increasing the wage up to productivity ($w = p$). On the other hand, if a worker only receives one offer, the wage equals the worker's reservation wage ($w = w_R$). Therefore the wage depends on the number of offers (denoted by j). The probability of getting the low (reservation) wage (given a match) is:

$$\begin{aligned} p_l(a) &\equiv \Pr(j = 1 | j > 0) \\ &= \frac{\Pr(j = 1)}{\Pr(j > 0)} \end{aligned} \tag{16}$$

The binomial probabilities as used so far lead to problems when a is not discrete. [4] So we assume that the number of job offers for an worker follows a Poisson distribution with mean λ.

Since a is not necessarily an integer, we abstain from interpreting it as the number of applications, but simply use it as a measure of search intensity. It may for example indicate the number of applications multiplied by the probability that an application is of high enough quality to be taken into account by the firm. λ equals the mean of the poisson distribution, which is the expected number of offers. Therefore we take $\lambda_n = \psi a$, which is search intensity multiplied by the probability

[4]For $a \in (0, 1))$ the maximum number of offers is one, such that everyone receives their reservation wage and no worker has an incentive to search anymore. This is the Diamond paradox.

Table 8: Estimated effects of the activation program on exit rate of participants and nonparticipants with restricted comparison groups.

	(1) 3 closest counties	(2) 3 closest counties	(3) 2 furthest counties	(4) 2 furthest counties	(5) 5 most similar counties	(6) 5 most similar counties
Treated	0.219 (0.030)***		0.192 (0.031)***		0.201 (0.029)***	
Control	−0.011 (0.030)		−0.040 (0.031)		−0.028 (0.028)	
Treated Sjutland		0.203 (0.042)***		0.175 (0.042)***		0.183 (0.041)***
Control Sjutland		−0.041 (0.042)		−0.070 (0.042)*		−0.059 (0.040)
Treated Storstrøm		0.233 (0.040)***		0.207 (0.040)***		0.216 (0.039)***
Control Storstrøm		0.015 (0.039)		−0.014 (0.039)		−0.000 (0.038)
Ind. characteristics	yes	yes	yes	yes	yes	yes
County fixed effects	yes	yes	yes	yes	yes	yes
Observations	32,723	32,723	29,378	29,378	61,715	61,715

Note: Robust standard errors in parenthesis, * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Closest counties are West-Zealand, Ribe and Funen, furthest counties are Viborg and North-Jutland, most similar counties are Funen, West-Zealand, North-Jutland, Viborg and Aarhus.

Table 9: Estimated effects of the experiment on the logarithm of vacancies with restricted comparison groups.

	(1) 3 closest counties		(2) 2 furthest counties		(3) 5 most similar counties	
Experiment Nov/Dec 2005	0.092	(0.094)	0.039	(0.168)	0.039	(0.098)
Experiment Jan/Feb 2006	0.127	(0.023)***	0.025	(0.144)	0.089	(0.060)
Experiment Mar/Apr 2006	0.146	(0.035)**	0.014	−0.074	0.106	(0.049)*
Experiment May/Jun 2006	0.158	(0.068)*	0.088	(0.053)	0.120	(0.049)*
Experiment Jul/Aug 2006	0.079	(0.069)	0.185	(0.033)**	0.095	(0.046)*
Experiment Sep/Oct 2006	0.009	(0.108)	−0.043	(0.040)	−0.066	(0.038)
County fixed effects	yes		yes		yes	
Month fixed effects	yes		yes		yes	
Observation period	Jan 04-Dec 07		Jan 04-Dec 07		Jan 04-Dec 07	

Note: Robust standard errors in parenthesis, * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Closest counties are West-Zealand, Ribe and Funen, furthest counties are Viborg and North-Jutland, most similar counties are Funen, West-Zealand, North-Jutland, Viborg and Aarhus.

that one unit of search intensity leads to an offer (as before we have $\phi = \frac{(1-\tau)a+\tau a'}{\theta}$ and $\psi = \frac{1}{\phi}(1-\exp(-\phi)))$. Similarly we have $\lambda_t = \psi a'$.

We can then derive the probability of receiving one offer, given a match, as

$$p_l(a) = \frac{\lambda_n \exp(-\lambda_n)}{1-\exp(-\lambda_n)}$$

Similarly we have the probability of getting the high wage (equal to productivity), given a match:

$$\begin{aligned} p_h(a) &= \Pr(j > 1 | j > 0) \\ &= \frac{\Pr(j > 1)}{\Pr(j > 0)} \\ &= \frac{1-\exp(-\lambda_n) - \lambda_n \exp(-\lambda_n)}{1-\exp(-\lambda_n)} \\ &= 1 - p_l(a) \end{aligned} \tag{17}$$

The same holds for the treated workers (with a replaced by a' and λ_n replaced by λ_t). Given these probabilities the value functions for employment can be adapted. Equation 5 is replaced by:

$$E_l = w_R + \frac{1}{1+r}(\delta U + (1-\delta)E_l) \tag{18}$$

$$E_h = p + \frac{1}{1+r}(\delta U + (1-\delta)E_h) \tag{19}$$

With E_l the value of being employed at the reservation wage and E_h the value of being employed with wage equal to productivity. While being unemployed, a worker uses the expectation of the value of employment, which depends on the probabilities of getting the high or the low wage. In turn these probabilities depend on the search intensity, such that the expected value of employment is different for untreated and treated workers. These expected values are:

$$E(a) = p_l(a)E_l + p_h(a)E_h \tag{20}$$

$$E(a') = p_l(a')E_l + p_h(a')E_h \tag{21}$$

Furthermore, and extra equation is required to define the reservation wage. The reservation wage is the wage level at which the value of employment equals the value of unemployment. Again we assume that firms do not observe treatment status. Clearly one of the two groups has a higher reservation wage, but all workers may report that they have this higher reservation wage (since it is unobserved). Therefore we assume that the reservation wage paid out by a firm is the maximum of the two groups. It is defined by

$$E_{w=w_R} = \max(U_{nt}, U_{tr})$$

This is equivalent to

$$w_R = \frac{r}{1+r}\max(U_{nt}, U_{tr})$$

Finally, the value function for a filled job is adapted, since it also depends on the wage level. We replace the previous value function for a filled job (equation (6)) with the following:

$$J_{w_R} = p - w_R + \frac{1}{1+r}(\delta V + (1-\delta)J_{w_R}) \tag{22}$$

$$J_p = 0 \tag{23}$$

$$J = \overline{p_l} J_{w_R} + \overline{p_h} J_p \tag{24}$$

The probabilities in the last equation are the average probabilities in the population (so $\overline{p_l} = (1-\tau)p_l(a) + \tau p_l(a'))$.