ESTIMATING EQUILIBRIUM EFFECTS OF JOB SEARCH ASSISTANCE

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February 7, 2017

Abstract

Identifying policy-relevant treatment effects from randomized experiments requires no spillovers between participants and nonparticipants (SUTVA) or variation in treatment levels. We find that SUTVA is violated for a Danish activation program for unemployed workers. Using a difference-in-difference model we show that the nonparticipants in the experiment regions find jobs slower after the introduction of the activation program (relative to workers in other regions). We then estimate an equilibrium search model. This model shows that a large-scale roll out of the activation program decreases welfare, while a standard partial microeconometric cost-benefit analysis concludes the opposite.

Keywords: randomized experiment, policy-relevant treatment effects, job search, externalities, indirect inference.
JEL-code: C21, E24, J64.

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We thank Jean Marc Robin, Philipp Kircher and Xiaoming Cai for useful comments, as well as seminar and conference participants at the University of Edinburgh, Boston University, University of Essex, INSEE Crest, Royal Holloway University, UCL, VU Amsterdam, Georgetown University, University of Cambridge, University of Wisconsin-Madison and the Annual Search and Matching conference in Cyprus.

1 Introduction

We estimate the labor market effects of a Danish activation program for unemployed workers taking into account general equilibrium effects. The program starts quickly after entering unemployment, and the goal is to provide intensive guidance towards finding work. To empirically evaluate the effectiveness of the activation program, a randomized experiment was set up in two Danish counties. Graversen and Van Ours (2008), Rosholm (2008) and Vikström et al. (2013) show that participants in the program found work 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 in the experiment counties with unemployed workers in comparison counties (using the same administrative data). Since both experiment counties were not selected randomly, we use pre-experimental data from all counties to control, in a difference-in-difference setting, for existing differences between counties. This allows us to estimate treatment effects on the non-treated workers. We find that the job finding rate of nonparticipants is lower during the experiment.

We also focus on how the experiment affects vacancy supply, wages and working hours. We find some borderline statistically significant evidence that the supply of vacancies increased faster in the experiment regions, but the post-unemployment job quality is unaffected. Next, we develop an equilibrium search model that incorporates the activation program and allows for both positive or negative congestion effects and for vacancy supply responses. We use the results from the empirical analysis to estimate the parameters of the equilibrium search model using indirect inference. The estimated equilibrium search model allows us to study the effects of a large-scale roll out of the activation program and compute the effects on labor market behavior and outcomes. Our main finding is that a large-scale roll out decreases welfare. This finding is robust to different specifications in terms of wage mechanism and matching function. The model that fits the data best has a matching function that allows for strong congestion effects (if the average search intensity increases, the aggregate matching rate can even decrease) and has Nash wage bargaining. In this model, aggregate unemployment increases slightly (half a percentage point) in case of a large-scale roll out.

A large number of papers stresses the importance of dealing with selective participation when evaluating the effectiveness of employment programs for disadvantaged workers. In particular, LaLonde (1986) demonstrates the difficulty of reproducing results from randomized experiments with non-experimental methods. 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 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 facilitates a decision on whether or not a program should be implemented on a large scale. If equilibrium effects exist, changing the treatment intensity affects the labor market outcomes of both participants and nonparticipants. As a result, the findings from an empirical evaluation in which outcomes of participants and nonparticipants are compared will depend on 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 sizable and Lise et al. (2004) find that the conclusion from a cost-benefit evaluation is reversed when taking account of equilibrium effects. Crépon et al. (2013) observe different treatment intensities for a sample of long-term unemployed workers in France and find some evidence of externalities and Lalive et al. (2013) report evidence of spillover effects of an extended benefits program for Austria.

Our paper not only contributes to the empirical treatment evaluation literature, but also to the macro (search) literature. We demonstrate how data from a randomized experiment combined with non-experimental data can be used as auxiliary moments to estimate congestion effects in the matching process and how vacancy supply responds to an increase in search intensity in a macro search model. We exploit the fact that, due to the experimental design, the increase in search intensity of participants in the activation program is truly exogenous. This makes the identification of the structural parameters more convincing than in typical calibration exercises. Our results also suggests that in settings where search intensity matters that the urnball matching function with multiple applications peforms better than a Cobb Douglas matching function. Our approach of combining data from a randomized experiment with a structural model relates to Lise et al. (2015) who use the exogenous variation from a randomized experiment to validate a structural model for evaluating an employment program in Canada. A similar approach is applied for evaluating PROGRESA by Attanasio et al. (2012) and Todd and Wolpin (2006).

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 analysis and the estimation results. In section 5 we develop an equilibrium search model that incorporates the activation program. Estimation of the model and policy simulations are presented in section 6. Section 7 concludes.

2 Background

2.1 The Danish experiment

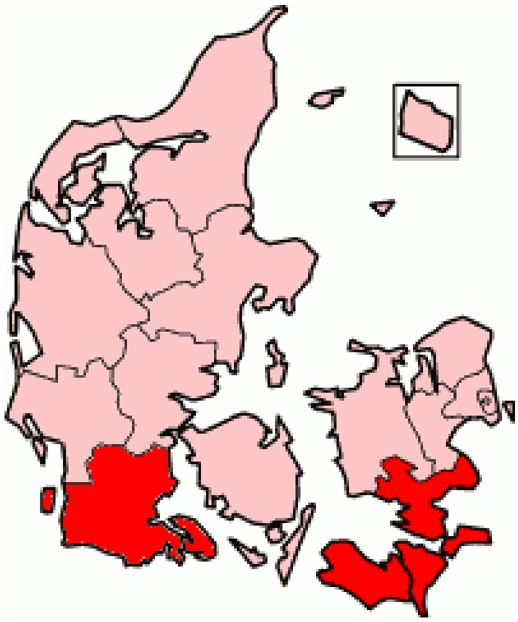
In this subsection, we provide some details about the activation program for unemployed workers. We discuss the randomized experiment used to evaluate the effectiveness of the program and review earlier evaluations of this experiment. More details on the institutional background can be found in Graversen 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, followed by weekly or biweekly meetings with a caseworker. During these meetings a job search plan is developed, search effort is monitored and suitable vacancies are provided. Finally, if after four months the worker has not found 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 DK (about 285 Euro, 355 USD), on average per entitled worker.[1]

To evaluate the effectiveness of the activation policy, a randomized experiment was conducted in two Danish counties, Storstrøm and South Jutland (see Figure 1). Both counties 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 who started collecting unemployment benefits between November 2005 and February 2006 participated in the experiment. Individuals born on the first to the 15$^{\text{th}}$ of the month participated in the activation program,

[1]These costs are in addition to the costs of the usual assistance offered to unemployed workers in Denmark.

Figure 1: Location of the experiment counties: South Jutland (left) and Storstrøm (right).



while individuals born on the 16th to the 31st 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.

At the time of the experiment Denmark consisted of 15 counties and had a population of about 5.5 million. Storstrøm and South Jutland each had about 250,000 inhabitants and both counties volunteered to run the experiment. The unemployment rate in Denmark was about 4.2 %. Unemployment benefits are relatively high, with an average of about 14,800 DKK per month (1987 EUR) and an average replacement rate of between 65 and 70 %. 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 large positive effects. The program increases the re-employment rate by about 30 %, 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 Van Ours (2008), Rosholm (2008) and Vikström et al. (2013) all investigate which elements of the activation program are most effective, and report a substantial threat effect as well as a positive effect of the job search assistance. Additional evidence for threat effects is provided by Graversen and Van Ours (2011). They show that the effect of the activation program is largest for individuals with the longest travel time to the program location. The finding that activation programs can have substantial threat effects is in agreement with Black et al. (2003).

All studies on the effect of the activation program ignore potential spillover effects between participants and nonparticipants. Graversen and Van Ours (2008) argue that spillover effects should be small because the fraction of the participants in the total population of unemployed workers never exceeds 8 %. However, we estimate that within an experiment county the fraction of participants in the stock of unemployed workers is much larger towards the end of the experiment period. Consider the following simple approximation. Around 5 % of all unemployed workers find work each week, implying that if the labor market is in steady state, after four months about 25 % of the stock of unemployed workers are participants. 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 will be close to 30 % of the stock of unemployed workers. Spillover effects seem plausible for such a treatment share.

2.2 Treatment externalities

In this subsection we briefly illustrate the definition of treatment effects in the presence of treatment externalities and discuss some recent empirical literature dealing with treatment externalities. We mainly focus on labor market applications, but also address 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 provides an estimate for the average treatment effect in the treated 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 roll 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 the treatment status of every other individual. Simplifying this extreme case somewhat, assume that only the fraction of the population in the same area receiving treatment is relevant. The latter is defined by $\tau = \frac{1}{N}\sum_{i=1}^N D_i$. In the case of the Danish activation program, the county is taken as the relevant area and assumed to act as the 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 roll out of a treatment, the policy relevant treatment effect is

$$\Delta = \frac{1}{N}\sum_i^N E[Y_{1i}^*|\tau = 1] - E[Y_{0i}^*|\tau = 0] \tag{2}$$

Identification of this treatment effect requires observing local labor markets in which all unemployed workers participate in the program as well as local labor markets in which no individuals participate. A randomized experiment within a single local labor market does not provide the required variation in the treatment intensity τ.

SUTVA might be violated in the case of activation programs for a number of reasons. 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 can respond by opening more vacancies. These equilibrium effects not only affect the nonparticipants but also the other participants in the program. In section 5 we provide a formal discussion of potential equilibrium effects due to the activation program.

As discussed in the previous subsection, the randomized experiment to evaluate the activation program was conducted in two Danish counties. The experiment provides an estimate of $\Delta(\hat{\tau})$, where $\hat{\tau}$ 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 counties. This should provide an estimate for $E[Y_{0i}^*|\tau = \hat{\tau}] - E[Y_{0i}^*|\tau = 0]$, i.e. the treatment effect on the non-treated workers. To deal with structural differences between counties, we use outcomes in all counties prior to the experiment and make a common trend assumption. In section 4 we provide more details about the empirical analysis. Still, the empirical approach only identifies treatment effects and equilibrium effects at a treatment intensity $\hat{\tau}$, while for a large-scale roll out of the program one should focus on $\tau = 1$. Therefore, in section 5 we develop an equilibrium search model, which we estimate using the estimated treatment effects. This model is used to investigate the case in which all unemployed workers participate ($\tau = 1$) and to obtain an estimate for the policy relevant treatment effect 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 is based on differences in timing of the implementation between regions, as well as on age requirements. The empirical results are inconclusive with regard to equilibrium effects. However, after using a more structural approach, Blundell et al. (2003) show that treatment effects can change sign when equilibrium 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 a conditional independence assumption to identify treatment effects. Lise et al. (2004) specify a matching model to quantify equilibrium effects of a wage subsidy program. Using experimental data, the model is calibrated to the control group and is found to be able to 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.

Crépon et al. (2013) perform a randomized experiment to identify equilibrium effects of a counseling program France. In addition to randomized program participation, the share of participants across regions is also randomized. The target population consist of high-educated unemployed workers below age 30 who had been unemployed for at least six months. The program affects treated workers positively, while a small negative (and statistically insignificant) effect on the non-treated workers is found. The small spillover effects can be due to the fact that the treated workers constitute only a small fraction of the total stock of unemployed workers. The fraction is small partly because of the specific target group and partly because participation (conditional on assignment to treatment) is voluntary and refusal rates are high.

