

# VU Research Portal

## Estimating equilibrium effects of job search assistance

Muller, Paul; van der Klaauw, Bas; Gautier, P.A.; Rosholm, Michael; Svarer, Michael

### **published in**

Journal of Labor Economics  
2018

### **DOI (link to publisher)**

[10.1086/697513](https://doi.org/10.1086/697513)

### **document version**

Publisher's PDF, also known as Version of record

### **document license**

Article 25fa Dutch Copyright Act

[Link to publication in VU Research Portal](#)

### **citation for published version (APA)**

Muller, P., van der Klaauw, B., Gautier, P. A., Rosholm, M., & Svarer, M. (2018). Estimating equilibrium effects of job search assistance. *Journal of Labor Economics*, 36(4), 1073-1125. <https://doi.org/10.1086/697513>

### **General rights**

Copyright and moral rights for the publications made accessible in the public portal are retained by the authors and/or other copyright owners and it is a condition of accessing publications that users recognise and abide by the legal requirements associated with these rights.

- Users may download and print one copy of any publication from the public portal for the purpose of private study or research.
- You may not further distribute the material or use it for any profit-making activity or commercial gain
- You may freely distribute the URL identifying the publication in the public portal ?

### **Take down policy**

If you believe that this document breaches copyright please contact us providing details, and we will remove access to the work immediately and investigate your claim.

### **E-mail address:**

[vuresearchportal.ub@vu.nl](mailto:vuresearchportal.ub@vu.nl)

# Estimating Equilibrium Effects of Job Search Assistance

Pieter Gautier, *Vrije Universiteit Amsterdam and Tinbergen Institute*

Paul Muller, *Gothenburg University*

Bas van der Klaauw, *Vrije Universiteit Amsterdam  
and Tinbergen Institute*

Michael Rosholm, *Aarhus University*

Michael Svarer, *Aarhus University*

Identifying policy-relevant treatment effects from randomized experiments requires the absence of spillovers between participants and nonparticipants (SUTVA) or variation in observed treatment levels. We find that SUTVA is violated for a Danish activation program for unemployed workers. Using a difference-in-differences model, we show that nonparticipants in the experiment regions find jobs more slowly after the introduction of the program than workers

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, Institut national de la statistique et des études économiques (INSEE) Crest, Royal Holloway University, University College London, Vrije Universiteit Amsterdam, Georgetown University, the University of Cambridge, the University of Oxford, the University of Wisconsin–Madison, and the Annual Search and Matching conference in Cyprus. Bas van der Klaauw acknowledges financial support from a Vici grant from the Dutch science foundation (NWO). Contact the corresponding author, Paul Muller, at paul.muller@gu.se. Information concerning access to the data used in this paper is available as supplementary material online.

[*Journal of Labor Economics*, 2018, vol. 36, no. 4]

© 2018 by The University of Chicago. All rights reserved. 0734-306X/2018/3604-0006\$10.00

Submitted July 1, 2016; Accepted July 5, 2017; Electronically published July 31, 2018

in other regions. We estimate an equilibrium search model to identify the policy-relevant treatment effect. A large-scale rollout of the program is shown to decrease welfare, while a standard partial microeconomic cost-benefit analysis concludes the opposite.

## I. 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 a worker enters unemployment, and the goal is to provide intensive guidance toward 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, Rosholm, and Svarer (2013) show that participants in the program find work faster than nonparticipants and that 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 those of unemployed workers in comparison counties (using the same administrative data). Since the experiment counties were not selected randomly, we use preexperimental data from all counties to control, in a difference-in-differences setting, for existing differences between counties. This allows us to estimate treatment effects on the nontreated workers. We find that the job-finding rate is lower for nonparticipants during the experiment than for unemployed in the comparison counties.

We focus on how the experiment affects vacancy supply, wages, and working hours. We find some marginally statistically significant evidence that the supply of vacancies increases 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 vacancy supply responses and positive or negative congestion effects. 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 rollout of the activation program and compute the effects on labor market behavior and outcomes. Our main finding is that a large-scale rollout decreases welfare. 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 rollout. The main findings are robust to different specifications of wage mechanism and matching function.

A large number of papers stress 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, Ashmore, and Deschênes (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 those of nonparticipants. This is the case not only in experimental evaluations but also in nonexperimental evaluations (after correction for selection). It implies that equilibrium effects are assumed to be absent (e.g., DiNardo and Lee 2011).

In the case of active labor market programs, equilibrium effects are likely to be important (e.g., Abbring and Heckman 2007). 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. A recent empirical literature shows the presence of spillover effects of various labor market programs (e.g., Blundell et al. 2004; Lise, Seitz, and Smith 2004; Albrecht, van den Berg, and Vroman 2009; Crépon et al. 2013; Ferracci, Jolivet, and van den Berg 2014; Lalive, Landais, and Zweimüller 2015).

