# 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. In this paper, 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 before and after the experiment. Our results suggest that negative treatment externalities exist. We then construct a theoretical search model and estimate it by indirect inference for the observed treatment intensity. The estimated model shows that aggregate welfare decreases if all workers would receive a treatment.

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# 1 Introduction

In this paper we estimate the labor market effects of a Danish activation program for unemployed workers taking into account equilibrium effects. The program starts quickly after entering unemployment, and the goal is to provide intensive guidance towards finding work.[1] To empirically evaluate the effectiveness of the activation program, a randomized experiment was setup in two Danish counties. Graversen and Van Ours (2008), Rosholm (2008) and Vikström et al. (2011) show that participants in the program found work significantly faster than nonparticipants, and the difference is substantial. To investigate the presence of congestion and general equilibrium effects, we compare job finding rates of nonparticipants 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 workers.

We also focus on how the experiment affects vacancy supply. Our estimation results show that during 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 allows for both negative congestion effects (it takes more time for non-treated workers in the treatment region to find work) and positive vacancy-supply effects. We use the results from the empirical analyses to estimate the parameters of the equilibrium search model using indirect inference. Using the estimated equilibrium search 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 unemployment rate decreases in case of a large scale role out. A cost-benefit analysis indicates that government expenditures are minimized if about 30 percent of the workers participate in the activation program, while welfare is maximized if around 20 percent of the workers participate in the program. If the treatment intensity is increased beyond that, the social marginal benefits become less than the marginal costs.

A growing literature stresses 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,

[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.

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 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 equilibrium effects are assumed to be absent (e.g. DiNardo and Lee (2011)).

In case of active labor market programs, equilibrium effects are likely to be important (e.g. Abbring and Heckman (2007)). Moreover, the goal of an empirical evaluation is to collect information that helps deciding whether or not a program should be implemented on a large scale. Therefore, taking account of equilibrium effects is important. If there are equilibrium effects, changing the treatment intensity affects the labor market outcomes of both participants and nonparticipants and this has consequences for the evaluation of the program. The results from the empirical evaluation in which outcomes of participants and nonparticipants are compared are then only relevant at the observed treatment intensity. Cahuc and Le Barbanchon (2010) show within a theoretical equilibrium search model that neglecting equilibrium effects can lead to wrong conclusions regarding the effectiveness of the program. Albrecht et al. (2009), Blundell et al. (2004) and Ferracci et al. (2010) show empirically that spillover effects of various labor market policies can be quite sizable and Lise et al. (2004) find 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 estimate this model in section 6 and use it for policy simulations. Section 7 concludes.

# 2 Background

## 2.1 The Danish experiment

In this subsection, we provide some details about the activation program for unemployed workers considered in this paper. We also discuss the randomized experiment used to evaluate the effectiveness of the program 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 program 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 receive a letter explaining the content of the program. The program consists of three parts. First, after five to six weeks of unemployment, workers have to participate in a two-week job search assistance program. Next, the unemployed worker meet a caseworker either weekly or biweekly. During these meetings a job search plan is developed, search effort is monitored and vacancies are provided. Finally, if after four months the worker still has not find work, a new program starts for at least three months. At this stage the caseworker has some discretion in choosing the appropriate program, which can 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 are 2122 DKK per entitled worker.

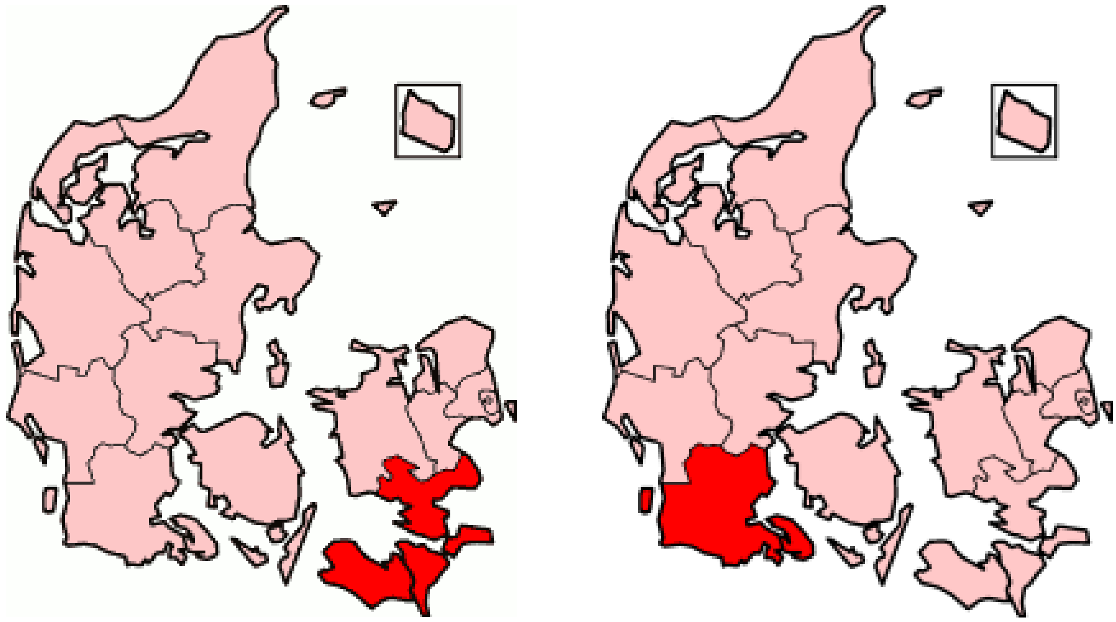
To evaluate the effectiveness of the activation policy, a randomized experiment was conducted in two Danish counties, Storstrøm and South Jutland. These counties are shown in Figure 1. Both regions are characterized by a small public sector relative to other Danish counties. The key economic sectors 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 $15^{\text{th}}$ of the month participated in the activation program, while individuals born on the $16^{\text{th}}$ to the $31^{\text{st}}$ 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 median 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 effects of the activation program separately for both counties. Graversen and

Figure 1: Location of the experiment counties.

(a) Storstrøm. (b) SouthJutland.



Van Ours (2008), Rosholm (2008) and Vikström 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 Vikström et al. (2011), who construct nonparametric bounds. 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 is largest for individuals with the longest 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, implying that if the labor market is in steady state that after four months, about 25 percent of the stock of unemployed workers is treated. If we take into account that the outflow of long-term unemployed workers is considerably lower than the outflow of short-term unemployed workers (which implies that competition for jobs occurs mostly between short-term unemployed workers), the treatment intensity is about 30 percent of the stock of unemployed workers.