Also outside the evaluation of active labor market programs, treatment externalities have received interest. Heckman et al. (1998) find that the effect of the size of the tuition fee on college enrollment becomes substantially smaller when general equilibrium effects are taken into account. Miguel and Kremer (2004) find spillover effects of de-worming drugs on schools in Kenya. Duflo et al. (2011) 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

We use two data sets. The first is an administrative data set describing unemployment spells, and subsequent earnings and hours worked. The second is a panel data set of the stock of open vacancies by county. Below we discuss both data sets in detail.

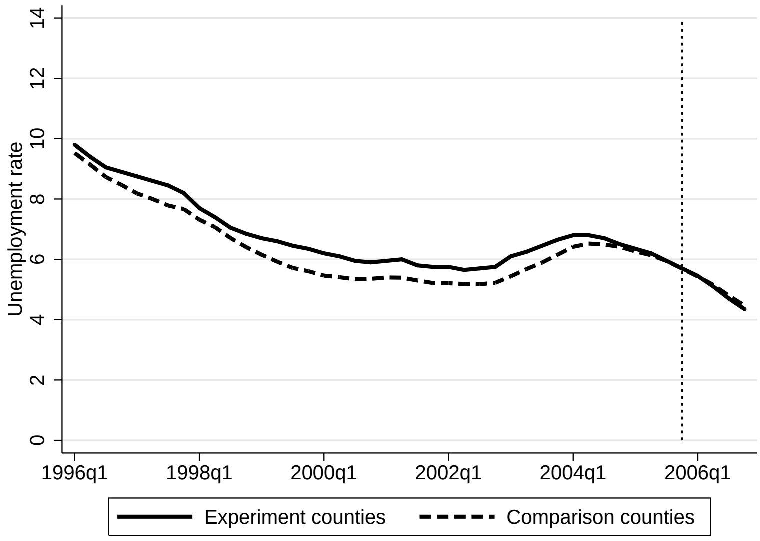
The experiment involved all individuals becoming unemployed between November 2005 and February 2006 in Storstrøm and South Jutland. The data are provided by the National Labor Market Board and include all 41,801 individuals who applied for regular benefits in the experiment period in all Danish counties.[2] We remove 1398 individuals from the sample for which the county of residence is inconsistent. Of the remaining 40,403 observations, 3751 individuals are living in either Storstrøm or South Jutland and participate in the experiment. Of the participants in the experiment, 1814 individuals are assigned to the treatment groups and 1937 to the control group.

The data also include individuals who started collecting benefits one year before the experiment period (between November 2004 and February 2005) and two years before the experimental period (between November 2003 and February 2004). We refer to these periods as the pre-experiment periods. In the empirical analysis we will use one pre-experiment period, containing those that enter one year before the experimental period. This period contains 49,063 individuals.

For each worker we observe the week in which (s)he starts 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 earnings shows that almost all observed exits in the first two years are to employment.

[2]We exclude Copenhagen, because it differs from the rest of Denmark in terms of labor market characteristics.

Figure 2: Pre-experiment unemployment rate in experiment and comparison counties.



Source: Statistics Denmark.

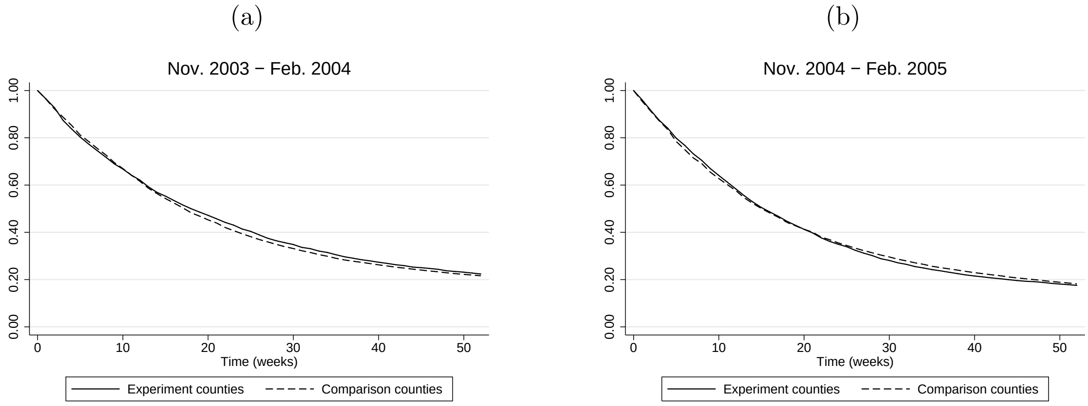
3.1 Pre-experiment trends

Our identification strategy requires the common trend assumption, stating that in the absence of the experiment, the trends in the unemployment duration are the same in the experiment counties and the comparison counties. Because Storstrøm and South Jutland volunteered to run the experiment, we first explore whether this assumption is likely to hold by investigating the pre-experiment years. Figure 2 shows the average unemployment rate in the experiment counties and the comparison counties in the ten years preceding the experiment. The vertical line indicates the start of the experiment. Changes in unemployment over time are very similar in the two groups, though the decrease in unemployment in 2004 is slightly larger in the experiment counties.[3]

Next, we compare outflow from unemployment in the pre-experiment periods. Figure 3 shows the Kaplan-Meier estimates for the survivor functions, for individuals who started collecting benefits in the pre-experiment periods (November 2003 until February 2004 and November 2004 until February 2005). Again, we distinguish between the experiment counties and comparison counties. To correct for differences

[3]In the empirical analysis we use one pre-experimental period, which runs from November 2004 to February 2005. During this period the unemployment rate is virtually identical in the experiment and comparison counties. If anything, changes in economic conditions are slightly more favorable in the experiment counties, leading to an underestimation of spillover effects.

Figure 3: Survivor functions for the experimental counties and the comparison counties in the years before the experiment.



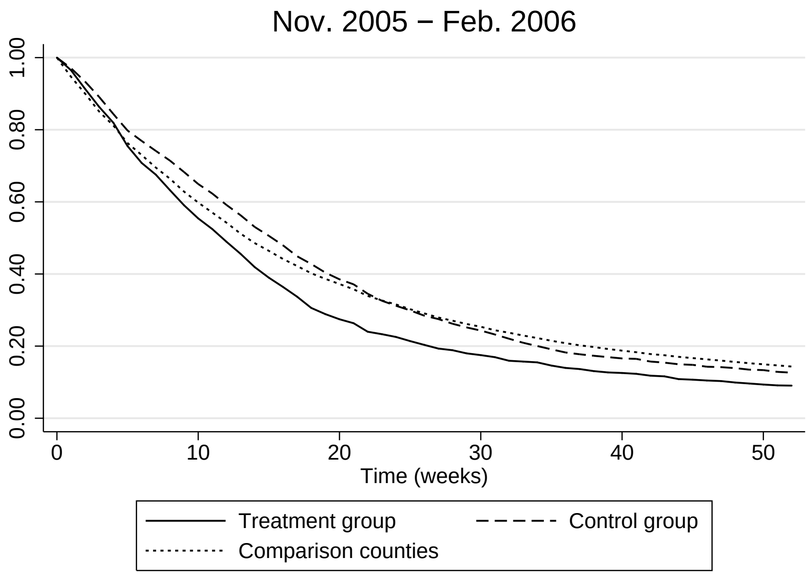
in observed characteristics, each survivor is weighted based on the distribution of gender, unemployment history and ethnicity in the comparison counties in the 2005-2006 period.

Panel (a) of Figure 3 shows that in the 2003-2004 period, the experiment counties were very similar to the comparison counties. The median unemployment duration is 18 weeks in the experiment counties and 17 weeks in the comparison counties. After one year, in both groups, 78 % of the unemployed leave unemployment. A logrank test cannot reject the null hypothesis that the distributions of unemployment durations in the experiment counties and in the comparison counties are the same (with a p-value of 0.25). Also in the 2004-2005 period (panel (b) of Figure 3) the survival functions of the experiment counties and comparison counties are very similar. For both groups, the median unemployment is 15 weeks. Again, a log-rank test can not reject that the unemployment distributions of the two groups are the same in 2004-2005 (with a p-value of 0.24). The strong similarity of the survivor functions in both pre-experiment periods suggests that, at least in terms of outflow from unemployment, the comparison counties and the experiment counties face similar trends.

3.2 Experimental period

Next, we consider individuals who entered unemployment in the experiment period (November 2005 until February 2006). Figure 4 shows the Kaplan-Meier estimates for the treatment and control group in the experiment counties and for individuals living in the comparison counties. 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 12

Figure 4: Survivor functions for the comparison counties, the control group and the treatment group during the experiment.



weeks about 50 % of the treated individuals leave unemployment, while this is 16 weeks for individuals in the control group and 14 weeks for individuals living in the comparison counties. Within the treatment group 91 % of the individuals leave unemployment within a year, compared to 87 % in the control group and 86 % in the comparison counties. 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). 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 0.47.

The data contain for each individual the annual earnings and annual hours worked from 2003 until 2010. Combining this information with the unemployment spells, we can compute weekly earnings for the period after the unemployment spell. Table 1 shows summary statistics for the experimental period and the pre-experimental year, for individuals in each of the five groups. On average, those individuals who are observed to have found work after unemployment, work about 35 hours per week and there are no substantial differences between the experiment countries and the comparison counties. The weekly earnings are higher in the experiment period than in the pre-experiment period and higher in the comparison counties than the experiment counties. Participants in the activation program work slightly more hours and have somewhat higher earnings than individuals in the control group.

The data include a number of individual characteristics. Age and immigrant status distributions are roughly similar across groups. In the experiment period

Table 1: Summary statistics.