Our paper contributes not only to the empirical treatment-evaluation literature but also to the macro (search) literature. We demonstrate how data from a randomized experiment, combined with nonexperimental 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, because of 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 suggest that in settings where search intensity matters, the urn-ball matching function with multiple applications performs better than a Cobb-Douglas matching function. Our approach of combining data from a randomized experiment with a structural model relates to Lise, Seitz, and Smith (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 (Mexico's Programa de Educación, Salud, y Alimentación) by Attanasio, Meghir, and Santiago (2012) and Todd and Wolpin (2006).

The remainder of the paper is organized as follows. Section II discusses the background of the Danish randomized experiment as well as literature on treatment externalities. Section III provides a description of the data, and Section IV presents the empirical analysis and the estimation results. In Section V, we develop an equilibrium search model that incorporates the activation program. Estimation of the model and policy simulations are presented in Section VI. Section VII concludes.

## II. Background

### A. 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 toward 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 5–6 weeks of unemployment, workers have to participate in a 2-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 4 months the worker has not found work, a new program starts for at least 3 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 2,122 DKK (Danish kroner; about €285, US\$355), on average, per entitled worker.<sup>1</sup>

To evaluate the effectiveness of the activation policy, a randomized experiment was conducted in two Danish counties, Storstrøm and South Jutland (see fig. 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 fifteenth of the month participated in the activation program, while individuals born on the sixteenth to the thirty-first did not receive this treatment. The control group received the usual assistance, consisting of meetings with

<sup>1</sup> These costs are in addition to the costs of the usual assistance offered to unemployed workers in Denmark.

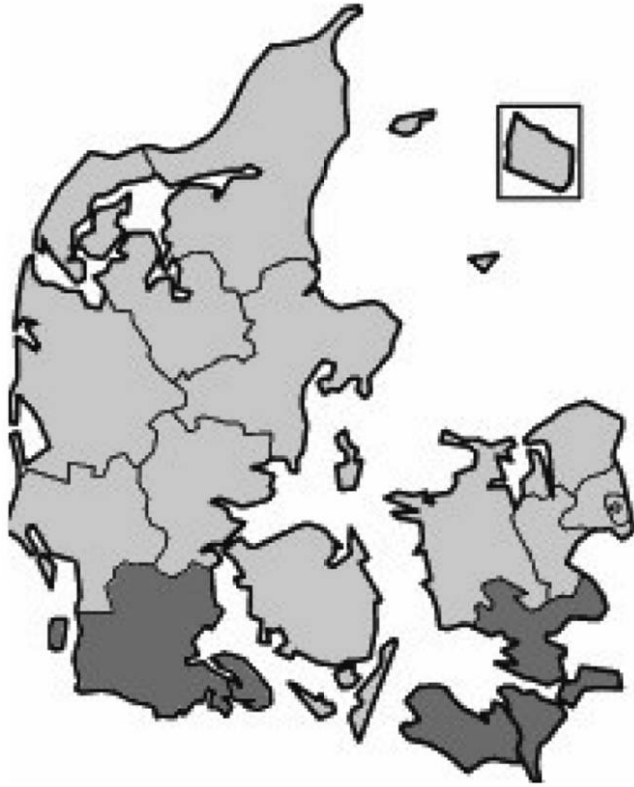


FIG. 1.—Location of the experiment counties: South Jutland (*left*) and Storstrøm (*right*).

a caseworker every 3 months and more intensive assistance after 1 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 (€1,987) 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 reemployment 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, Rosholm, and Svarer (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 toward 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.

### B. 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.

Consider a population in which a fraction  $\tau$  participates in a treatment. In this setting, the average effect of participating in the treatment is defined as

$$\Delta(\tau) = E[Y_1^* | \tau] - E[Y_0^* | \tau],$$

where  $Y_0^*$  and  $Y_1^*$  denote the potential outcomes without and with treatment, respectively. A common assumption in the treatment-evaluation literature is that an individual's behavior and outcomes do not directly affect the behavior and outcomes 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). We write this assumption as



$$(Y_1^*, Y_0^*) \perp \tau.$$

If SUTVA holds, then the treatment effect is no longer dependent on the treatment status of other individuals. Therefore, the average treatment effect does not depend on the treatment participation rate  $\tau$ , and we can write  $\Delta = \Delta(\tau)$  for all values of  $\tau$ . In that case, 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.

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 rollout of a program (e.g., Heckman and Vytlačil 2005; DiNardo and Lee 2011). Then the fraction of the population in the same area receiving treatment is relevant. 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 low geographical mobility in Denmark. When the ultimate goal is the large-scale rollout of a treatment, the policy-relevant treatment effect is

$$\Delta^* = E[Y_1^* | \tau = 1] - E[Y_0^* | \tau = 0]. \quad (1)$$

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  $\tau$ .

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 affect not only the nonparticipants but also the other participants in the program. In Section V, 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



$E[Y_0^* | \tau = \hat{\tau}] - E[Y_0^* | \tau = 0]$ , that is, the treatment effect on the nontreated workers. To deal with structural differences between counties, we use outcomes in all counties before the experiment and make a common-trend assumption. In Section IV, we provide more details about the empirical analysis. Still, the empirical approach identifies only treatment effects and equilibrium effects at a treatment intensity  $\hat{\tau}$ , while for a large-scale rollout of the program one should focus on  $\tau = 1$ . Therefore, in Section V 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 (1).