## 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 address some empirical studies 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 $i$ receives treatment and zero otherwise. A standard assumption in the treatment evaluation literature is that each individual's behavior and 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), Rubin (1978)),

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

If 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 in the same area, then only the fraction of the population in the same area receiving treatment might be relevant. 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

$$\Delta = \frac{1}{N}\sum_{i}^{N} E[Y_{1i}^{*}|\bar{D}_N = 1] - E[Y_{0i}^{*}|\bar{D}_N = 0] \tag{2}$$

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 are 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 participants search more intensively, this can reduce the job finding rates of nonparticipants 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 nonparticipants but also to other participants in the program. 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 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 $E[Y_{0i}^{*}|\bar{D}_N = \bar{d}_N] - E[Y_{0i}^{*}|\bar{D}_N = 0]$, i.e. the treatment effect on the non-treated workers. 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 $\Delta$ defined in equation (2).

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 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 workers 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 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. Heckman et al. (1998) find that the effects of the size of the tuition fee on college enrollment are substantially smaller if general equilibrium effects are taken into account. Miguel and Kremer (2004) find 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.

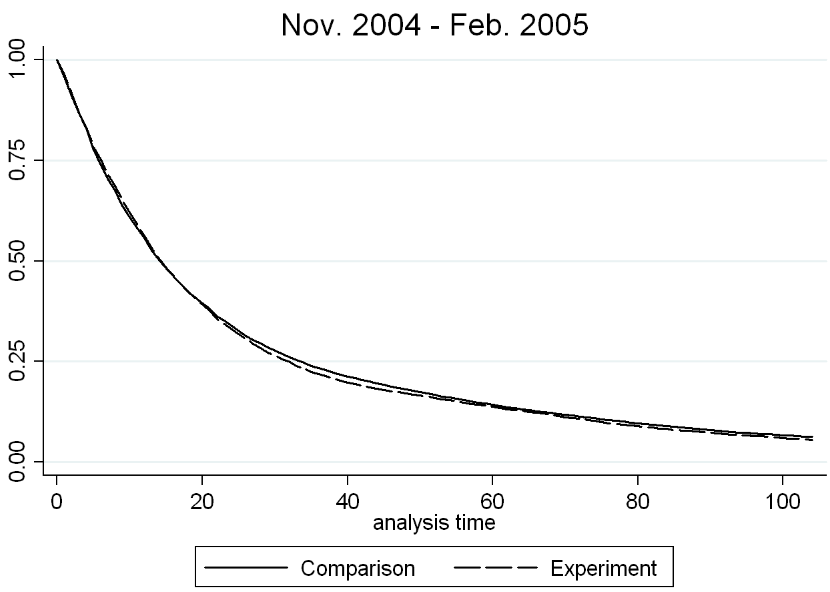
The randomized experiment discussed in subsection 2.1 involved all individuals becoming unemployed between November 2005 and February 2006 in Storstrøm and South Jutland. Our data are from the National Labor Market Board and include all 36,652 individuals who applied for benefits in the experiment period 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 refer to this as the pre-experiment sample.

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 percent of the data were removed.

For each worker we observe the week of starting collecting benefits and the duration of collecting benefits measured in weeks. Workers 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-Meier 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. Because Storstrøm and South Jutland volunteered to run the experiment, it is 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 have left unemployment, and this

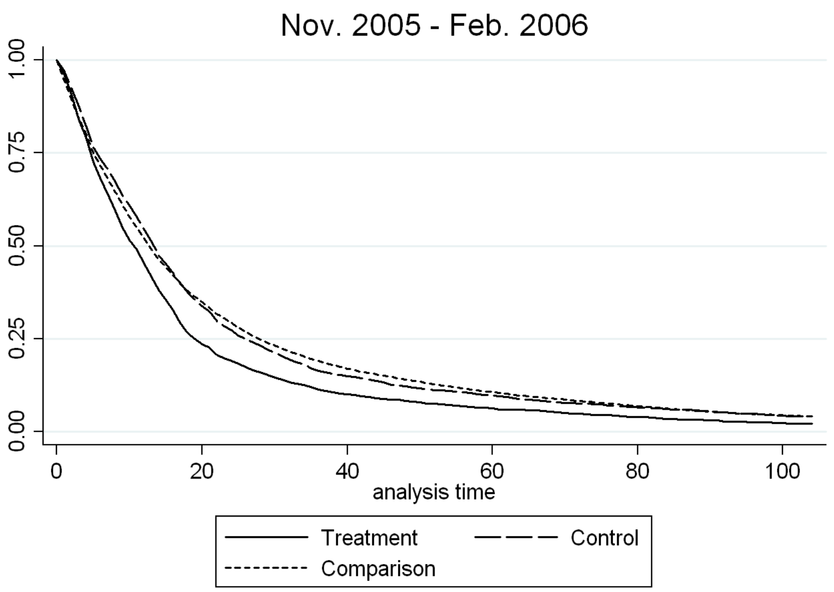
Figure 2: Survivor functions for the experimental counties and the comparison counties in the year before the experiment.




was 83.4 percent in the comparison regions. This shows that in the period prior to the experiment the survivor functions were very similar. To test this more formally, we have performed a logrank test. This test cannot reject the null hypothesis that the distributions of unemployment durations in the experiment region and in the comparison region are the same, 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 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 leave 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, over time the unemployment duration distribution changed. In the comparison regions this

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




distribution was substantially different between the pre-experiment period and the experiment period ($p$-value for similarity equals 0.01).

The data 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 have, on average, slightly more weeks of previous benefits receipt than in the comparison regions. The gender composition and nationality distribution are 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, on average, 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.

Our second data set describes monthly information on the average number of

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 |

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 shows how in both 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.

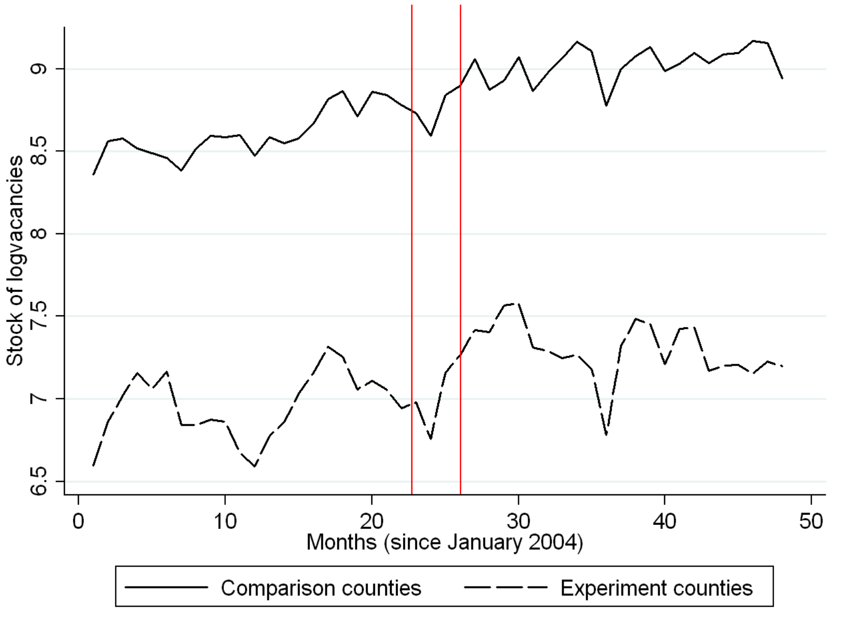
# 4 Estimations

The previous section discussed 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 equilibrium effects.