	Experiment counties			Comparison counties	
	2004–2005	Treatment	Control	2004–2005	2005-2006
Hours worked (per week)	35.4	36.6	34.9	35.0	36.1
Earnings (DK per week)	5950	6271	6160	6256	6586
Male (%)	54.6	60.8	59.2	53.0	52.4
Age	42.0	42.4	42.3	41.3	41.2
Native (%)	94.8	93.2	94.4	93.7	93.0
West. Immigrant (%)	3.2	4.0	3.4	2.8	3.2
Non-West. Immigrant (%)	2.0	2.8	2.2	3.5	3.8
Benefits previous year (in weeks)	10.5	9.8	9.0	10.2	11.1
Benefits past two years (in weeks)	12.7	12.3	11.9	12.5	13.8
Previous hours worked (per week)	27.5	28.4	28.5	27.1	27.0
Previous earnings (DK per week)	4903	5191	5436	4993	5113
Education category: (%)					
no qualifying education	34.6	35.8	40.5	33.7	37.3
vocational education	49.4	50.7	47.6	45.2	44.2
short qualifying education	4.1	4.9	3.5	4.7	4.8
medium length qualifying educ.	9.8	5.9	6.3	11.6	8.7
bachelors	0.5	0.8	0.8	0.8	2.1
masters or more	1.5	1.9	1.3	4.0	3.1
Observations	5321	1496	1572	37,082	31,586
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

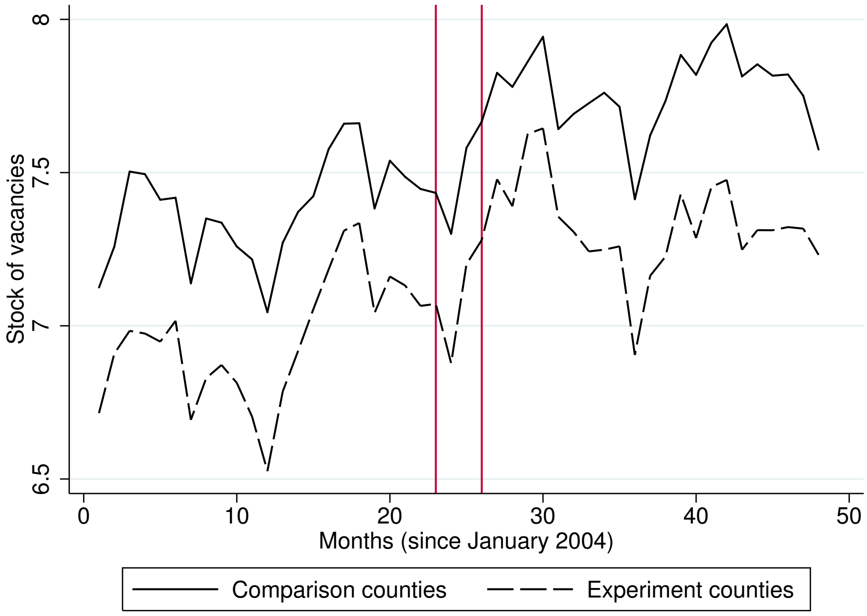
Source: Individual characteristics are based on own calculations, aggregate statistics are from Danmarks Statistik.

there is a higher fraction of males among those becoming unemployed in the experiment counties than in the comparison counties. In the comparison counties in the experiment period the unemployed workers have a slightly longer history of benefits receipt than in the pre-experiment period. Earnings and hours worked preceding the unemployment spell are roughly similar across groups and also in education categories there are only minor differences.

The lower panel of the table shows some county level statistics. In both the experiment counties and the comparison counties the local unemployment rate declines and GDP per capita increases between the pre-experiment and the experiment period. The labor force participation rate remains virtually unchanged. These changes are suggestive of similar calendar time trends in the experiment and comparison countries. However, in both time periods the labor market conditions are,

Figure 5: Logarithm of the stock of vacancies per month (experiment period between the vertical lines).

(a) Log vacancies



on average, more favorable in the comparison counties than in the experiment 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 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 5 shows how in both the experiment counties and the comparison counties the average number of open vacancies changes over time. Both lines follow the same business cycle pattern. During the experiment period and just afterwards, the increase in the vacancy stock is somewhat larger in the experiment counties than in the comparison counties.

4 Empirical Analysis

The previous section discusses descriptive evidence on the impact of the activation program. In this section we empirically investigate its effect on exit rates from unemployment, post-unemployment earnings and hours worked, 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 duration

The aim of the activation program is to stimulate participants to find work faster. In the presence of spillovers, a simple comparison of outcomes between 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 Binary outcomes

We first consider the effect of the program on the probability of exiting unemployment within a fixed time period. Let E_i be an indicator for exiting unemployment within this period. 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. To estimate the effect of the activation program on the participants and the non-participants, we estimate the following linear probability model:

$$E_i = \alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i + \eta_{p_i} + U_i \tag{3}$$

This is a difference-in-difference model. Differences in the probabilities to exit unemployment between counties are controlled for by county fixed effects α_{r_i}, where r_i describes the county in which individual i lives. The common time trend is described by η_{p_i}, where p_i is either the experiment period or the pre-experiment period. The dummy variable d_i is equal to one if individual i belongs to the treatment group, and c_i is equal to one if individuals i belongs to the control group.[4] A vector of covariates (x_i) contains gender, immigrant status, age dummies, education level, log previous earnings, history of benefit receipt and 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 participants and nonparticipants, respectively. The parameter γ thus describes possible spillover effects. The key identifying assumption for the spillover

[4]The previous program evaluations (Graversen and Van Ours (2008), Rosholm (2008) and Vikström et al. (2013)) showed that the program was effective from the very start (it was found that even before any program started, the announcement letter had a positive ‘threat’ effect on outflow).

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

	three months (1)		one year (2)		two years (3)	
Participants	0.059	(0.007)***	0.039	(0.004)***	0.010	(0.005)**
Nonparticipants	-0.033	(0.014)**	0.013	(0.003)***	-0.006	(0.003)**
Mean dependent variable[a]	0.500		0.901		0.969	
Individual characteristics	yes		yes		yes	
County fixed effects	yes		yes		yes	
Observations	77,057		77,057		77,057	

Note: Clustered standard errors in parentheses. * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Individual characteristics include gender, age dummies, education level, log previous earnings, immigrant status, labor market history and quarter of entering unemployment. [a]The aggregate outflow probability in the experiment counties during the experiment.

effects is a common trend in exit probabilities between the experiment counties and the comparison counties. The randomized experiment identifies the difference in exit probabilities between participants and nonparticipants in the experiment counties, which is $\delta - \gamma$.

Table 2 shows the parameter estimates for the linear probability model. 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 positive and highly significant. The decrease in size is not surprising. After longer periods the fraction of survivors is reduced substantially and the parameter estimates describe absolute changes in survival probabilities. Also Graversen and Van Ours (2008), Rosholm (2008) and Vikström et al. (2013) find that the effect of the activation program is largest early during the unemployment spell.

After three months, participants in the program are more than 9 %-points $(0.059 + 0.033)$ more likely to have found work than the nonparticipants, but over one third of this difference is due to reduced job finding of the nonparticipants. The effect of the activation program on those randomly assigned to the control group during the experiment is substantial and significant after three months. During this period the activation program is most intense, containing a job search assistance program and frequent meeting with caseworkers. Early in the unemployment spell, relatively many participants in the activation program leave 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 smaller in magnitude and has changed sign. After two years, the negative effect on the nonparticipants is more than half the size of the effect on the participants. Both effects are significant, but small. Only slightly more than 3 % of the participants in the experiment are still unemployed after two years.

4.1.2 Log duration model

A disadvantage of the linear probability model is that is uses only part of the available information on unemployment duration. Therefore, we estimate a linear model using the log of unemployment duration as the dependent variable. We use the same difference-in-differences specification.

$$\log(T_i) = \alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i + \eta_{p_i} + U_i \tag{4}$$

A problem with this approach is that it cannot deal with censoring. However, because censoring occurs after two years, only 3 % of the observations are censored. Estimation results are presented in column (1) of Table 3 and are in line with the results in Table 2. The activation program reduces the unemployment duration of participants by approximately 14 %, while the unemployment duration of non-participants increases by approximately 7 %. Both effects are significant at the 1 % level.

4.1.3 Duration model

Previous studies used duration models to evaluate the experiment (Rosholm (2008) Graversen and Van Ours (2008)). Therefore, we also specify a proportional hazard model for the exit rate from unemployment. The exit rate at duration t (measured in weeks) is described by $\theta(t)$ and has the following specification,

$$\theta(t|p_i, r_i, x_i, d_i, c_i) = \lambda_{p_i}(t)\exp(\alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i) \tag{5}$$

where $\lambda_{p_i}(t)$ describes duration dependence, which is allowed to differ between individuals who entered unemployment in the experiment period and those that enter in the pre-experiment period. This difference captures the common time trend. All other notation is the same as in the previous models.

To estimate the parameters of interest we use stratified partial likelihood estimation (e.g. Ridder and Tunalı (1999)).[5] The key advantage is that this does not

[5] We tried estimating the model parameters using MCMC methods allowing for unobserved

Table 3: Estimated effects of the activation program on log unemployment duration, post-unemployment wages and hours worked.

	(1) Log unemployment duration		(2) Log Weekly earnings		(3) Weekly hours worked	
Participants	-0.14	(0.02)***	0.01	(0.02)	0.87	(1.24)
Nonparticipants	0.07	(0.02)***	0.01	(0.02)	0.05	(1.22)
Ind. characteristics	yes		yes		yes	
County fixed effects	yes		yes		yes	
Observations	77,057		68,979		68,980	

Note: Clustered standard errors in parentheses. * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Individual characteristics include gender, age dummies, education level, immigrant status, log previous earnings, labor market history and quarter of entering unemployment. For the hours regression (column (3)) it also includes previous hours worked.

require any functional form restriction on the duration dependence pattern $\lambda_{p_i}(t)$. 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 exit from unemployment was observed and value 0 if the observation has been censored. Stratified partial likelihood estimation optimizes the likelihood function

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

The set $\mathcal{I}_p$ 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. A Hausman test rejects that the pattern of duration dependence is the same in both time periods (p-value less than 0.01). This coincides with the earlier discussion that labor market conditions were relatively favorable at the moment of the experiment. It stresses the importance of allowing for calendar time effects in the hazard rate.

Column (1) of Table 4 shows the estimates using all information on unemployment durations in the data. Participating in the activation program increases the exit rate from unemployment with $100\% \times (\exp(0.154) - 1) \approx 17\%$ compared to not having any activation program. The effect is significant at the 1 % level. The effect

heterogeneity. Since there is not much dispersion in unobserved heterogeneity, the estimated treatment effects are very similar. Only because standard errors are much smaller than in the Cox model, both the treatment effect on the treated and on the nontreated are highly significant.

Table 4: Estimated effects of the activation program on exit rates of participants and nonparticipants.

	Data censored after:					
	2 years		1 year		3 months	
	(1)		(2)		(3)	
Participants	0.154	(0.031)***	0.167	(0.032)***	0.151	(0.042)***
Nonparticipants	-0.044	(0.030)	-0.031	(0.031)	-0.115	(0.044)***
Individual characteristics	yes		yes		yes	
County fixed effects	yes		yes		yes	
Observations	77,057		77,057		77,057	

Note: Standard errors in parentheses. * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Individual characteristics include gender, age dummies, education level, log previous earnings, immigrant status, labor market history and quarter of entering unemployment.

of the presence of the activation program on the exit rate of the nonparticipants in the program is negative, however, not significant.

The results in subsection 4.1.1 show that most effects of the activation program are in the first months of the program. This is in line with Rosholm (2008). The proportional hazard model assumes that the effect of the activation program on the exit rate remains constant during the period of unemployment. As a result, the estimated effect of the program is an average over the observation period of two years. Our previous results suggest that the program effect might be larger early in the spell of unemployment. Therefore we estimate the same proportional hazard model, but censor unemployment spells after either one year, or three months. Results are shown in column (2) and (3) of Table 4. Censoring the data after one year has little effect on the results, the estimated effects are close to those in column (1). When we censor the spells after 3 months, the negative effect of the program on non-participants is much larger in magnitude and significant at the 1 % level (see column (3)). The coefficient corresponds to a 11 % decrease in the exit rate during the first 3 months of unemployment for the nonparticipants. The effect for the participants remains similar to both other specifications.

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 the nonparticipants. A simple comparison of the participants and nonparticipants overestimates the effectiveness of the activation program.[6]

In our specification we allow the duration dependence pattern to be different in both calendar time periods and we include 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 try restricting the group of comparison counties. We include only counties located closely to the experiment counties, 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 and can be found in appendix A.