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 workers. 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, Costa Dias, and Meghir (2003) show that treatment effects can change sign when equilibrium effects are taken into account. Also, Ferracci, Jolivet, and van den Berg (2014) 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 nonexperimental nature of their data, in both steps they rely on a conditional independence assumption to identify treatment effects. Lise, Seitz, and Smith (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 in France. In addition to randomized program participation, the share of participants across regions is also randomized. The target population consists of highly educated unemployed workers below age 30 who had been unemployed for at least 6 months. The program affects treated workers positively, while a small negative (and statistically insignificant) effect on nontreated 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.

Treatment externalities have also received interest outside the evaluation of active labor market programs. Heckman, Lochner, and Taber (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 deworming drugs on schools in Kenya. Duflo, Dupas, and Kremer (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.

### III. 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 involves 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.<sup>2</sup> We remove 1,398 individuals from the sample for which the county of residence is inconsistent. Of the remaining 40,403 observations, 3,751 individuals live in either Storstrøm or South Jutland and participate in the experiment. Of the participants in the experiment, 1,814 individuals are assigned to the treatment groups and 1,937 to the control group.

The data also include individuals who started collecting benefits 1 year before the experiment period (between November 2004 and February 2005) and 2 years before the experimental period (between November 2003 and February 2004). We refer to these periods as the preexperiment periods. In the empirical analysis, we use one preexperiment period, containing those who entered 1 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 2 years after becoming unemployed. All individuals are entitled to at least 4 years of collecting benefits. Combining the data on unemployment durations with data on earnings shows that almost all observed exits in the first 2 years are to employment.

<sup>2</sup> We exclude Copenhagen, because it differs from the rest of Denmark in terms of labor market characteristics.

### A. Preexperiment 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 preexperiment years. Figure 2 shows the average unemployment rate in the experiment counties and the comparison counties in the 10 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, although the decrease in unemployment in 2004 is slightly larger in the experiment counties.<sup>3</sup>

Next, we compare outflow from unemployment in the preexperiment periods. Figure 3 shows the Kaplan-Meier estimates of the survivor functions for individuals who started collecting benefits in the preexperiment periods (November 2003–February 2004 and November 2004–February 2005). Again, we distinguish between the experiment counties and comparison counties. To correct for differences in observed characteristics, each survivor is weighted on the basis of distribution of gender, unemployment history, and ethnicity in the comparison counties in the 2005–6 period.

Figure 3A shows that in the 2003–4 period, the experiment counties are 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 1 year, in both groups, 78% of the unemployed leave unemployment. A log-rank test cannot reject the null hypothesis that the distributions of unemployment durations in the experiment and comparison counties are the same (with a  $p$ -value of .25). The survival functions of the experiment counties and comparison counties are also very similar in the 2004–5 period (fig. 3B). For both groups, the median unemployment duration is 15 weeks. Again, a log-rank test cannot reject that the unemployment distributions of the two groups are the same in 2004–5 (with a  $p$ -value of .24). The strong similarity of the survivor functions in both preexperiment periods suggests that, at least in terms of outflow from unemployment, the comparison counties and the experiment counties face similar trends.

### B. Experimental Period

Next, we consider individuals who entered unemployment in the experiment period (November 2005–February 2006). Figure 4 shows the Kaplan-

<sup>3</sup> In the empirical analysis, we use one preexperimental 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.

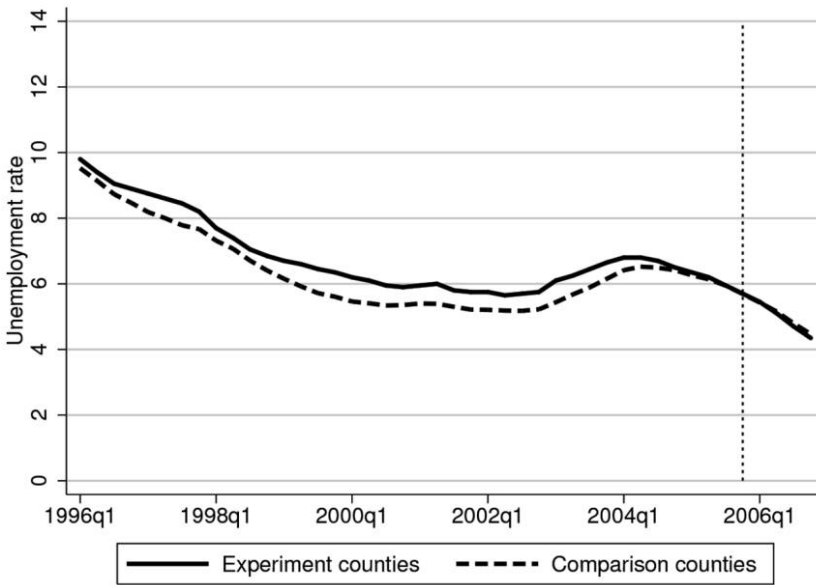


FIG. 2.—Preexperiment unemployment rate in experiment and comparison counties. Source: Statistics Denmark.