## 4.1 Unemployment durations

The aim of the activation program is to stimulate participants to find work faster. In previous studies of the randomized experiment, participants were compared to nonparticipants (see Graversen and Van Ours (2008), Rosholm (2008) and Vikström et al. (2011)). In the presence of spillovers, a simple comparison of outcomes of

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




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. 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 $x$, the county $r$ in which the individual lives, the calendar time moment $\tau$ of becoming unemployed (experiment or pre-experiment period), and whether or not the individual was assigned to the treatment group $d$ or control group $c$ of the experiment. In our baseline specification, the exit rate from unemployment for individual $i$ 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). This also captures business cycle effects. The parameters $\alpha_{r_i}$ are county fixed effects and $\beta$ 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 $\delta$ and $\gamma$, which describe the effect of the activation program on participants and nonparticipants, respectively. The parameter $\gamma$ 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 participants and nonparticipants 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, therefore, 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 effective in stimulating participants in leaving unemployment, but there is some evidence that the program is associated with negative externalities to

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.

the nonparticipants. A simple comparison of the participants and nonparticipants overestimates 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 particular, in Storstrøm the experiment has been implemented more strictly than in Southern Jutland.

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. Alternatively, we can include 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 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. The estimation results are very robust to the choice of comparison counties (see Appendix A). Finally, if there would be substantial worker mobility between counties, our estimate of the spillover effect would be an underestimate of the true spillover effect at the given treatment intensity. However, the Danish research council (2002) reports that within a year only one percent of the Danish unemployed and 1.4 percent of the employed workers move location.

#### 4.1.2 Binary outcomes

Above, we used a duration model to estimate the effects 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 imposed 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

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.

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. To estimate the effect of the activation program on the participants and the nonparticipants, we use 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 $\alpha_{r_i}$ are fixed effects for the different counties and $\eta_{\tau_i}$ describe the common time trend. The framework is a difference-in-difference model and the parameters of interest are $\delta$ and $\gamma$, 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 above.

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 participants becomes smaller for longer unemployment durations, but is always highly significant. The decrease in the size is not surprising. After longer periods the fraction survivors is reduced substantially and the parameter estimates describe absolute changes in survival probabilities. However, also Graversen and Van Ours (2008), Rosholm (2008) and Vikström 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 largest. Early in the unemployment spell also relatively many participants in the activation program exit unemployment, which reduces treatment externalities for the nonparticipants later in the unemployment spell. 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 small. After two years, only slightly more than 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 rate 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 of vacancies on time dummies $\alpha_t$, an indicator for the experiment $D_{rt}$, and we allow for county fixed effects $\theta_r$,

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

Because the dummy variable $D_{rt}$ only takes value one during the experiment, this is a difference-in-differences model. The parameter of interest is $\delta$, which describes the fraction by which the stock of vacancies changed during the experiment. The key identifying assumption is that the experiment regions and the comparison regions have a common trend, described by $\alpha_t$, 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, $\delta$ 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 (see Bertrand et al. (2004) for an extensive discussion).[2]

Table 4 reports the estimation results. Column (1) shows that during the four months of the experiment (November 2005 until February 2006), the stock of vacancies increased by about five percent 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 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 adjusts. In the beginning of 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. Finally, it takes some time to fill a vacancy. 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 increase 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 equilibrium effects. Nonparticipants in the experiment have

[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 South Jutland | | | 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 parentheses, * indicates significant at 10% level, ** at the 5% level and *** at the 1% level.

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 of outcomes between participants and nonparticipants does not estimate the most 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 estimate the model by indirect inference where we use the estimates in the previous section as our auxiliary model given a treatment rate of 30 percent. We then use the estimated model to study the effects of the activation program for higher treatment rates including the case where the program is implemented in Denmark as a whole.

## 5.1 The labor market

Point of departure is a discrete-time DMP matching model. We extend the model with an endogenous matching function that depends on labor market tightness, the individual number of applications and the average number of applications (see Albrecht et al. (2006) for a related matching function). Workers are risk neutral and all have the same productivity. They only differ in whether or not they participate in the activation program. Participation in the program reduces the costs of making a job application but costs time. 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. Finally, we impose symmetry (identical workers play identical strategies) and anonymity (firms treat identical workers equally).

When a worker becomes unemployed, she receives benefits $b$ and a value of non-market time, $h$. She must also decide how many applications to send out. The choice variable $a$ describes the number of applications, which workers make simultaneously within a time period. A worker becomes employed in the next period if one of the job applications was successful, otherwise she remains unemployed and must 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. $\gamma_0 a^2$.

An important feature of our model is that we allow the success of an application to depend on the search behavior of other unemployed workers and the number of posted vacancies. Let $\bar{a}$ describe the average number of applications made by other unemployed workers, $u$ be 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 $\bar{a}$.

Let $r$ be the discount rate and $E(w)$ be the flow value of being employed at a job that pays $w$. We assume that benefits and search costs are realized at the end of the period to simplify notation (if one prefers benefits and search costs to be realized at the beginning of a period they should be multiplied by $(1+r)$). For an unemployed worker who does not participate in the activation program, the value of unemployment is summarized by the Bellman equation,

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

which can be rewritten as,

$$rU_0 = \max_{a \geq 0} b + h - \gamma_0 a^2 + m(a;\bar{a},\theta) \left[ E(w) - U_0 \right] \tag{3}$$

The optimal number of applications that a worker, who does not participates in the activation program, sends out $(a_0^*)$ follows from the first-order condition

$$a_0^* = \frac{E(w) - U_0}{2\gamma_0} \frac{\partial m(a;\bar{a},\theta)}{\partial a} |_{a=a_0^*} \tag{4}$$

The activation program consists of meetings with caseworkers and a job search assistance program which are time-consuming for participants. We assume that this eliminates the non-market time $h$ that nonparticipating unemployed workers have. The benefit of the program is that it reduces the costs of making job applications to $\gamma_1 < \gamma_0$. Again, 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

$$a_1^* = \frac{E(w) - U_1}{2\gamma_1} \frac{\partial m(a;\bar{a},\theta)}{\partial a} |_{a=a_1^*} \tag{5}$$

Furthermore, let $\tau$ 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. With probability $\delta$ 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 employees 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,

$$rE(w) = w - \delta\left[(E(w) - \bar{U})\right] \tag{6}$$

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 unemployed workers and on labor market tightness $\theta$. 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. The Bellman equation for the value $V$ of a vacancy is