4.2 Earnings and hours worked

Participation in the activation program may affect not only job finding, but also the quality of the job. We consider weekly earnings and hours worked after unemployment. If, for example, the activation program induces job seekers to lower their reservation wage, they may find jobs faster, but will have lower earnings, on average. On the other hand, if the program points the job seekers to the most suitable jobs, this may result in better matches and higher earnings, on average. Similar arguments can be made for hours worked. We estimate a model similar to equation (3):

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

where the outcome Y_i is either the logarithm of weekly earnings of individual i or hours worked per week.[7] In the set of covariates we include respectively weekly earnings and hours worked prior to becoming unemployed. The results are presented in columns (2) and (3) of Table 3. These show no effects of the activation program on both the participants and the nonparticipants.

We conclude that even though the activation program significantly reduces the duration until job finding for participants, and increases the duration for nonparticipants, the program has no impact on post-unemployment earnings and hours worked.

[6] In theory, we can allow the treatment effects δ and γ to depend on the treatment intensity τ. This is possible because workers enter unemployment at different moments in the experiment period and the treatment intensity (the share of treated in the stock of unemployed) changes over calendar time. However, this provides estimates that are imprecise and also not robust to different specifications.

[7] Both earnings and hours are measured as the monthly average over the remainder of the calendar year in which employment was found.

4.3 Vacancies

The results in the previous subsection provide evidence for treatment externalities. A likely channel is that unemployed job seekers compete for the same job openings, 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. Additionally, the increase in job search effort affects the efficiency of the matching process (either positively or negatively). Firms observing these changes adapt the number of vacancies, which influences both participants and non-participants. In this subsection we investigate to what extent the stock of vacancies is affected by the experiment.[8]

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

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

This is, again, a difference-in-differences model. The parameter of interest is δ, which describes the fraction by which the stock of vacancies changes during the experiment. The key identifying assumption is that the experiment counties and the comparison counties 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 are 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 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).

Table 5 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 5 % in the experiment counties but this effect is not significant. 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. Therefore, we allow the effect of the experiment to change over time. The parameter estimates reported in column (2) show that during the experiment the stock of vacancies started to increase in the experiment counties compared to other counties. This effect peaks in May/June, which is three to four months after program assignment stopped and decreases afterwards.

[8] The vacancy data come from job centers. We assume that the total stock of vacancies (formal and informal) is correlated with the vacancies posted in job centers and that the use of job centers is not affected by the experiment.

Table 5: Estimated effect of the experiment on logarithm of vacancies.

	(1) Log Vacancies		(2) Log Vacancies		(3) Log Vacancies		(4) Log Normalized Vacancies	
Experiment	0.047	(0.050)						
Exp. nov/dec 05			0.057	(0.084)	0.007	(0.055)	0.050	(0.092)
Exp. jan/feb 06			0.067	(0.032)*	0.016	(0.032)	0.051	(0.033)
Exp. mar/apr 06			0.081	(0.033)**	0.031	(0.041)	0.046	(0.036)
Exp. may/june 06			0.182	(0.046)***	0.132	(0.034)***	0.128	(0.033)***
Exp. july/aug 06			0.114	(0.027)***	0.064	(0.031)*	0.095	(0.024)***
Exp. sept/oct 06			-0.049	(0.046)	-0.099	(0.068)	-0.098	(0.066)
County f.e.	yes		yes		yes		yes	
Month f.e.	yes		yes		yes		yes	
Obs. period	Jan 04–Dec 07		Jan 04–Dec 07		Jan 05–Dec 06		Jan 04–Dec 07	

Note: Robust standard errors in parentheses, * indicates significant at 10% level, ** at the 5% level and *** at the 1% level. Column (4) present results from an estimation weighted with the county-variance of log-vacancies, in the pre-experiment periods.

Column (3) presents the same analysis as column (2), but restricts the observation period to January 2005 - December 2006. The pattern in the effects of the experiment on the stock of vacancies remains similar, although fewer parameter estimates are significant. Estimated effects are slightly smaller, while standard errors are sometimes somewhat larger and sometimes somewhat smaller. Finally, as in the empirical analyses on unemployment durations, we perform a robustness analysis restricting the set of comparison counties. The estimated effects vary somewhat depending on the choice of the set of comparison counties. Overall, both the estimated effects of the experiment and the standard errors increase somewhat (the estimation results are provided in appendix A).

5 Equilibrium analysis

The empirical results on unemployment duration, earnings and the stock of vacancies indicate the presence of equilibrium effects. Nonparticipants in the experiment have somewhat reduced exit rates from unemployment, the stock of vacancies increases after three months (though the magnitude of this effect varies between specifications) and the activation program does not affect earnings and hours worked conditional on employment. 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 roll out of the program changes 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 analyse how externalities vary with the treatment intensity of the activation program. The model is estimated by indirect inference where we use the estimates in the previous section as the auxiliary model given a treatment rate of approximately 30 %. 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 for all unemployed individuals.

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's 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. Program participation reduces the costs of sending an application but costs time. It may also affect the effectiveness of a given application. Recall that the goal of the activation program was to stimulate job search effort. The regular meetings did not include elements that increase human capital or productivity (e.g. Graversen and Van Ours (2008)). Indeed, we do not find any effect of the activation program on job characteristics. Firms are also assumed to be identical. Although the nature of the experiment is temporary, we consider a labor market in steady state. Since unemployment inflow and outflow are high in Denmark, this assumption is not too restrictive. Furthermore, within our stationary model, we treat the program as being permanent.[9] 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 nonmarket 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 is successful, otherwise she remains unemployed and must apply again

[9] When estimating the model, we take into account that workers do not expect program participation in the future, which reflects the temporary nature of the experiment. The model used for policy simulations allows workers to take future program participation into account.

in the next period. Making job applications is costly, and we assume these costs to be quadratic in the number of applications, i.e. $\gamma_0 a^2$. Convex search costs are convenient and can be motivated by the fact that workers consider the "low hanging fruit" first and that finding additional vacancies becomes increasingly difficult.

An important feature of the 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 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 $\bar{a}$. As will be explained below the matching function is different for participants and nonparticipants in the activation program.

Let r be the discount rate and $E(w)$ 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 following Bellman equation,

$$U_0 = \max_{a \geq 0} \frac{1}{1+r} \left[b + h - \gamma_0 a^2 + m_0(a; \bar{a}, \theta) E(w) + (1 - m_0(a; \bar{a}, \theta)) U_0 \right]$$

which can be rewritten as,

$$rU_0 = \max_{a \geq 0} b + h - \gamma_0 a^2 + m_0(a; \bar{a}, \theta) \left[E(w) - U_0 \right] \tag{6}$$

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

$$a_0^* = \frac{E(w) - U_0}{2\gamma_0} \frac{\partial m_0(a; \bar{a}, \theta)}{\partial a} \mid_{a = a_0^*} \tag{7}$$

The activation program consists of meetings with caseworkers and a job search assistance program which are both time-consuming for participants. We set the value of non-market time for the participants at zero and for the non participants at h (where at this moment we do not rule out that $h < 0$. The benefit of the program is that it reduces the costs of making job applications to $\gamma_1 < \gamma_0$. This implies that for participants in the activation program the value of unemployment is given by

$$rU_1 = \max_{a \geq 0} b - \gamma_1 a^2 + m_1(a; \bar{a}, \theta) \left[E(w) - U_1 \right]$$

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

$$a_1^* = \frac{E(w) - U_1}{2\gamma_1} \frac{\partial m_1(a; \bar{a}, \theta)}{\partial a} \, |_{a=a_1^*} \tag{8}$$

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 ignore on-the-job search. This is also motivated by data restrictions; our data do not contain any information on job-to-job transitions. With probability δ a job is destroyed and the employed worker becomes unemployed. Under the assumption that wages are paid at the end of the period, the value function for the state of employment at wage w is,

$$rE(w) = w - \delta(E(w) - \bar{U}) \tag{9}$$

When estimating the model parameters we take account of the temporary nature of the experiment and replace $\bar{U}$ in equation (9) with U_0.[10] However, for our policy simulations which consider the role out of the program, we use $\bar{U} = \tau U_1 + (1-\tau)U_0$, where τ is the fraction of workers who start participating in the activation program when becoming unemployed.

Vacancies are opened by firms at a per period cost of c_v. The probability of filling a vacancy depends on the fraction of unemployed workers participating in the activation programs, the the application behavior of participants and nonparticipants, and on labor market tightness θ. The probability of filling a vacancy is (given that the matching function exhibits constant returns to scale), $\frac{m(a_0^*, a_1^*; \tau, \theta)}{\theta}$, which we derive below. The value of a vacancy V follows from,

$$rV = -c_v + \frac{m(a_0^*, a_1^*, \tau, \theta)}{\theta}(J(w) - V) \tag{10}$$

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

$$rJ(w) = y - w - \delta(J(w) - V) \tag{11}$$

[10]This assumes that employed workers do not expect to participate in the activation program. However, employed workers do not make behavioral decisions, so the more important implication is that unemployed workers know that next time they enter unemployment the experiment has ended.

5.2 Wages and the matching function

Wages are determined by Nash bargaining. The bargaining takes place after the worker and firm meet. Firms observe whether or not the unemployed worker participates in the activation program. In the absence of renegotiation, the different outside options for participating and non-participating workers provide them different equilibrium wages. Let β denote the bargaining power of the workers. Then, the generalized Nash bargaining outcome for a worker in participation state $i = \{0, 1\}$ implies

$$w_i^* = \arg\max_{w_i} (E(w_i) - U_i)^{\beta} (J(w_i) - V)^{1-\beta}.$$

Equilibrium wages follow from the first-order condition:

$$(1 - \beta)(w_i^* + \delta \bar{U} - (r + \delta)U_i) = \beta(y - w_i^*) \tag{12}$$

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 similar results in terms of labor market flows, vacancy creation and effects of the activation program. The outcomes are discussed in more detail in subsection 6.3.

Finally, we specify the matching rates $m_0(a; \bar{a}, \theta)$ and $m_1(a; \bar{a}, \theta)$ for nonparticipating and participating unemployed workers and $\frac{m(\bar{a},\theta)}{\theta}$ for vacancies. Since participation in the activation program reduces search costs, the matching functions allow for different search intensities of participants and nonparticipants. Moreover, they allow for congestion effects between unemployed job seekers. We adjust the matching function of Albrecht et al. (2006) to incorporate these factors.[11] 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.[12] If a firm receives multiple applications, it randomly selects one applicant who receives a job offer. The other applications are rejected. 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

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

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 approximately a Poisson random variable with mean $\bar{a}/\theta$. For a nonparticipant in the

[11] In a sensitivity analysis we consider a Cobb-Douglas matching function instead. See section 6.3.1 for details.