Meier estimates for the treatment and control groups 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 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 log-rank test rejects that the distributions of unemployment durations are the same in the treatment and control groups ( $p$ -value  $< .01$ ). 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 .47 (but this test does not correct for county fixed effects).

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 preexperimental year for individuals in each of 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 counties and the comparison

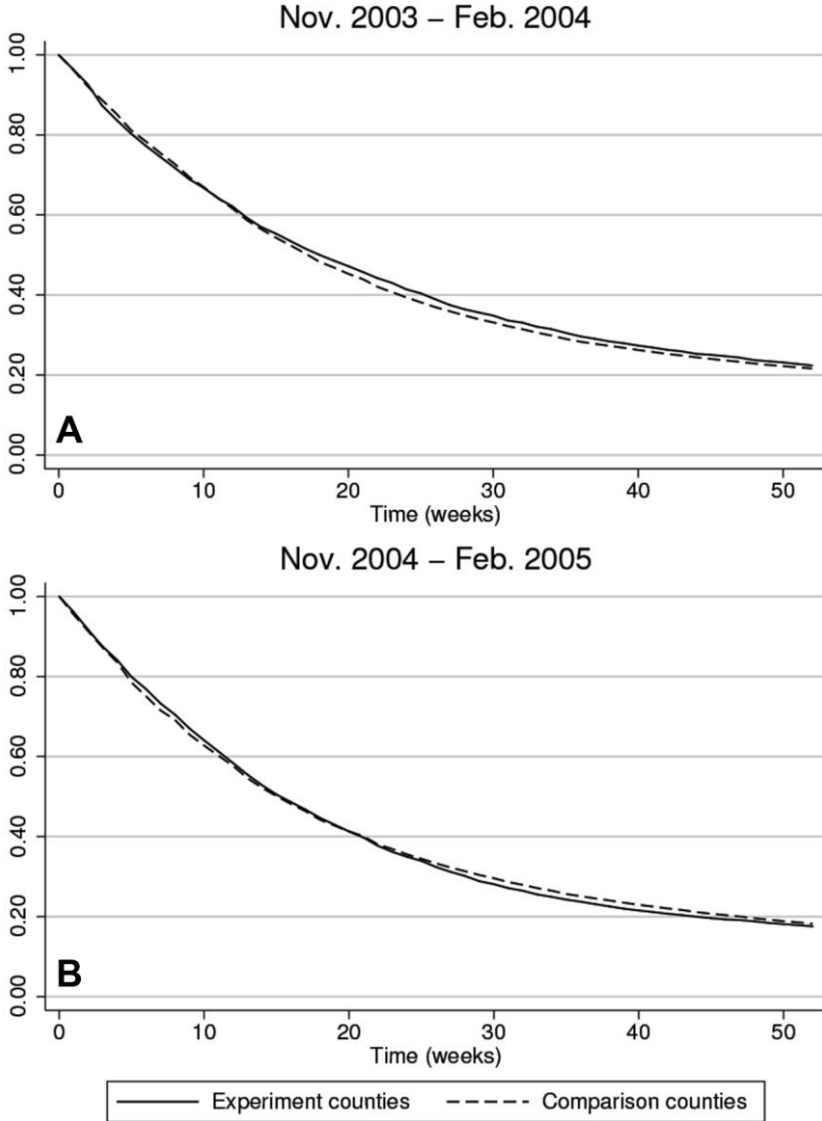


FIG. 3.—Survivor functions for the experimental counties and the comparison counties in the years before the experiment.

counties. The weekly earnings are higher in the experiment period than in the preexperiment period and higher in the comparison counties than in the experiment counties. Participants in the activation program work slightly more hours and have somewhat higher earnings than individuals in the control group.

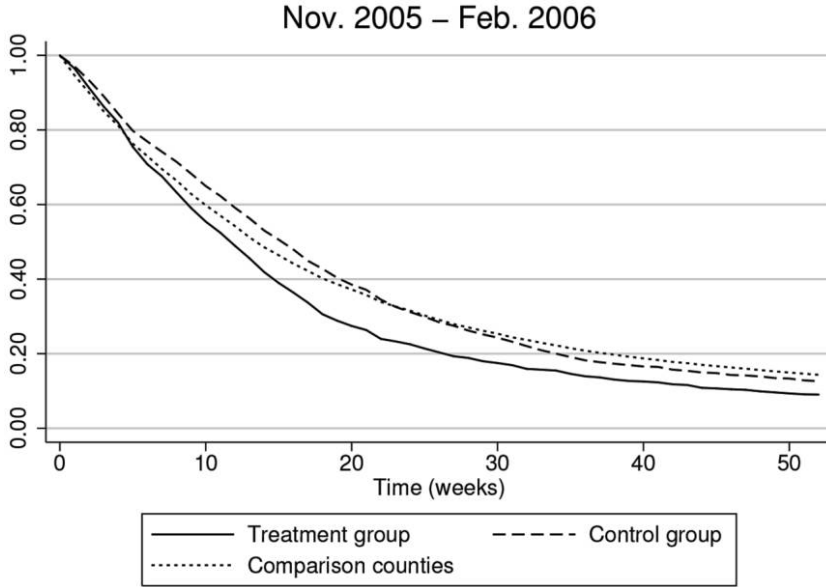


FIG. 4.—Survivor functions for the comparison counties, the control group, and the treatment group during the experiment.