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

where $J$ is the value of filled vacancy. Each period that a job exists, the firm receives the value of output $p$ minus wage cost $w$. With probability $\delta$ the job is destroyed and the job switches from filled to vacant. The value of filled vacancy $J$ is, therefore, given by,

$$rJ = p - w - \delta(J - V) \tag{8}$$

## 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 anyway. Let $\beta$ 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

$$w^* = \frac{\beta p\Big[(r+\delta)(r+m_0+m_1) + m_0 m_1 - \delta\overline{m}\Big] + (1-\beta)\Big[(1-\tau)m_1\pi_0 + \tau m_0 \pi_1 + r\overline{\pi}\Big]}{(r+\delta)(r+m_0+m_1-\overline{m}) + \beta(r\overline{m} + m_0 m_1)} \tag{9}$$

where $m_0 = m(a_0^*; \bar{a}, \theta)$, and $m_1 = m(a_1^*; \bar{a}, \theta)$. The function $\overline{m}$ describes the population average $\tau m_1 + (1-\tau)m_0$, and similarly $\overline{\pi} = \tau\pi_1 + (1-\tau)\pi_0$. The wage level increases in the productivity of a match ($p$) and in the (average) net flow income of unemployment ($\pi_0$ and $\pi_1$), which increases the outside option of the worker.

In Appendix B we solve the model for the wage mechanism of Albrecht et al. (2006) where workers with multiple offers have their wages bid up by Bertrand competition. This gives very similar results in terms of labor market flows, vacancy creation and the effects of the activation program. This outcomes are discussed in more detail in subsection 6.5.

Finally, we have to specify the matching functions $m(a; \bar{a}, \theta)$ for unemployed workers and $\frac{m(\bar{a},\theta)}{\theta}$ for vacancies. Since participation in the activation program reduces search costs, the matching function should allow for different search intensities of participants and nonparticipants. Moreover, it should allow for congestion effects between unemployed job seekers. Below we adjust the matching function of Albrecht et al. (2006) to incorporate this.[3] There are two coordination frictions affecting job finding: (*i*) workers do not know where other workers apply, and (*ii*) firms do not know which candidates are considered by other firms. This last coordination friction is absent in a usual Cobb Douglas matching function. If a firm receives multiple applications, it randomly selects one applicant who receives a job offer. The other applications are turned down as rejections. 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 accepts it.

The expected number of applications per vacancy is given by

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

[3]As a sensitivity analysis we also tried a Cobb-Douglas matching function. But we did not manage to get the parameters of the matching function such that it could explain both a negative effect of the activation program on the nonparticipants in the program and a higher stock of vacancies. We take this as evidence that our matching function is preferred over a Cobb-Douglas matching function.

If the number of unemployed workers and the number of vacancies are sufficiently large, then the number of applications that arrive at a specific vacancy is a Poisson random variable 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 applications $a_1^*$ and $a_0^*$, we obtain the matching rates for the participants and the nonparticipants in the activation program, respectively.

The aggregate matching function is simply $u\bar{m}$ and it is first increasing in the number of applications per worker and then decreasing. More applications per worker reduce the first coordination problem mentioned above but amplify the second one.

## 5.3 Equilibrium and welfare

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,

$$u^* = \frac{\delta}{\delta + \tau m(a_1^*; \bar{a}, \theta) + (1-\tau) m(a_0^*; \bar{a}, \theta)} \tag{10}$$

The zero-profit condition for opening vacancies $V = 0$ 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

$$\frac{m(\bar{a}, \theta^*)}{\theta^*} = \frac{(r+\delta)c_v}{p - w^*} \tag{11}$$

The left-hand size is decreasing in $\theta$ and goes to infinity when $\theta$ approaches zero. Because wages are increasing in $\theta$, the right-hand size is increasing in $\theta$. Therefore, there is a unique $\theta^*$ that satisfies the equilibrium condition in equation (11). We can now define the equilibrium as the tuple $\{a_0^*, a_1^*, w, u^*, \theta^*\}$ that satisfies equations (4), (5), (9), (10) and (11).

Now we have solved the model and have derived conditions for equilibrium, we can use the model for policy simulations. The decision parameter for the policy maker is the intensity $\tau$ of the activation program. Let $c_p$ describe the costs of assigning an unemployed worker to the activation program. This is a lump-sum amount paid at the start of participation in the activation program. A welfare analysis should take account of the productivity of the workforce $(1-u)p$, the costs of open vacancies $vc_v$, the time costs of unemployed workers $(h - \gamma_0 a_0^{*2})$ and $-\gamma_1 a_1^{*2}$ for nonparticipants and participants respectively and the costs of the program $c_p$. Welfare is given by

$$W(\tau) = (1-u)p + u\left((1-\tau)\frac{h-\gamma_0 a_0^{*2}}{1+r} + \tau\frac{-\gamma_1 a_1^{*2}}{1+r}\right) - \delta(1-u)\tau c_p - vc_v \qquad (12)$$

Note that the welfare function does not include unemployment insurance benefits because those must be paid for and are thus a matter of redistribution. After having estimated the model parameters, we can investigate if the experiment increased welfare, i.e. if $W(0.3) > W(0)$ and if a large-scale role out of the activation program would increase welfare $W(1) > W(0)$. Furthermore, we can compute the welfare-maximizing value for $\tau$.

Alternatively, a naive policymaker may be interested in the effect of the program on the government budget. Since $\delta(1-u)$ describes the inflow into unemployment, total program costs are $\delta(1-u)\tau c_p$. The naive policymaker confronts the costs of the program with the total reduction in benefit payments. The total amount of benefit payment equals $ub$. This implies that the naive policymaker chooses $\tau$ such that it minimizes the costs of the unemployment insurance program,

$$C_{UI}(\tau) = ub + \delta(1-u)\tau c_p \qquad (13)$$

Finally, it is interesting to compare the results of these policy parameters to results from a typical microeconometric evaluation. In a microeconometric evaluation, the costs of a program are typically compared to reductions in benefit payments. The reduction in benefit payments is usually estimated from comparing expected benefit durations of participants and nonparticipants (e.g. Eberwein et al. (2002) and Van den Berg and Van der Klaauw (2006)),

$$ME_{\tau=0.3} = \left(b\left(\frac{1}{m(a_1^*;\bar{a},\theta)} - \frac{1}{m(a_0^*;\bar{a},\theta)}\right) - c_p\right) \qquad (14)$$

where $\frac{1}{m(a_1^*;\bar{a},\theta)} - \frac{1}{m(a_0^*;\bar{a},\theta)}$ is the difference in expected unemployment duration between unemployed workers participating and not participating in the activation program. A positive value implies positive returns to the program. This evaluation ignores equilibrium effects and foregone leisure of the participants.

# 6 Estimation and evaluation

In this section we first describe the estimation of the equilibrium search model by indirect inference using the treatment effects estimated in section 4 as our auxiliary model (see Smith (1993) and Gourieroux et al. (1993)). Next, we use the estimated model to study the welfare effects of the program and the effects of a large-scale implementation. Finally, we provide some sensitivity analyses.