[12] The second coordination friction is absent in a usual Cobb-Douglas matching function.

activation program, an application results in a job offer with probability $\frac{1}{1+j}$, where j 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 for a program nonparticipant results in a job offer equals

$$\psi_0 = \sum_{j=0}^{\infty} \frac{1}{1+j} \frac{\exp(-\bar{a}/\theta)(\bar{a}/\theta)^j}{j!} = \frac{\theta}{\bar{a}} \left(1 - \exp\left(-\frac{\bar{a}}{\theta}\right)\right)$$

We allow the activation program to affect not only the application cost but also the success rate of an application. Suppose that a worker has a positive productivity for a fraction κ of the vacancies. After screening its candidate, the firm learns whether the worker is productive or not and if he is not. The probability that an application of a program participant results in a job offer is therefore

$$\psi_1 = \frac{\kappa\theta}{\bar{a}} \left(1 - \exp\left(-\frac{\bar{a}}{\theta}\right)\right)$$

When $\kappa < 1$ and $\gamma_1 < \gamma_0$, the program induces participants to make additional applications, but those applications are on average less successful than the ones he would send out by himself.

The matching probability for respectively a program participant ($i = 1$) and a non-participant ($i = 0$) who makes a applications is given by by

$$m_i(a_i; \bar{a}, \theta) = 1 - (1 - \psi_i)^{a_i}$$

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 (with a little abuse of notation) simply given by: $m(a_0^*, a_1^*; \tau, \theta) = \tau m_1(a_1^*; \bar{a}, \theta) + (1 - \tau) m_0(a_0^*; \bar{a}, \theta)$ and for reasonable values of θ it is first increasing in the number of applications per worker and then decreasing.[13] More applications per worker reduces the first coordination problem mentioned above (vacancies are more likely to get at least one applicant), but amplifies the second one (it is more likely that one unemployed worker receives multiple job offers).

5.3 Equilibrium and welfare

In steady state, the inflow into unemployment equals the outflow from unemployment, which gives

$$\delta(1 - u) = (\tau m_1(a_1^*; \bar{a}, \theta) + (1 - \tau) m_0(a_0^*; \bar{a}, \theta))u$$

[13] Only for very low values of θ, the matching function is monotonically decreasing in the number of applications.

The equilibrium unemployment rate is, therefore,

$$u^* = \frac{\delta}{\delta + \tau m_1(a_1^*; \bar{a}, \theta) + (1-\tau) m_0(a_0^*; \bar{a}, \theta)} \tag{13}$$

The zero-profit condition for opening vacancies $V = 0$ implies that the flow value of a filled vacancy (paying wage w_i) equals

$$J(w_i) = \frac{y - w_i^*}{r + \delta}$$

Substituting this into the Bellman equation for vacancies (10) gives

$$\frac{m(\bar{a}, \theta^*)}{\theta^*} = \frac{(r+\delta)c_v}{y - \bar{w}^*} \tag{14}$$

where $\bar{w}^*$ is the expected equilibrium wage, which is the weighted average of the wages earned by participating and non-participating workers: $(1-\tau)w_0^* + \tau w_1^*$.

The left-hand size is decreasing in θ and goes to infinity when θ approaches zero. Because wages are increasing in θ, the right-hand side is increasing in θ. Therefore, there is a unique θ^* that satisfies the equilibrium condition in equation (14). We can now define the equilibrium as the tuple $\{a_0^*, a_1^*, w_0^*, w_1^*, u^*, \theta^*\}$ that satisfies equations (7), (8), (12), (13) and (14).

After solving the model and deriving conditions for equilibrium, we can now use the model for policy simulations. The decision parameter for the policy maker is the intensity of the activation program (τ).[14] 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. Implicitly, we assume here that it is paid from a non-distortionary tax. Introducing distortionary taxes, would make the program less desirable. It would reduce net earnings and thereby reduce incentives to work. Besides those costs, a welfare analysis should take account of the productivity of the workforce $(1-u)y$, the costs of keeping vacancies open vc_v, and the time costs of unemployed workers $(h - \gamma_0 a_0^{*2})$ and $-\gamma_1 a_1^{*2}$ for nonparticipants and participants respectively. We define welfare as net (of all pecuniary and non-pecuniary cost) output per worker,

$$W(\tau) = (1-u)y + u\left((1-\tau)\frac{h - \gamma_0 a_0^{*2}}{1+r} + \tau\frac{-\gamma_1 a_1^{*2}}{1+r}\right) - \delta(1-u)\tau c_p - vc_v \tag{15}$$

[14] Most policy makers make a program available either to all eligible unemployed workers or to none. This implies that τ is either one or zero. However, in our policy simulations we consider also values between zero and one. This provides insights in the spillover effects of the program. Also cases exist where programs have a limited budget or capacity such that not all eligible unemployed workers can enroll.

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 roll out of the activation program would increase welfare $W(1) > W(0)$. The latter program effect is based on the policy relevant treatment effect defined in equation (2). Furthermore, we can compute the welfare-maximizing value for τ.

Alternatively, a naive policy maker 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 policy maker 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 policy maker chooses τ such that it minimizes the costs of the unemployment insurance program,

$$C_{UI}(\tau) = ub + \delta(1-u)\tau c_p \tag{16}$$

Finally, it is interesting to compare the results of these policy parameters to results from a typical microeconometric evaluation. As discussed in subsection 2.2 most microeconometric evaluations impose SUTVA, and typically compare the costs of a program with the 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)),

$$ME_{\tau=0.3} = \left(b \left(\frac{1}{m(a_1^*;\bar{a},\theta)} - \frac{1}{m(a_0^*;\bar{a},\theta)} \right) - c_p \right) \tag{17}$$

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 not only ignores equilibrium effects, but also, for example, foregone leisure of the participants, see Van den Berg and Van der Klaauw (2006).

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, the estimated model is used 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

Our key interest is the causal effect of modifying the intensity of the activation program on various aggregated labor market outcomes. To avoid making additional functional form assumptions in the estimation procedure, we estimate the equilibrium model using indirect inference. This also avoids that we, for example, have to allow for measurement errors and has the advantage that the estimation is less computer intensive than alternative approaches for estimating structural models. Furthermore, our approach estimates the structural parameters using mainly information directly related to the activation program. Indirect inference has the additional advantage that it is transparent about which information drives the identification of a parameter. Below we discuss the estimation in more detail.

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 to one month. Next, we fix the treatment intensity of the activation program during the experiment to 0.3 (see the discussion in subsection 2.1). We denote the treatment intensity during the experiment by τ^e. In subsection 6.3 we investigate the sensitivity of the results by estimating the model for alternative levels of τ^e. As mentioned before, the main goal of estimating the model is to learn what the policy relevant treatment effect ($\tau = 1$) is in steady state. However, when we estimate the model, we must take into account that workers realized that the program would not continue forever and therefore we take into account that workers expect that when they will lose their job, they enter state U_0 (i.e. they receive no treatment). The discount rate is set to 10 % annually, which implies that r is 0.008. This is 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 y is normalized to one. The upper panel of Table 7 summarizes the values for the model parameters that we fix a priori.

After fixing the discount rate, the treatment intensity and productivity, there are 8 remaining unknown parameters, which we estimate using indirect inference. The parameters are determined such that a set of data moments is matched as closely as possible by the corresponding model predictions.

We use 9 moment restrictions, presented in Table 6. The model should capture: (1) the unemployment and (2) vacancy rates in the experiment counties, (3) the estimated program effect on the participants and (4) on the nonparticipants, (5) the estimated increase in vacancies due to the experiment, (6) the average matching rate in the experiment counties, (7) the small (insignificant) wage increase for participants and (8) for nonparticipants and (9) the fact that unemployment bene-

Table 6: Moment conditions.

	Data moment	Inverse weight[a]	Description	Corresponding value model
1. Program effect on participants	0.059	0.007^2	Estimated effect (see Table 2)	$[1-(1-(m_1\|\tau=\tau^e))^3]-[1-(1-(m_0\|\tau=0))^3]$
2. Program effect on nonparticipants	-0.033	0.014^2	Estimated effect (see Table 2)	$[1-(1-(m_0\|\tau=\tau^e))^3]-[1-(1-(m_0\|\tau=0))^3]$
3. Program effect on log vacancies	4.7 %	0.05^2	Estimated percentage effect on vacancies (see Table 5)	$\frac{(v^*\|\tau=\tau^e)-(v^*\|\tau=0)}{(v^*\|\tau=0)}$
4. Outflow within three months	0.51	0.0007	Fraction of unemployed in Storstrøm and South Jutland that leaves unemployment within three months (see Figure 4)	$1-\tau(1-(m_1\|\tau=\tau^e))^3-(1-\tau)(1-(m_0\|\tau=\tau^e))^3$
5. Program effect on log wage participants	0.01	0.02^2	Estimated effect on wages (see Table 3)	$\frac{w_1\|\tau=\tau^e-w_0\|\tau=0}{w_0\|\tau=0}$
6. Program effect on log wage nonparticipants	0.01	0.02^2	Estimated effect on wages (see Table 3)	$\frac{w_0\|\tau=\tau^e-w_0\|\tau=0}{w_0\|\tau=0}$
7. Unemployment rate	0.05	0.0001	Unemployment rate Storstrøm and South Jutland during the experiment (see Table 1)	$u^*\|\tau=\tau^e$
8. Vacancy rate	0.01	0.001	Approximation of the number of vacancies as a percentage of the labor force in Storstrøm and South Jutland	$v^*\|\tau=\tau^e$
9. Replacement rate	0.65	0.001	Unemployment benefits are 65 % of the wage level	$b\frac{1}{(\bar{w}^*\|\tau=0)}$

[a] The inverse of the weight used in estimation equals the variance of the data moments. Moments 1-6 have variances estimated in the empirical models. Moments 7-9 do not have estimated variances. The unemployment rate is measured very precisely though, and thus we assign a large weight (thus a small variance). The vacancy rate is measured much more noisily and thus has a larger variance. The replacement rate (moment 9) is an approximate value and thus receives a low weight also.

fits are approximately 65 % of the wage level. Define $\xi = (\gamma_0, \gamma_1, \delta, \kappa, b, \beta, c_v, h)$ as the vector of parameters to be estimated. For given values of ξ the model can be solved and the set of model predictions can be computed. To obtain estimates for ξ, we minimize the sum of squared differences between the data moments and the corresponding model predictions over ξ, where each squared difference is given an appropriate weight based on the variance of the (estimated) data moment (see the third column in Table 6). This implies that the imprecision with which for example the vacancy effect and the wage effects are estimated, is taken into account.

The estimates for the parameters are presented in the upper panel of Table 7 (first column, baseline model). Standard errors are computed using the delta method.[15] 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.[16] The job destruction rate is slightly over 1 % per month. The parameter estimate κ indicates that the probability that an application results in a match is smaller for participants. Thus, program participants send more applications at a lower cost, but the applications have an approximately 12 % lower probability of being effective. [17] Unemployment benefits are 64 % of productivity, and the bargaining power of workers is 0.81. The leisure costs of participating in the activation program are close to zero and approximately 4 % of productivity.

The middle panel of Table 7 presents the fit of the model, by listing the difference between the model moment and the (targeted) data moments. The baseline model is able to reproduce the empirical findings. The effect of the program on participants and nonparticipants are both matched perfectly. Also the positive effect on vacancies and the outflow probability within three months are matched perfectly. The wage effects are also close to the data but since the empirical wage effects were statistically insignificant (see Table 3) they receive a low weight in the estimation procedure. Finally, the unemployment rate, vacancy rate and replacement rate are also matched very closely.