The data include a number of individual characteristics. Age and immigrant status distributions are roughly similar across groups. In the experiment period, there is a higher fraction of males among those becoming unemployed in the experiment counties than in the comparison counties. In the comparison counties, the unemployed workers have a slightly longer history of benefits receipt in the experiment period than in the preexperiment period. Earnings and hours worked before the unemployment spell are roughly similar across groups, and there are also only minor differences in education categories.

The lower part of the table shows some county-level statistics. In both the experiment counties and the comparison counties, the local unemployment rate declines and gross domestic product (GDP) per capita increases between the preexperiment and experiment periods. The labor force participation rate remains virtually unchanged. These changes are suggestive of similar calendar time trends in the experiment and comparison counties. However, in both time periods the labor market conditions are, on average, more favorable in the comparison counties than in the experiment counties, that is, a 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

**Table 1**  
**Summary Statistics**

	Experiment Counties			Comparison Counties	
	2004–5	Treatment	Control	2004–5	2005–6
Hours worked per week	35.4	36.6	34.9	35.0	36.1
Earnings (DKK per week)	5,950	6,271	6,160	6,256	6,586
Male (%)	54.6	60.8	59.2	53.0	52.4
Age (years)	42.0	42.4	42.3	41.3	41.2
Native (%)	94.8	93.2	94.4	93.7	93.0
Western immigrant (%)	3.2	4.0	3.4	2.8	3.2
Nonwestern immigrant (%)	2.0	2.8	2.2	3.5	3.8
Benefits previous year (weeks)	10.5	9.8	9.0	10.2	11.1
Benefits past 2 years (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 (DKK per week)	4,903	5,191	5,436	4,993	5,113
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 education	9.8	5.9	6.3	11.6	8.7
Bachelor's	.5	.8	.8	.8	2.1
Master's or more	1.5	1.9	1.3	4.0	3.1
Observations	5,321	1,496	1,572	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 per capita (000s DKK)	197.5	201.3		219.8	225.1

NOTE.—Individual characteristics are based on authors' own calculations; aggregate statistics are from Statistics Denmark.

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

#### IV. 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.



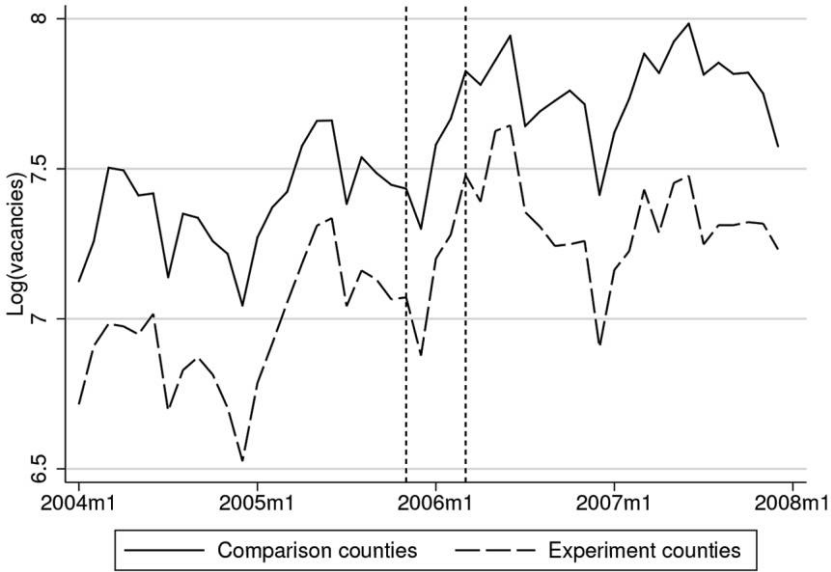


FIG. 5.—Logarithm of the stock of vacancies per month (experiment period between vertical lines); m1 = January.

### A. Unemployment Duration

The aim of the activation program is to stimulate those in the treatment group to find work faster. In the presence of spillovers, a simple comparison of outcomes between treatment and control groups 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 preexperiment period to control for structural differences between counties.