## 6.1 Parameter values

By the nature of our 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 $\tau$ of the activation program to 0.3 (see the discussion in subsection 2.1. In subsection 6.5 we estimate the model for alternative levels of treatment intensities during the experiment. We set the discount rate equal to ten percent annually, which implies that $r$ is 0.008. This is a smaller than the discount rates used by, for example, Lise et al. (2004), Fougère et al. (2009) 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. The parameters are determined such that a set of data moments is matched as closely as possible by the corresponding model predictions. The moments that we consider are presented in Table 6. The model should capture the unemployment and vacancy rates from the data, the estimated program effect on the participants and on the nonparticipants, the estimated increase in vacancies due to the experiment, the average matching rate in the experiment counties and finally the fact that unemployment benefits are approximately 65 percent of the wage level. Define $\xi = (\gamma_0, \gamma_1, h, \delta, c_v, b, \beta)$ as the vector of parameters to be estimated. For given values for $\xi$ the model can be solved and the set of model predictions can be computed. To obtain estimates for $\xi$, we minimize the sum of squared differences between the data moments and the corresponding model predictions over $\xi$, where each squared difference is given an appropriate weight based on the variance of the (estimated) data moment.

The estimates for $\xi$ are presented in the lower panel of Table 5 (standard errors

Table 5: Parameter values.

| | | |
|---|---|---|
| *Fixed parameter values* | | |
| $\tau$ | 0.3 | 30% of the unemployed workers are treated |
| $r$ | 0.008 | annual discount rate equals 10%. |
| $p$ | 1 | productivity normalized to 1 |
| *Estimated parameter values* | | |
| $\gamma_0$ | 0.202 (0.012) | cost of sending an application for nonparticipants |
| $\gamma_1$ | 0.114 (0.020) | cost of sending an application for program participants |
| $h$ | 0.013 (0.028) | value non-market time for nonparticipants |
| $b$ | 0.640 (0.008) | UI benefits |
| $\delta$ | 0.011 (0.000) | job destruction rate |
| $c_v$ | 0.820 (0.147) | per period cost of posting a vacancy |
| $\beta$ | 0.751 (0.029) | bargaining power |

Note: Standard errors in parentheses.

are computed using the delta method). In line with the goal of the activation program, we find that the costs of making job applications are lower for participants than for nonparticipants. The leisure costs of participating in the activation program are over one percent of productivity or almost two percent of the unemployment benefits level. The job destruction rate is slightly over one percent per month, unemployment benefits are 64 percent of productivity, and the bargaining power of workers is 0.75.

## 6.2 Increasing the intensity of the activation program

We now use the model to predict how the program effects depend on the fraction of the unemployed population participating in the activation program. We are interested in the effects on the matching rates of both participants and nonparticipants, as well as the effects on aggregate unemployment and vacancy rates, wages and welfare.

We simulate the model for a gradually increasing fraction of program participants $\tau$ in the unemployed population. The results are shown in Figure 5. The graph on the top-left shows that the unemployment rate decreases in $\tau$ until about 70 percent of the unemployed workers participate in the program. In this part the unemployment rate decreases because due to the increased search effort it is less likely that vacancies receive no applications. Once the program intensity exceeds 70

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) | $u^*\|\tau = 0.3$ |
| Program effect on log vacancies | 0.081 | Estimated percentage effect on vacancies 5-6 months after the beginning of the experiment (see Table 4) | $\frac{(v^*\|\tau=0.3)-(v^*\|\tau=0)}{(v^*\|\tau=0)}$ |
| Program effect on participants | 0.070 | Estimated effect (see Table 3) | $[1-(1-(m_1\|\tau=0.3))^3]-[1-(1-(m_0\|\tau=0))^3]$ |
| Program effect on nonparticipants | $-0.027$ | Estimated effect (see Table 3) | $[1-(1-(m_0\|\tau=0.3))^3]-[1-(1-(m_0\|\tau=0))^3]$ |
| Outflow rate after three months | 0.51 | Fraction of unemployed in Storstrøm and South Jutland that leaves 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 labor force in Storstrøm and South Jutland | $v^*\|\tau = 0.3$ |
| Replacement rate | 0.65 | Unemployment benefits are 65% of the wage level | $\frac{b}{w^*}\|\tau = 0.3$ |