[15]Standard errors are computed as follows. Define the vector of data moments (listed in Table 6) as θ, with covariance matrix $\hat{\Sigma}_\theta$. The estimates of ξ are a function of the data moments: $\hat{\xi} = f(\theta)$. The covariance matrix of the estimated parameters is given by:

$$\hat{\Sigma}_{\hat{\xi}} = (\frac{\partial f(\theta)}{\partial \theta})\hat{\Sigma}_\theta(\frac{\partial f(\theta)}{\partial \theta})'$$

[16]Note that the relatively large standard errors for γ_0 and γ_1 are due to the fact that levels of the application cost parameters are not very precisely identified from the data. In particular, there are different levels that lead to almost similar fit of the model. The ratio of the two is very precisely identified though.

[17]Holzner and Watanabe (2016) give direct evidence for Germany that the fraction of suitable applicants at vacancies registered at the public employment office are 10 % less likely to be suitable.

Table 7: Structural model estimation and simulation results

		Parameters		
		Baseline model	Cobb Douglas	Bertrand wages
	Fixed			
Treatment share experiment	τ^e	0.3	0.3	0.3
Per period discount rate	r	0.008	0.008	0.008
Productivity	y	1	1	1
	Estimated			
Application cost nonpart.	γ_0	0.504 (0.142)	0.907 (0.663)	0.033 (0.009)
Application cost part.	γ_1	0.262 (0.080)	0.651 (0.467)	0.016 (0.007)
Job destruction rate	δ	0.011 (0.004)	0.011 (0.005)	0.011 (0.002)
Share of vacancies with positive surplus	κ	0.915 (0.040)	-	0.895 (0.043)
UI benefits	b	0.638 (0.007)	0.650 (0.205)	0.527 (0.034)
Bargaining power	β	0.702 (0.040)	0.991 (0.004)	-
Vacancy costs	c_v	0.981 (0.119)	2.217 (1.342)	9.349 (0.849)
Value non-market time	h	0.036 (0.038)	0.023 (0.155)	0.109 (0.027)
Search intensity elasticity	χ	-	0.000 (0.000)	-

		Model fit: deviations from data moments		
		Baseline model	Cobb Douglas	Bertrand wages
Effect on job finding nonpart.	-0.033	0.000	0.010	0.008
Effect on job finding participants	0.059	0.000	0.001	0.000
Effect on vacancies (%)	0.047	-0.004	-0.025	-0.004
Outflow within 3 months	0.5	0.000	0.000	0.000
Effect on wage non-treated (%)	0.01	-0.011	-0.010	-0.028
Effect on wage treated (%)	0.01	-0.009	-0.010	-0.004
Unemployment (for τ=0.3)	0.05	0.000	0.000	-0.001
Vacancy rate	0.01	0.001	-0.010	0.002
Replacement rate	0.65	0.000	0.000	0.004

	Key model predictions		
	Baseline model	Cobb Douglas	Bertrand wages
Policy relevant treatment effect[a]	-0.003 (0.009)	0.004 (0.006)	0.011 (0.001)
Experimental (naive) treatment effect[b]	0.050 (0.004)	0.045 (0.334)	0.046 (0.003)
Change in unemployment[c]	0.001 (0.002)	-0.001 (0.014)	-0.002 (0.0003)
Change in welfare[c,d]	-0.004 (0.003)	-0.001 (0.010)	-0.012 (0.002)
Change in government expenditure[c]	0.001 (0.001)	0.0003 (0.010)	-0.001 (0.0001)

Standard errors in parentheses.

[a] The policy relevant treatment effect is defined as $\mathrm{E}[m(a_1^*;\bar{a},\theta)|\tau=1] - \mathrm{E}[m(a_0^*;\bar{a},\theta)|\tau=0]$

[b] The experimental treatment effect estimate is defined as $\mathrm{E}[m(a_1^*;\bar{a},\theta)|\tau=0.3] - \mathrm{E}[m(a_0^*;\bar{a},\theta)|\tau=0.3]$

[c] All changes are defined as $(x|\tau=1) - (x|\tau=0)$

[d] Welfare is measured in terms of average monthly output per worker (which is normalized to 1) net of all costs. See equation (15) for details.

6.2 Increasing the intensity of the activation program

Next, we use the model to predict how the program effects depend on the fraction τ 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, market tightness, wages and welfare.

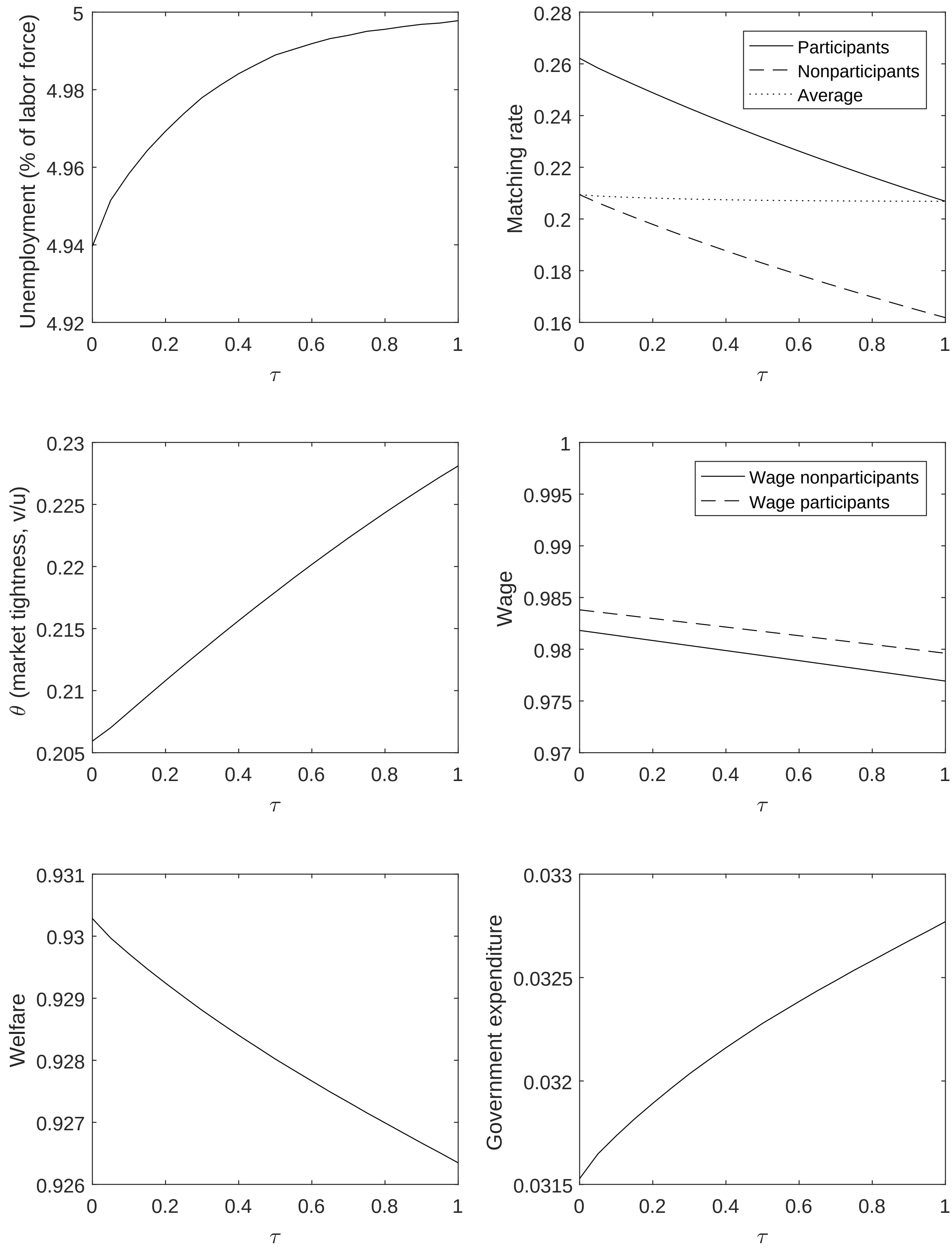
We simulate the model for a gradually increasing fraction of program participants τ in the unemployed population. The results are shown in Figure 6. The graph on the top-left shows that increasing the treatment rate τ monotonically increases the unemployment rate. The difference between no treatment and full treatment is approximately 0.1 %-points. The increase in unemployment can be explained by the matching rates which are presented in top-right graph. Because participants in the activation program send out more applications than nonparticipants, they always have a higher matching rate. The difference in matching rates remains stable for different values of τ and shows that participants are about 5 %-points more likely to find a job within a given month. The matching rates of both participants and nonparticipants decrease monotonically as τ increases and even the aggregate (average) matching rate decreases marginally. For a program intensity of 30 %, participants have a 28 % higher matching rate than nonparticipants.

The simulated matching rates can be related to the treatment effects presented in subsection 2.2. Results, including standard errors, can be found in the lower panel of Table 7. The evaluation of the randomized experiment estimates a treatment effect on the matching rates equal to $0.243-0.193 = 0.050$, which is $\mathrm{E}[m(a_1^*;\bar{a},\theta)|\tau = 0.3] - \mathrm{E}[m(a_0^*;\bar{a},\theta)|\tau = 0.3]$. However, the policy relevant treatment effect is $\mathrm{E}[m(a_1^*;\bar{a},\theta)|\tau = 1] - \mathrm{E}[m(a_0^*;\bar{a},\theta)|\tau = 0]$, which is $0.205 - 0.209 = -0.004$. Even though the microeconometric evaluation suggests a positive effect on the matching rate, the policy relevant treatment effect is close to zero.

Figure 6 shows that because of the congestion effects of the treatment, unemployment rises from 4.95 to 5.05 %. The higher unemployment rate combined with more applications per job seeker lead to an increase in the number of vacancies. As can be seen in the middle left graph, market tightness increases in τ, as vacancies supply increase more than unemployment. The wage negotiated by program participants is slightly higher than that of nonparticipants, due to their better outside option. The difference is small though, and both levels are relatively constant as τ increases. Furthermore, we find that the average number of applications per month in the population rises from 0.87 to 1.27 as τ increases from 0 to 1 (not shown in the graphs).

The estimated model allows for different types of cost-benefit analyses, as dis-

Figure 6: Simulation results baseline model.



cussed in section 5.3. Equation (15) defines welfare as a function of τ, which is plotted in the bottom-left graph of Figure 6. In line with rising unemployment, rising vacancies (and thus vacancies posting costs) and program costs, welfare decreases with τ and is thus maximized for $\tau = 0$. A large scale roll out of the program reduces welfare with 0.004, which corresponds to 0.4 % of workers productivity. The reason for the decline in welfare is that the negative congestion effects of the increased search intensity tend to dominate. Second, we consider total government expenditures on unemployment benefits and the costs of the activation program (see equation (16)). Presented in the bottom-right graph, we find that government expenditure increases in the share of the unemployed that participate in the activation program. A large scale roll out of the activation program increases total government expenditure on the unemployment benefits program by 5 %.

Finally, microeconometric evaluations often ignore equilibrium effects. Equation (17)) shows the cost-benefit analysis that is usually applied in microeconometric evaluations. It simply compares the costs of the program with the difference in total benefits payments between participants and nonparticipants. The costs of the program (c_p) are 2122 DK (about 382 dollar), while the change in average unemployment duration is 1.1 months. Average monthly benefit payments are 14,800 DK. The gain for the government budget is, therefore, 14,158 DK for each participant in the activation program. This microeconometric evaluation thus erroneously provides a positive assessment of the activation program.