We consider the effect of the program on the probability of exiting unemployment within a fixed time period.<sup>4</sup> Let  $E_i$  be an indicator for exiting unemployment within this period. We consider exit within 3 months, 1 year, and 2 years. So in the first case, the variable  $E_i$  takes the value 1 if individual  $i$  is observed to leave unemployment within 3 months and 0 otherwise. To estimate the effect of the activation program on the treatment and control groups, we estimate the following linear probability model:

$$E_i = \alpha_r + x_i\beta + \delta d_i + \gamma c_i + \eta_{p_i} + U_i. \tag{2}$$

<sup>4</sup> In app. A, two other model specifications are discussed. These give similar estimates for the effects of the activation program on the treatment and control groups.

This is a difference-in-differences model. Differences in the probabilities of exiting unemployment between counties are controlled for by county fixed effects  $\alpha_{r_i}$ , where  $r_i$  describes the county in which individual  $i$  lives. The common time trend is described by  $\eta_{p_i}$ , where  $p_i$  is either the experiment period or the preexperiment period. The dummy variable  $d_i$  is equal to 1 if individual  $i$  is in the treatment group, and  $c_i$  is equal to 1 if individual  $i$  is in the control group.<sup>5</sup> 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  $\delta$  and  $\gamma$ , which describe the effect of the activation program on those in the treatment group and those in the control group, respectively. The parameter  $\gamma$  thus describes possible spillover effects. The key identifying assumption for the spillover 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 treatment and control groups 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 effect on the treatment group becomes smaller for longer unemployment durations but is always positive and highly statistically 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, Rosholm, and Svarer (2013) find that the effect of the activation program is largest early in the unemployment spell.

After 3 months, individuals in the treatment group are more than 9% ( $0.059 + 0.033$ ) more likely to have found work than individuals in the control group, but over one-third of this difference is due to reduced job finding of the control group. The effect of the activation program on those randomly assigned to the control group during the experiment is substantial and significant after 3 months. During this period, the activation program is most intense, containing a job search assistance program and frequent meetings with caseworkers. Early in the unemployment spell, relatively many participants in the activation program leave unemployment, which reduces treatment externalities for the control group later in the unemploy-

<sup>5</sup> The previous program evaluations (Graversen and van Ours 2008; Rosholm 2008; Vikström, Rosholm, and Svarer 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**

	3 Months (1)	1 Year (2)	2 Years (3)
Treatment group	.059*** (.007)	.039*** (.004)	.010** (.005)
Control group	-.033** (.014)	.013*** (.003)	-.006** (.003)
Mean dependent variable <sup>a</sup>	.500	.901	.969
Individual characteristics	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Observations	77,057	77,057	77,057

NOTE.—Standard errors (clustered by county interacted with time period) are in parentheses. Individual characteristics include gender, age dummies, education level, log previous earnings, immigrant status, labor market history, and quarter of entering unemployment.

<sup>a</sup> The aggregate outflow probability in the experiment counties during the experiment.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

ment spell. Indeed, we find that after 1 year the effect on the control group is smaller in magnitude and has changed sign. After 2 years, the negative effect on the control group is more than half the size of the effect on the treatment group. Both effects are statistically significant but small. Only slightly more than 3% of the individuals in the treatment group are still unemployed after 2 years.

### B. 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 (2):

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

where the outcome  $Y_i$  is either the logarithm of weekly earnings of individual  $i$  or hours worked per week.<sup>6</sup> In the set of covariates we include weekly earnings and hours worked before becoming unemployed. The results are presented in columns 1 and 2, respectively, of table 3. These show no effects of the activation program on both the treatment group and the control group.

We conclude that even though the activation program significantly reduces the duration until job finding for the treatment group and increases

<sup>6</sup> Both earnings and hours are measured as the monthly average over the remainder of the calendar year in which employment was found.

**Table 3**  
**Estimated Effects of the Activation Program on Post-unemployment Wages and Hours Worked**

	Log Weekly Earnings (1)	Weekly Hours Worked (2)
Treatment group	.01 (.02)	.89 (1.24)
Control group	.01 (.02)	.14 (1.22)
Individual characteristics	Yes	Yes
County fixed effects	Yes	Yes
Observations	68,979	68,980

NOTE.—Standard errors are in parentheses. 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 (col. 2), they also include previous hours worked.

the duration for the control group, the program has no impact on post-unemployment earnings and hours worked.

### C. 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 the search effort of participants affects the exit rate to work of other unemployed job seekers in the same local labor market. In addition, 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 nonparticipants. In this subsection, we investigate to what extent the stock of vacancies is affected by the experiment.<sup>7</sup>

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  $\alpha_t$  and an indicator for the experiment  $D_{rt}$  and include county fixed effects  $\theta_r$ ,

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

This is, again, a difference-in-differences model. The parameter of interest is  $\delta$ , 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  $\alpha_t$ , in the changes in the stock of vacancies. Furthermore, the experiment should affect only the local labor market in the experiment counties. If there are spillovers between counties,  $\delta$  would underestimate the effect of the experiment on vacancy creation. Finally, since the unit of time is a month,

<sup>7</sup> 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.

there is likely autocorrelation in the error terms  $U_{it}$ . Because the total number of counties equals 14, we report cluster-robust standard errors to account for the autocorrelation (see Bertrand, Duflo, and Mullainathan 2004 for an extensive discussion).