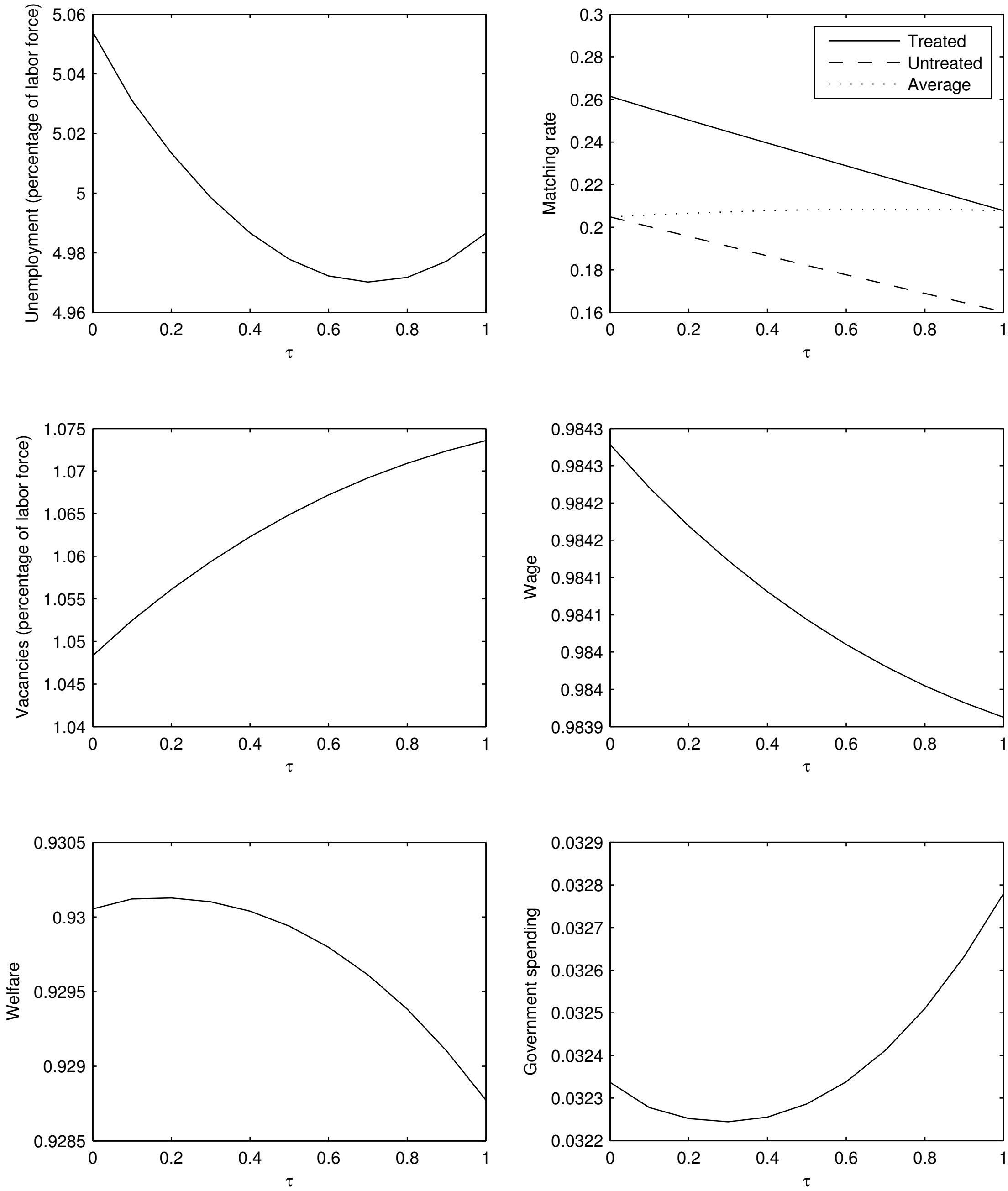



Figure 5: Simulation results baseline model.

percent the unemployment rate increases again, which is the result of dominating congestion effects (multiple firms make a job offer to the same unemployed worker). Compared not assigning any unemployed worker to the activation program, the unemployment rate decreases by slightly over 0.08 percentage point when the program intensity is 70 percent and almost 0.07 percentage point in case all unemployed workers participate. The latter corresponds to about 1.4 percent reduction in the number of unemployed workers.

The graph on the top-right shows the matching rates for program participants and nonparticipants. Because participants in the activation program make more applications than nonparticipants, they always have a higher matching rate. The difference in matching rates remains similar for different values of $\tau$ and shows that participants are slightly over 5 percentage point more likely to find a job within a given month.

The matching rates of both the treated and the untreated decrease monotonically, while treated individuals have a 28% higher matching rate than untreated for $\tau = 0.3$. The average matching rate is maximized for $\tau = 0.1$ and decreases slighly for higher $\tau$. Note that both the small decrease in the matching rate for the untreated over $\tau = [0, 0.3]$ and the difference between the matching rates of the treated and untreated are very close to the empirical estimates from section 4. In table 7 we compare the estimated 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 $\tau$ the model predicts that matching rates for both groups continue to decrease, as externalities such as congestion in the matching process become larger.

The vacancy rate increases, as we established empirically before, but the size of the increase is small. The increase in the supply of vacancies is not only due to the higher search intensity but is also caused by the fact that the "stick effect" of the program slightly decreases (reservation) wages and this increases labor demand.

## 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 $\tau$ is increased from 0 to 1. As can be seen in the graph, the average matching rate is almost constant. To be exact, $(\overline{M}|\tau = 1) - (\overline{M}|\tau = 0) = 0.003$, such that the effect of full treatment is

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) | 0.205 | 0.191 | 0.182 | 0.160 |
| $m_t$ (Data) | - | 0.238 | - | - |
| $m_t$ (Simulated) | 0.261 | 0.245 | 0.234 | 0.208 |

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

practically zero. 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. The difference is 0.054, which is a 28% increase in the matching rate. This difference is rather stable as $\tau$ changes. The main conclusion we draw from this analysis is that a comparison of the control and the treatment group strongly overestimates the policy-relevant treatment effect. Taking general equilibrium effects into account reduces the treatment effect on the job finding rate to almost zero.

## 6.4 Cost-benefit analysis and welfare

The model predictions allow for different types of cost-benefit analyses, as described in section 5.3. First of all, welfare, as defined in equation (12), can be computed for each value of $\tau$. The result is also presented in figure 5. Despite the fact that unemployment decreases in $\tau$ for $\tau < 0.8$, welfare only increases slightly to maximum at $\tau = 0.2$ and decreases for higher treatment intensities due to higher vacancy costs and costs of the program. So welfare is maximized at $\tau = 0.2$.

Second, a cost benefit analysis for the policy maker can be performed. Total expenditure is the sum of unemployment benefits and spending on active labor market programs: GS= $ub + \delta(1-u)\tau c_p$. Also this expression is presented in figure 5. Government spending is lowest for $\tau = 0.3$ and increases afterwards.

A naive cost-benefit analysis would ignore potential equilibrium effects and simply compare the reduction in unemployment duration (and corresponding unemployment benefits) with the costs of the program (as defined in equation (14)). The cost of the program ($c_p$) is 2122 DKK (about 285 euro, 355 USD), 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. A naive evaluation thus erroneously suggests that full treatment is optimal.

To sum up, we find that the matching rates of both the treated and the untreated decrease strongly as the share of treated increases, while also the average matching rate decreases slightly. As a result the policy relevant treatment effect is only marginally positive. Unemployment is minimized for $\tau = 0.7$. Comparing the control group with the treatment group overestimates the treatment effect. Vacancies show a small increase. Cost-benefit analyses suggest that $\tau = 0.2$ maximizes welfare while a standard micro-econometric evaluation, which ignores equilibrium effects, concludes that the gains of the program would always exceed the costs. In the next section we perform a number of robustness checks.

## 6.5 Robustness checks

### 6.5.1 Different values of $\tau$

*However, if we take into account that during the experiment, unemployment was decreasing in Denmark (i.e. the outflow was larger than the inflow), the fraction of treated would be closer to 20 percent. The percentage of treated workers will influence our estimates of how important general equilibrium effects are. In section 4 we will show that the exit rate of workers in the control group relative to workers in regions where there was no treatment, decreased during the experiment. The higher the fraction of treated workers we use in our structural estimation, the larger the estimated negative congestion effects will be. We choose to be conservative in our main analysis and therefore we set the treatment percentage at 30 percent. As we will see, this already gives negative welfare effects if the program would be introduced on a larger scale (despite a positive treatment effect on the treated). However, in section 6.5 we also estimate our model for lower treatment intensities (20 and 25 percent) which make the welfare effects of an economy-wide implementation even worse.*

As discussed before, we set the share of unemployed participating in the program at 30 % when estimating the model. This number was based on a steady state assumption. However, during the experiment, unemployment outflow exceeded the inflow and therefore it is likely that the actual share was lower. Let $\tau^e$ be the fraction of the stock of unemployment that received a treatment in the experiment. As a robustness check we estimated the model also with values of $\tau^e$ being equal to 0.25 and 0.2 and present the simulation results in figure 6 together with the baseline results ($\tau^e = 0.3$). Given our estimated negative treatment effect on the control group this reduction in $\tau^e$ will increase the estimated negative congestion effects. We only show the results for unemployment and welfare. Unemployment is monotonically increasing for $\tau^e = 0.2, 0.25$, where the increase is more than 0.5%