The main conclusions from the analysis above is that even though matching rates of participants and nonparticipants are different, the aggregate matching rate slightly decreases with the intensity of the activation program. As a result, the program effects are not positive if equilibrium effects are taken into account. At 0 % program participation the unemployment rate is minimized and welfare is maximized. These conclusions do not concur with the results from a standard microeconometric evaluation that typically ignores equilibrium effects.

6.3 Robustness checks

In this subsection we address the robustness of our empirical results. We focus on modelling choices in the equilibrium search model. We made three key assumptions. First, our matching function is of the urn-ball type rather than the commonly used Cobb-Douglas type. Second, wages are determined by Nash bargaining. Third, the treatment intensity of 30 % is based on a steady state assumption. Below, we discuss alternatives to those assumptions.

6.3.1 The matching function

The urn-ball matching function with multiple applications and without full recall has the property that initially, the average matching rate is increasing in average search intensity, but for sufficiently high average search intensity a further increase in the number of applications reduces the matching rate. This captures the idea that a firm can fail to hire because it loses its candidate to other firms. The negative welfare effects are however not solely driven by congestion effects as we show below. One way to switch them off is by estimating the model with a Cobb-Douglas (CD) matching function. The CD function allows for different search intensities for treated and non-treated individuals, which are optimally chosen by the job seeker.

By allowing for a distinct elasticity (χ) for the aggregate search intensity, we use an aggregate matching function that is more flexible than the one that is commonly used in the literature. The aggregate number of monthly matches is given by

$$M(\bar{a}, u, v) = \bar{a}^{\chi} u^{\alpha} v^{1-\alpha} \tag{18}$$

where $\bar{a}$ is the average search intensity in the population. This specification is used because in a standard version[18], the estimate of α converges to zero (or to a negative value if no restriction is used in the estimation procedure). This is unrealistic and a clear signal that this version of the model is not flexible enough to reproduce the empirical findings. To keep the number of estimated parameters equal to the baseline model, we estimate χ and set $\alpha = 0.74$ (as was estimated for Denmark by Albaek and Hansen (1995)). Results are however very similar when using $\alpha = 0.5$. Parameter estimates and fit of the model can be found in the second column of Table 7. Note that the estimate for χ equals zero, indicating that the spillover effect on nonparticipants can only be matched with a non-positive relation between search intensity and aggregate matches. The fit of the model is not as good as in the baseline model. Specifically, both the spillover effect on nonparticipants and the effect on vacancies are only partially reproduced. Also the vacancy rate is much smaller than the targeted level. The simulation results (lower panel of Table 7) show that the main predictions are quite similar to the baseline model, though standard errors are larger. The policy relevant treatment effect is close to zero while the experimental (naive) estimate is positive. Unemployment decreases slightly, but the change in welfare is still negative and government expenditure increases as τ increases. The simulation results are summarized in Figure 8 in the Appendix. We conclude that the qualitative results are very similar to the ones in our baseline model but that the fit is worse. The policy implications remain the same.

[18] $M(\bar{a}, u, v) = (\bar{a}u)^{\alpha} v^{1-\alpha}$

6.3.2 The wage mechanism

In the baseline model, wages are determined by Nash bargaining. In subsection 5.2 ex post Bertrand competition was mentioned as an alternative wage setting mechanism. Below we briefly discuss the results from Bertrand competition (see Albrecht et al. (2006)).

Under Bertrand competition workers with one offer receive their reservation wage while workers with multiple offers receive the full match surplus. This has the theoretical advantage that it endogenizes the bargaining power which reduces the number of parameters to estimate by one. Appendix B provides a detailed description of the equilibrium search model with Bertrand competition.

Parameter estimates, the fit of the model and simulation predictions can be found in the last column of Table 7. The simulation results are presented in Figure 9 in the Appendix. We find that, except for a higher value of non-market time and very large vacancy costs, the parameter estimates are similar to the ones in the baseline model. Standard errors are smaller though. The fit of the model is not as good as in our baseline model. In particular, the program effect on nonparticipants and the vacancy effect are less well matched. The simulations predict that the policy relevant treatment effect is positive but small, while the experimental (naive) treatment effect is larger. Unemployment decreases slightly, but welfare still decreases when τ increases, as was the case in the baseline model. Government expenditures decrease slightly. Thus, the main conclusion, that due to spillovers the program leads to small but negative welfare effects, continues to hold under Bertrand wages.

6.3.3 Treatment intensity

The model has been estimated under the assumption that about 30 % of the unemployed workers were participating in the activation program towards the end of the experiment period ($\tau^e = 0.3$). The choice of this parameter follows from a steady state assumption with constant inflow and a 5% job finding rate. Both assumptions might be violated. First, the exit rate from unemployment shows negative duration dependence. If we take into account that the exit rate declines during the spell of unemployment, the fraction of program participants among the stock of unemployed workers reduces to about 26 %. Furthermore, recall from section 3 that the inflow into unemployment was higher in the pre-experiment year than in the experiment year. If we take this decline in inflow rate into account, the intensity of the activation program at the end of the experiment period is about 21 %. As a sensitivity check, we estimate the model using a lows values of $\tau^e = 0.25$ and $\tau^e = 0.2$. The intuition for how this changes results is straightforward. A lower value of τ^e implies that the observed negative program effects on the nonparticipants must be the result

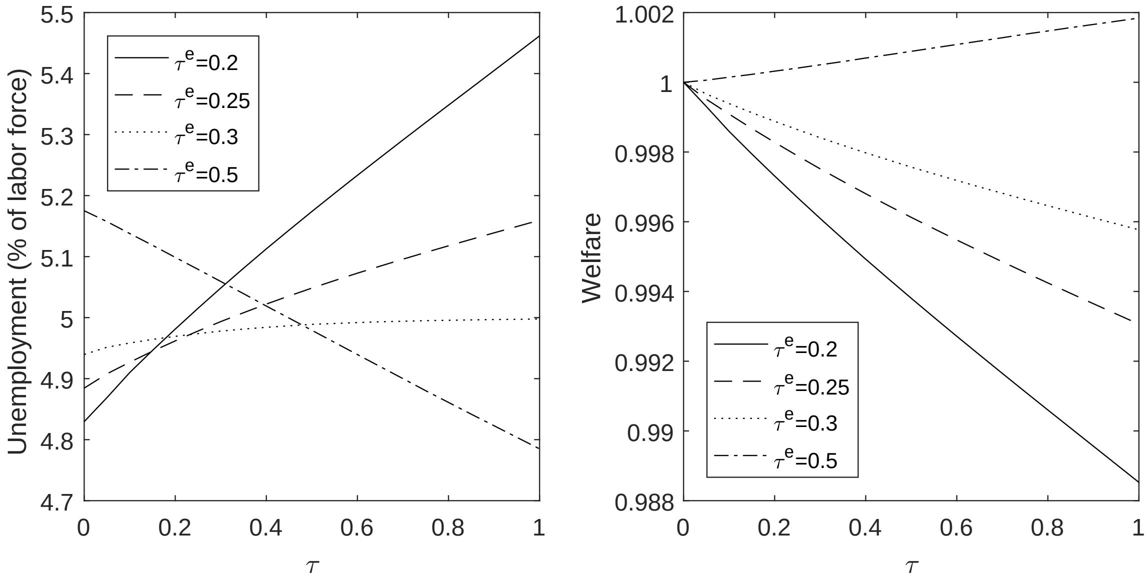


Figure 7: Simulation results from estimations using different values of τ^e

of a smaller group of treated individuals and thus congestion effects must be even larger. Consequently, welfare decreases faster.

Heterogeneity in the pool of unemployed could also affect the value of τ^e in a different way. One may argue that perhaps longer term unemployed are unable to find a job anyway, and therefore are not hurt by the higher job finding rate of the program participants. If one takes this argument to the limit, the spillovers effects are only relevant for the group of recent unemployed, which is the 50% of newly unemployed that were randomized out of the program. Note that this is an extreme upper bound, as it assumes that the entire stock of unemployed at the start of the experiment was unaffected by the experiment.[19] The resulting value of τ^e would be 0.5. Even though this is clearly an unlikely value, we present estimates based on $\tau^e = 0.5$ as a lower bound on the spillover effects.[20]

Taking both arguments into account we believe that $\tau^e = 0.3$ is a reasonable value for the treatment intensity during the experiment. We present results for

[19]Another argument against this case is the fact that almost everyone in our sample finds a job within 2 years, such that a scenario with zero job finding for long-term unemployed is unlikely.

[20]One may argue that our model describes the bottom segment of the labor market, where unemployment occurs more frequently. The unemployment rate among low-skilled unemployed workers is twice the overall unemployment rate and spillovers between labor market segments requiring different skill levels may be low. Therefore, we also estimated our model with a twice as high target unemployment rate of 10%. The results for this case are almost identical to our baseline results.

lower and higher values simply to demonstrate whether the conclusions are sensitive to the choice of this parameter. Simulation results based on the various values of τ^e are presented in Figure 7 together with the simulation results from the baseline model ($\tau^e = 0.3$). In the figure we show the unemployment rate and the welfare. To make the different estimates comparable, we normalize welfare to 1 in the case wherein no unemployed worker enters the activation program. As expected, a lower value of τ^e aggravates the negative effect of the activation program on the unemployment rate and welfare. For $\tau^e = 0.2$ the effect on unemployment and welfare are large, and full participation would increase unemployment by almost 1 %point and decrease welfare by more than 1%. Instead, setting $\tau^e = 0.5$ is sufficient to get a modestly positive effect of the program. In this scenario, full participation reduces unemployment slightly and increases welfare slightly (relative to the case where no workers participate in the activation program). These findings show that the model is able to generate positive welfare effects, but only by setting τ^e at unrealistically high levels. It illustrates that our negative baseline program effect estimates are not an artifact of the model structure, but are the result of the relatively strong negative spillovers that we observe at a realistic treatment intensity of 30%.

6.3.4 Heterogeneity, on-the-job search and steady state

Finally, we discuss some simplifying assumptions imposed in our equilibrium search model, and how these assumptions might affect the empirical results.

We assumed that workers and jobs are homogeneous. Clearly, there are differences between workers and between jobs. Allowing for two sided heterogeneity would complicate the analysis considerably, because sorting issues should be taken into account. Incorporating two-sided heterogeneity introduces other potentially interesting treatment effects. For example, if the employment office is able to lead workers to those jobs where their marginal productivity is highest, better matches can be created. In that case wages of the workers in the treatment group should be higher. However, as shown in section 4.2, there is no evidence for an effect on wages. Therefore, it suffices for our application to restrict ourselves to homogeneous workers, rather than extending the model to include mechanisms for which we do not find empirical evidence.

Another abstraction of our model is that we do not allow for on-the-job search. Allowing for on-the-job search increases the pool of job searchers and since only unemployed workers can receive treatment, it reduces the fraction of treated workers (τ^e) in the total pool of searchers. In section 6.3.3, we replicated our analysis for different values of τ^e and we found that reducing τ^e implies that workers impose larger negative spillovers on other workers. This is necessary to explain the observed differences in job finding rates between workers in experiment and non-experiment counties. So if anything, allowing for on-the-job search makes implementing the program economy wide, less desirable.