Table 4 reports the estimation results. Column 1 shows that during the 4 months of the experiment (November 2005–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 a worker enters unemployment and that workers start the 2-week job search assistance program 5–6 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 starts to increase in the experiment counties, compared to other counties. This effect peaks in May–June, 3–4 months after the end of program assignment, and decreases afterward.

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. In column 4, we find very similar results, using normalized log vacancies as the outcome variable.<sup>8</sup> 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 app. B).

## V. 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 3 months (although 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 rollout 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

<sup>8</sup> We weight the regression by using the standard deviation of log vacancies at the county level (in the preexperiment periods).

**Table 4**  
**Estimated Effect of the Experiment on Logarithm of Vacancies**

	Log Vacancies (1)	Log Vacancies (2)	Log Vacancies (3)	Normalized Log Vacancies (4)
Experiment	.047 (.050)			
Experiment Nov–Dec 2005		.057 (.084)	.007 (.055)	.050 (.092)
Experiment Jan–Feb 2006		.067* (.032)	.016 (.032)	.051 (.033)
Experiment Mar–Apr 2006		.081** (.033)	.031 (.041)	.046 (.036)
Experiment May–Jun 2006		.182*** (.046)	.132*** (.034)	.128*** (.033)
Experiment Jul–Aug 2006		.114*** (.027)	.064* (.031)	.095*** (.024)
Experiment Sep–Oct 2006		–.049 (.046)	–.099 (.068)	–.098 (.066)
County fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Observation period	Jan 2004–Dec 2007	Jan 2004–Dec 2007	Jan 2005–Dec 2006	Jan 2004–Dec 2007

NOTE.—Standard errors (clustered by country) are in parentheses. Column 4 presents results from an estimation weighted with the country variance of log vacancies in the pre-experiment periods.

\* Significant at the 10% level.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

1982; and Pissarides 2000) to analyze 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.

#### A. 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, Gautier, and Vroman 2006 for a related matching function). We try to keep our matching model as simple as possible, but with the restriction that it can describe the estimated treatment effects well. Workers are ex ante homogeneous, they are risk neutral, and all have the same productivity. They differ only in whether or not they participate in the activation program. Program participation reduces the costs of sending an application but costs time. It also affects the effectiveness of a given application. Recall that the goal of the activation program is to stimulate job search effort. The regular meetings do 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.<sup>9</sup> 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  $b$ . 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 in the next period. Making job applications is costly, and we assume these costs to be quadratic in the number of applications, that is,  $\gamma_0 a^2$ . Convex search costs are conve-

<sup>9</sup> 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.



nient and can be motivated by the idea that workers consider the “low-hanging fruit” first, while 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 Section V.B, 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 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:

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

which can be rewritten as

$$rU_0 = \max_{a \geq 0} b + b - \gamma_0 a^2 + m_0(a; \bar{a}, \theta)(E(w) - U_0). \tag{3}$$

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

$$a_0^* = \frac{E(w) - U_0}{2\gamma_0} \frac{\partial m_0(a; \bar{a}, \theta)}{\partial a} \Big|_{a = a_0^*}. \tag{4}$$

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 nonmarket time at 0 for the participants and at  $b$  for the nonparticipants (where, at this moment, we do not rule out that  $b < 0$ ). 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

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

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

$$a_1^* = \frac{E(w) - U_1}{2\gamma_1} \frac{\partial m_1(a; \bar{a}, \theta)}{\partial a} \Big|_{a=a_1^*} \tag{5}$$

Furthermore,  $\tau$  is 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  $\delta$ , 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

$$rE(w) = w - \delta(E(w) - \bar{U}). \tag{6}$$

When estimating the model parameters, we take account of the temporary nature of the experiment and replace  $\bar{U}$  in equation (6) with  $U_0$ .<sup>10</sup> However, for our policy simulations, which consider a complete and permanent role out of the program, we use  $\bar{U} = \tau U_1 + (1 - \tau) U_0$ , where  $\tau$  is the fraction of workers who start participating in the activation program when they become unemployed. This implies that employed workers realize that when they are fired, there is a positive probability of receiving job search assistance.

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 application behavior of participants and nonparticipants, and labor market tightness  $\theta$ . The probability of filling a vacancy (given that the matching function exhibits constant returns to scale) is  $m(a_0^*, a_1^*; \tau, \theta) / \theta$ , which we derive below. The value of a vacancy  $V$  follows from

$$rV = -c_v + \frac{m(a_0^*, a_1^*; \tau, \theta)}{\theta} (J(w) - V), \tag{7}$$

where  $J(w)$  is the value of a filled vacancy. Each period that a job exists, the firm receives the value of output  $y$  minus wage cost  $w$ . With probability  $\delta$ ,

<sup>10</sup> This assumes that employed workers do not expect to participate in the activation program. Given that separations are exogenous, the more important implication is that unemployed workers know that the next time they enter unemployment, the experiment has ended.

the job is destroyed and switches from filled to vacant. The value of a filled vacancy  $J(w)$  is therefore given by

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

### B. 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, participating and nonparticipating workers have different outside options, and consequently we must allow their equilibrium wages to differ. Let  $\beta$  denote the bargaining power of 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:

$$(1 - \beta)(w_i^* + \delta \bar{U} - (r + \delta)U_i) = \beta(y - w_i^*). \tag{9}$$

In appendix C, we solve the model for the wage mechanism of Albrecht, Gautier, and Vroman (2006), where workers with multiple offers receive the full surplus because of Bertrand competition between the firms that made them an offer. This gives similar results in terms of labor market flows, vacancy creation, and welfare effects of the activation program. The outcomes are discussed in more detail in Section VI.C.2.