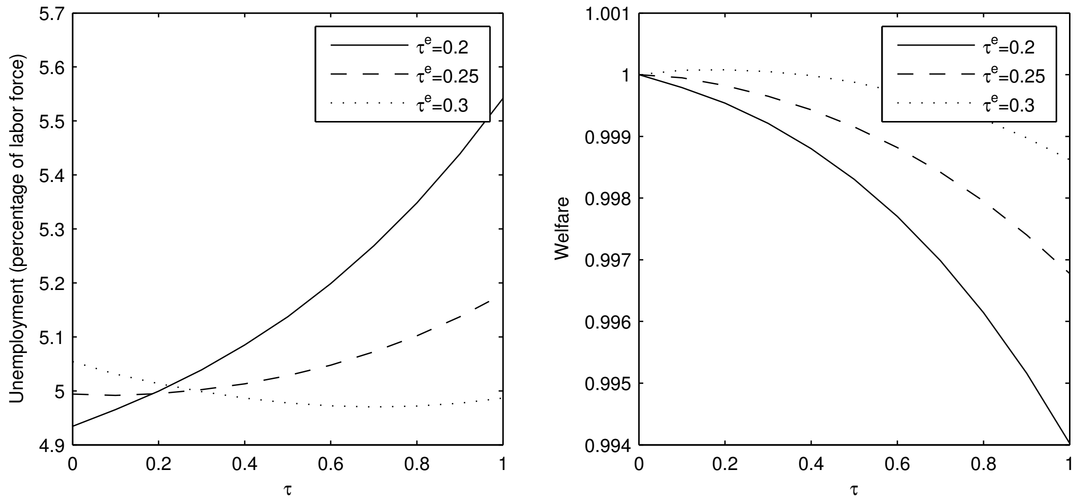



Figure 6: Simulation results from estimations using different values of $\tau^e$

points in the case of $\tau^e = 0.2$. Welfare is normalized to 1 in case of no treatment, in order to make the different estimates comparable. In all three cases, welfare decreases as the treatment intensity increases, but the decrease is much stronger if the model is estimated under the assumption of $\tau^e = 0.2$. The congestion effects for the low values of $\tau$ are so large that unemployment would increases if the program would be implemented in the entire country (the negative effect that more firms consider the same workers dominates the positive effect that it is less likely that a vacancy receives no applications). Finally, the negative welfare effects are not an artifact of the structure of the model. If we would use (unrealistically large) values of $\tau^e(\geq 0.4)$ for the program, the model would predict small congestion effects and positive welfare effects. This shows that the model is sufficiently flexible to capture both positive and negative welfare effects. However, for all feasible values of $\tau^e$ we find negative effects on welfare.

### 6.5.2 Bertrand competition for workers

In this section we modify the model by replacing Nash bargaining by ex post Bertrand competition (similar to for example Albrecht et al. (2006)), to see whether the results are sensitive to the wage mechanism.

Bertrand competition implies that workers with one offer receive the reservation wage while workers with multiple offers receive the full match surplus. A theoretical advantage is that the bargaining power is now endogenous (workers with multiple offers get the full surplus and else firms get the full surplus). This implies that there are only six parameters to estimate which makes the estimates more precize A complete derivation of the model under Bertrand competition is provided in the appendix. Here we simply summarize the results.

Using the modified model, we again estimate the parameters using the same approach. The results are presented in Table 8.

Table 8: Parameter values for the model with Betrand competition

| | | |
|---|---|---|
| *Fixed parameter values* | | |
| $\tau$ | 0.3 | 30% of the unemployed workers are treated. |
| $r$ | 0.008 | Annual discount rate equals 10%. |
| $p$ | 1 | Productivity normalized to 1. |
| *Estimated parameter values* | | |
| $\gamma_0$ | 0.036 (0.023) | (cost of sending an application for untreated workers) |
| $\gamma_1$ | 0.139 (0.019) | (cost of sending an application for treated workers) |
| $h$ | 0.257 (0.025) | (value non-market time for untreated unemployed) |
| $b$ | 0.628 (0.044) | (UI benefits) |
| $\delta$ | 0.011 (0.001) | (job destruction rate) |
| $c_v$ | 2.897 (1.725) | (per period cost of posting a vacancy) |

We then simulate the model for different values of $\tau$, the share of treated unemployed workers. The effect of $\tau$ on unemployment, vacancies, the matching rates, the reservation wage, government spending and welfare is shown in Figure 7. This model also matches the empirical findings well. Despite the fact that assuming a different wage mechanism changes the estimates of some of the primitives, our main results results are quite similar to the outcomes under Nash bargaining. Treated individuals have a higher matching rate and both matching rates decrease as $\tau$ increases. The small increase in vacancies is also matched by the model. Full treatment would lead to an increase in the average matching rate and a decrease in unemployment of almost 0.6% point. As expected, the average reservation wage deceases monotonically.

The decrease in unemployment leads to an optimal treatment rate for the government budget of 100%. However, when we take into account all costs and consider welfare, we find that it is monotonically decreasing in the treatment intensity.

# 7 Conclusion

In this paper we investigate the existence and magnitude of externalities of job search assistance. Using data from a randomized experiment we find evidence that,