Finally, we assume that the labor market is in steady state, while the data from the experiment do not represent a steady state. In particular, the share of program participants in the pool of unemployed job searchers increases during the experiment period, and decreases again afterwards. The extent to which this discrepancy matters depends on how quickly the labor market moves towards steady state. In the case of Denmark this assumption is not very restrictive, because inflow and outflow rates in unemployment are high and in that case, the half-life of a deviation from steady-state unemployment is short, see Shimer (2012) and Elsby et al. (2009).

7 Conclusion

In this paper we investigate the existence and magnitude of equilibrium effects of an activation program for unemployed workers. Combining data from a randomized experiment in two Danish counties with data from other counties, we are able to not only estimate the program effect on the participants, but also on the nonparticipating individuals. We consider various outcomes. In particular, we find that the activation program increased job finding of participants, but had adverse effects on the job finding of the nonparticipants. This implies that simply comparing unemployment durations of participants and nonparticipants overestimates the effects of the activation program. For none of the groups was there an effect on the quality of the post-unemployment job. However, we find some suggestive evidence that the activation program increased the number of vacancies in the experimental counties.

These empirical findings are used to estimate an equilibrium search model. Despite its simplicity the model is able to replicate the empirical findings closely, especially when an urn-ball matching function is used rather than a Cobb-Douglas matching function. The structural parameters are estimated using indirect inference. The model allows for simulation of scenarios in which the number of participants in the activation program is increased up to a large scale roll out. This provides the policy relevant treatment effect of the activation program.

The simulation experiments show that despite the increased job finding rate for program participants and the increase in vacancies, the unemployment rate increases if the number of program participants is increased. Congestion effects in the labor market play an important role. A large-scale roll out of the activation program reduces welfare, not only because the unemployment rate increases and government expenditure on the activation program increases, but also because of increased search costs of unemployed job seekers. These results are robust against alternative specifications of the equilibrium search model. The findings do not concur with the results from a standard microeconometric evaluation, and thus emphasize the importance of considering spillover effects when evaluating labor market programs.

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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 as 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 8 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 4 shows that the estimated effects are quite robust against the choice of the comparison counties.

In Table 9 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 5.

B Equilibrium search model with Bertrand competition

In this appendix, we follow Albrecht et al. (2006) and assume that wages are determined by ex-post Bertrand competition rather than Nash bargaining. Bertrand competition implies that if a worker receives offers from multiple firms, wages are driven up to productivity ($w = y$). But if a worker only receives one offer, the firm receives the full surplus. In this latter case the worker receives the reservation wage ($w = w_l$). Therefore, the wage depends on the number of offers (denoted by j), and

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

		(1) 3 closest counties		(2) 2 furthest counties		(3) 5 most similar counties	
Exit within 3 months							
	Participants	0.072	(0.008)***	0.078	(0.010)***	0.073	(0.007)***
	Nonparticipants	-0.021	(0.014)	-0.015	(0.016)	-0.029	(0.014)
Log duration							
	Participants	-0.198	(0.019)***	-0.155	(0.019)***	-0.156	(0.022)***
	Nonparticipants	0.004	(0.022)	0.046	(0.020)*	0.047	(0.025)*
PH censored at 3 months							
	Participants	0.208	(0.046)***	0.205	(0.047)***	0.189	(0.043)***
	Nonparticipants	-0.061	(0.048)	-0.066	(0.049)	-0.079	(0.045)*
Log earnings							
	Participants	0.020	(0.025)	-0.005	(0.024)	0.009	(0.023)
	Nonparticipants	0.020	(0.025)	-0.007	(0.024)	0.009	(0.023)
Hours worked							
	Participants	1.002	(1.203)	0.364	(1.286)	0.531	(1.271)
	Nonparticipants	0.776	(1.191)	0.216	(1.275)	0.387	(1.258)
Ind. characteristics		yes		yes		yes	
County fixed effects		yes		yes		yes	
Observations		28,394		25,530		53,682	

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. Individual characteristics include gender, age dummies, education level, immigrant status, log previous earnings, labor market history and quarter of entering unemployment.

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.

the probability of receiving the low reservation wage given a match is:

$$p_l(a) \equiv \Pr(j = 1|j > 0) = \frac{\Pr(j = 1)}{\Pr(j > 0)}$$

Recall from subsection 5.2 that the probability that an application results in a job offer equals $\psi = \frac{\kappa\theta}{\bar{a}}\left(1 - \exp\left(-\frac{\bar{a}}{\kappa\theta}\right)\right)$, with $\kappa = 1$ for the nonparticipants. In a large labor market the number of job offers when making a applications follows a Poisson distribution with intensity ψa. This implies that

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

where $p_h(a)$ is the probability of receiving the high wage.

In the model with Nash bargaining, there are two wage levels (one for program participants and one for nonparticipants). In case of Bertrand competition, each of these types can have a low wage or be employed at $w = p$ and thus there are three wage levels in the population.

$$rE_{i,l}(w_{i,l}) = w_{i,l} - \delta(E_{i,l} - \bar{U})$$
$$rE_h = y - \delta(E_h - \bar{U})$$

where $i = 0$ for nonparticipants and $i = 1$ for participants. Note that E_h lacks a subscript i as both types of workers will receive $w = y$ in case of multiple offers.[21] For an unemployed worker who is in treatment state i and who sends out a applications, the expected value of employment equals

$$E_i = p_{i,l}(a_i)E_{i,l} + p_{i,h}(a_i)E_h$$

and the value functions of unemployed nonparticipants and participants are respectively:

$$rU_0 = \max_{a \geq 0} b + h - \gamma_0 a^2 + m(a; \bar{a}, \theta)\left[E_0 - U_0\right] \tag{19}$$
$$rU_1 = \max_{a \geq 0} b - \gamma_1 a^2 + m(a; \bar{a}, \theta)\left[E_1 - U_1\right] \tag{20}$$

Since participants and nonparticipants in the activation program make a different number of application denoted by a_1^* and a_0^*, they will also have different reservation wages.

[21] When estimating the parameters of the model, we use a slightly different specification where an employed worker expects U_0 rather than $\bar{U}$ when being fired. This reflects the temporary nature of the experiment. In the simulations the program is assumed to be offered permanently, and thus workers expect $\bar{U}$ when fired. The same distinction was made in the baseline model.

Bertrand competition for workers implies that if a worker has only one offer, all surplus of the match goes to the firm.

$$E_{0,l} = U_0$$
$$E_{1,l} = U_1$$

and therefore

$$w_{i,l} = (r + \delta)U_i - \delta\bar{U}$$

The value functions for a filled job (equation (11)) now become,

$$\begin{aligned} rJ(w_{i,l}) &= y - w_{i,l} - \delta(J(w_{i,l}) - V) \\ rJ(y) &= 0 \\ J &= (1-\tau)p_{0,l}(a_0)J(w_{0,l}) + \tau p_{1,l}(a_1)J(w_{1,l}) + \bar{p}_h J(y) \end{aligned}$$

This last equation gives the expected value of a filled vacancy, where $\bar{p}_h$ describes the average probability in the population ($\bar{p}_l = (1-\tau)p_{0,l}(a_0^*) + \tau p_{1,l}(a_1^*)$).

Figure 8: Simulation results model with Cobb-Douglas matching function

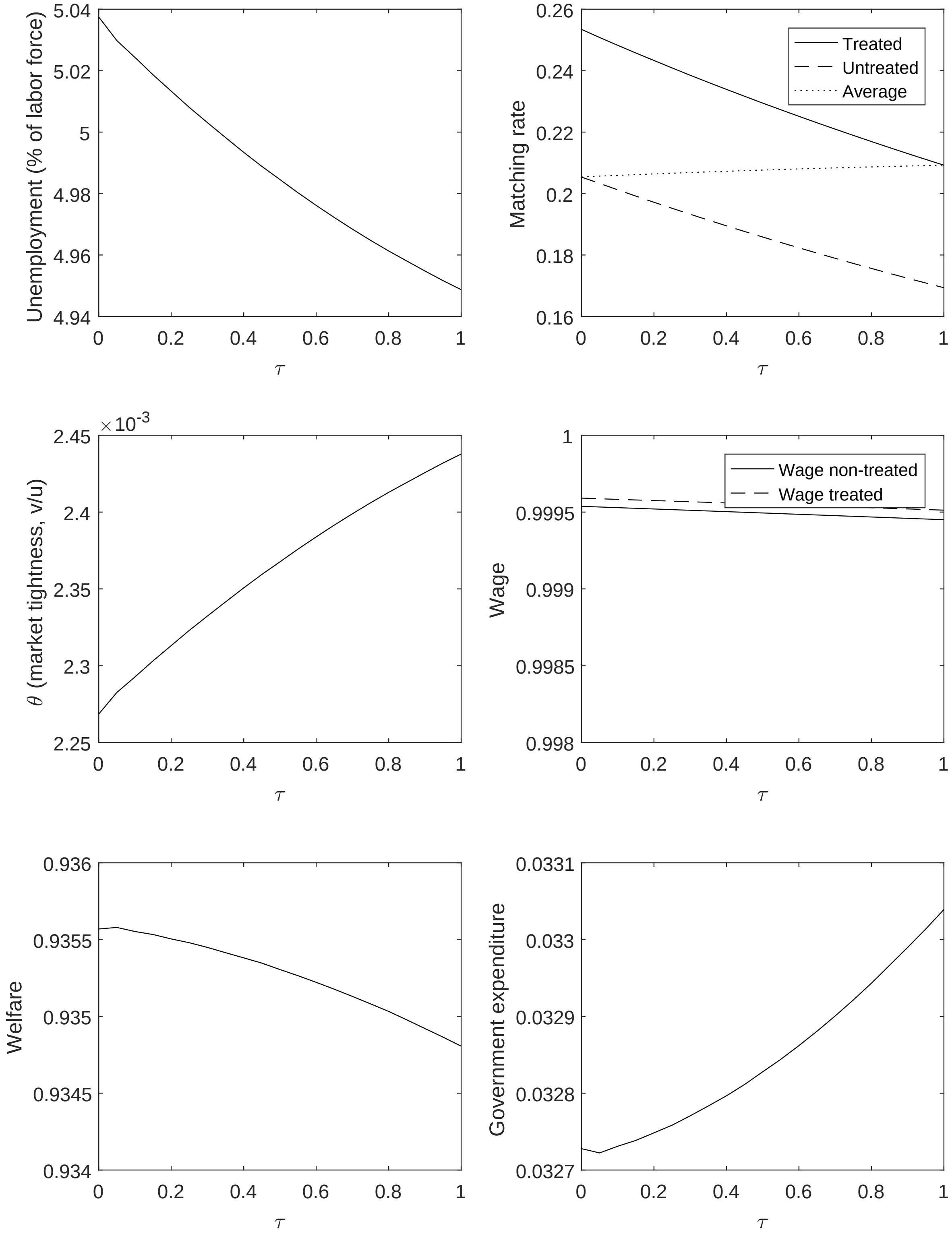

Figure 9: Simulation results due to changes in τ with Bertrand wages