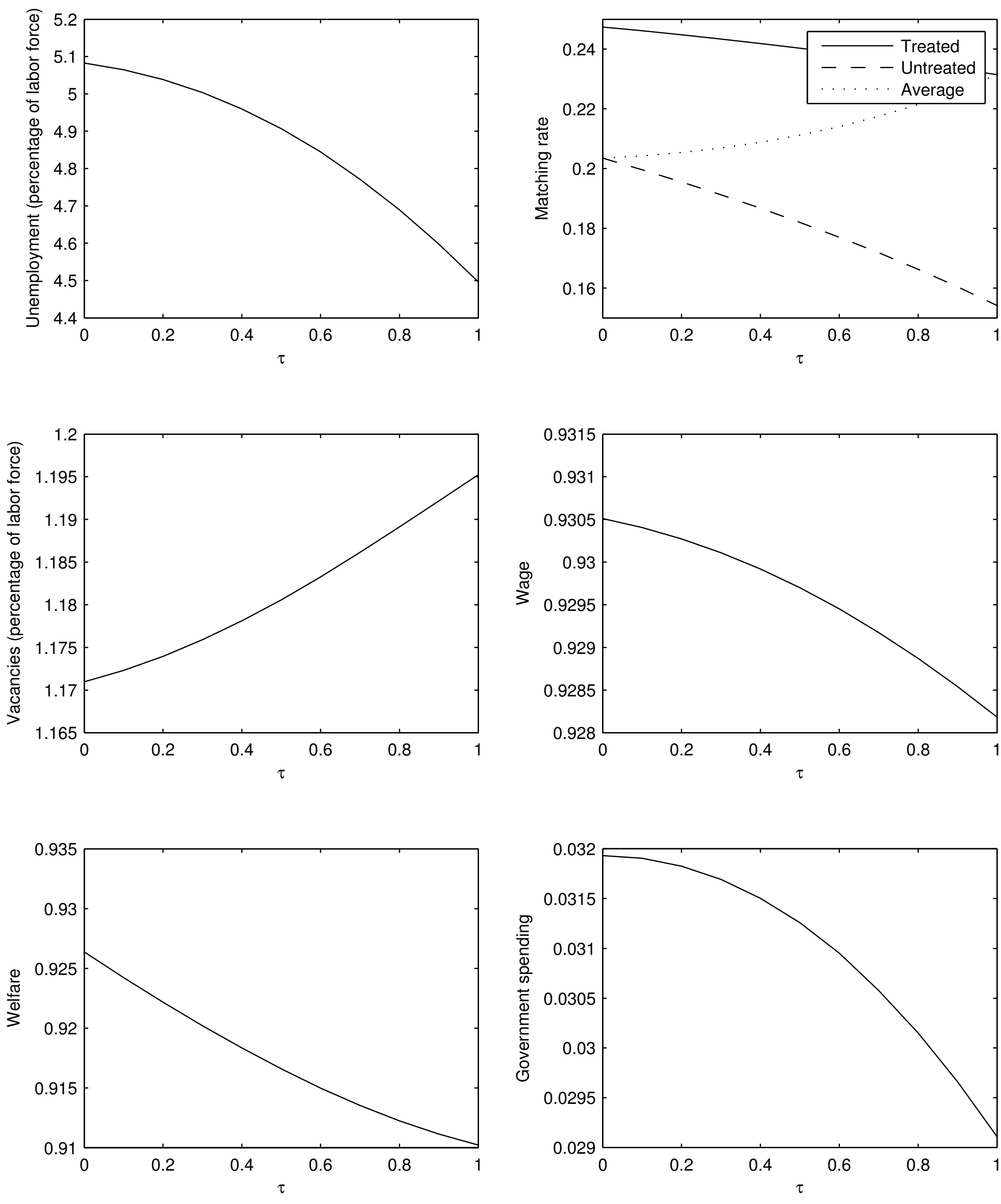



Figure 7: Simulation results due to changes in $\tau$ with Bertrand wages

the matching rate of the control group decreased due to the experiment. This implies that simply comparing unemployment durations of the treatment and control group overestimates the true effect. In order to get an idea of the policy relevant treatment effect, we estimate a search model at the observed treatment intensity and use this model to simulate what happens if we increase the treatment intensity. The model can mimic both the positive treatment effect of the treated, the negative treatment effect of the non treated and the increase in the vacancy supply in the treatment regions. When the share of treated is increased to 100 percent, unemployment is predicted to increase with 0.2 percentage points while welfare decreases and government expenditure increases. Our results suggest that even programs that are highly successful at a small scale level need not be desirable at the aggregate level. We find that government expenditure is minimized and welfare is maximized for a treatment intensity of $\tau = 0$.

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# A Empirical analyses with restricted comparison counties

In section 4 we presented our empirical results, which 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 choice of comparison counties.

First, we consider a comparison counties the three counties which are closest to the experiment counties. These counties might be most similar and experience a trend very close to the experiment counties. However, if there are spillovers between counties due to, for example, workers commuting between counties, this most likely affects neighboring counties most. Therefore, as a second sensitivity analysis we consider the two counties which are furthest from the experiment counties as control counties. Finally, we consider a control counties five counties which are most similar in aggregate statistics to the experiment counties.

Table 9 shows for the duration model for the unemployment durations the estimation results for the three sensitivity analyses. Comparing the parameter estimates across the different columns and with those presented in Table 2 shows that the estimated effects are quite robust against the choice of the comparison counties.

In Table 10 we repeat the sensitivity analyses but now for the difference-in-difference model for the stock of vacancies. Although the significance levels differ between the different choice of comparison counties, all results indicate substantial equilibrium effects quantitatively similar to those presented in Table 4.

Table 9: 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 10: 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.

# B Equilibrium search model with Bertrand competition

As in Albrecht et al. (2006), we assume now that wages are determined by ex-post Bertrand competition rather than Nash bargaining. If a worker receives offers from multiple firms, wages are driven up to productivity ($w = p$) but if a worker only receives one offer, the firm receives the full surplus, (the worker receives the reservation wage) ($w = w_l$). Therefore, the wage depends on the number of offers (denoted by $j$). The probability of getting the low (reservation) wage (given a match) is:

$$\begin{aligned} p_l(a) &\equiv \Pr(j = 1 | j > 0) \\ &= \frac{\Pr(j = 1)}{\Pr(j > 0)} \end{aligned} \tag{15}$$

In a large labor market, the number of job offers follows a Poisson distribution with mean $\lambda$.

Since $a$ is not necessarily an integer, we think of it as a measure of search intensity. Think of it as the number of applications multiplied by the probability that an application is of high enough quality to be taken into account by the firm. Note that $\lambda_n = \psi a$, which is mass of relevant applications multiplied by the probability that an application results in 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 $\lambda_t = \psi a'$.

The probability of receiving exactly one offer (implying a low wage), conditional on $a$ and a match, is

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

Similarly the probability of getting the high wage (equal to productivity) is equal to the probability of receiving 2 or more offers conditional on a match:

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

The same holds for the treated workers (with $a$ replaced by $a'$ and $\lambda_n$ replaced by

$\lambda_t$). Given these probabilities equation 6 is replaced by:

$$rE_l = w_l - \delta(E_l - \bar{U}) \tag{17}$$

$$rE_h = p - \delta(E_h - \bar{U}) \tag{18}$$

$$\tag{19}$$

where $E_l$ is the value of being employed at the reservation wage and $E_h$ the value of being employed at one's marginal productivity. Since treated workers send more applications, they are more likely to receive a high wage and we must take this into account. A worker who sends out $a$ applications expected value of employment equals

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

while for a treated worker we have to replace $a$ by $a'$. Furthermore, strictly speaking, treated and non-treated workers will have different reservation wages so without further restrictions on the strategy space this requires mixing a la Albrecht Axell (1983). Of course this is only relevant in case the worker receives exactly one offer. To keep things simple, we do not allow for mixing. In order for all contacts to result in a match (i.e. to prevent that firms choose to offer the low reservation wage), the government should set the minimum wage equal to the maximum of the outside option of treated and non treated workers. The main results are not sensitive to this. We find in fact that for any linear combination of the high and low reservation wage that welfare is decreasing in $\tau$.

$$E_l = \max(U_0, U_1)$$

This is equivalent to

$$w_l = (r + \delta)\max(U_0, U_1) - \delta\bar{U}$$

The value function for a filled job (equation (8)) now becomes,

$$rJ_{w_R} = p - w_R - \delta(J_{w_R} - V) \tag{21}$$

$$rJ_p = 0 \tag{22}$$

$$J = \overline{p_l}J_{w_R} + \overline{p_h}J_p \tag{23}$$

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