Finally, we specify the matching rates  $m_0(a; \bar{a}, \theta)$  and  $m_1(a; \bar{a}, \theta)$  for non-participating and participating unemployed workers, respectively, and  $m(a_0^*, a_1^*; \tau, \theta)/\theta$  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, Gautier, and Vroman (2006) to incorporate these factors.<sup>11</sup> There are two coordination frictions affecting job finding: (1) workers do not know where other workers apply, and (2) firms do not know which candidates are considered by other firms.<sup>12</sup> 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.

<sup>11</sup> In a sensitivity analysis, we consider a Cobb-Douglas matching function instead. See Sec. VI.C.1 for details.

<sup>12</sup> The second coordination friction is absent in a usual Cobb-Douglas matching function.

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 activation program, an application results in a job offer with probability  $1/(1 + j)$ , where  $j$  is the number of competitors for that job (which equals the number of other applications for that vacancy). This implies that the probability that an application for a program nonparticipant results in a job offer equals

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

The activation program can change the application behavior of participants. The program may affect the types of vacancies participants apply for, which can change their success rate. We model this by introducing a parameter  $\kappa$ , which describes the fraction of vacancies for which a participant has a positive productivity. After screening its candidate, the firm learns whether the worker is productive or not. The probability that an application of a program participant results in a job offer therefore equals

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

When  $\kappa < 1$  and  $\gamma_1 < \gamma_0$ , the program induces participants to send out more applications, but the success rate of an individual application is relatively low.

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

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

Once we substitute for  $a$  the optimal number of applications  $a_1^*$  and  $a_0^*$ , we obtain the matching rates for, respectively, the participants and the non-participants in the activation program.

The aggregate matching function is (with a little abuse of notation) simply given by  $m(\bar{a}_0^*, \bar{a}_1^*; \tau, \theta) = \tau m_1(\bar{a}_1^*; \bar{a}, \theta) + (1 - \tau)m_0(\bar{a}_0^*; \bar{a}, \theta)$ , and for reasonable values of  $\theta$  it is first increasing in the number of applications per worker and then decreasing.<sup>13</sup> Having more applications per worker reduces

<sup>13</sup> Only for very low values of  $\theta$  is the matching function monotonically decreasing in the number of applications.

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, while only one firm can hire him).

### C. Equilibrium and Welfare

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

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

The equilibrium unemployment rate is therefore

$$u^* = \frac{\delta}{\delta + \tau m_1(a_1^*; \bar{a}, \theta) + (1 - \tau)m_0(a_0^*; \bar{a}, \theta)}. \tag{10}$$

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

$$J(w) = \frac{y - w}{r + \delta}.$$

Participants and nonparticipants receive different wages, and the expected wage (for new vacancies) equals  $\bar{w}^* = (1 - \tau)w_0^* + \tau w_1^*$ . Since the flow value of a filled vacancy is a linear function of the wage, we can substitute the expected wage in this flow value and next substitute the expected flow value in the Bellman equation for vacancies (eq. [7]). This gives

$$\frac{m(a_0^*, a_1^*; \tau, \theta^*)}{\theta^*} = \frac{(r + \delta)c_v}{y - \bar{w}^*}. \tag{11}$$

The left-hand side is decreasing in  $\theta$  and goes to infinity when  $\theta$  approaches 0. Because wages are increasing in  $\theta$ , the right-hand side is increasing in  $\theta$ . Therefore, there is a unique  $\theta^*$  that satisfies the equilibrium condition in equation (11). We can now define the equilibrium as the tuple  $\{a_0^*, a_1^*, w_0^*, w_1^*, u^*, \theta^*\}$  that satisfies equations (4), (5), (9), (10), and (11).

After solving the model and deriving conditions for equilibrium, we use the model for policy simulations. The decision parameter for the policy maker is the intensity of the activation program ( $\tau$ ).<sup>14</sup> 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 pro-

<sup>14</sup> Most policy makers make a program available either to all eligible unemployed workers or to none. This implies that  $\tau$  is either 1 or 0. In our policy simulations we also consider values between 0 and 1. This provides insights into the spillover effects of the program. Cases also exist where programs have a limited budget or capacity, such that not all eligible unemployed workers can enroll.

gram. Implicitly, we assume here that it is paid from a nondistortionary tax. Introducing distortionary taxes makes the program less desirable. It reduces net earnings and thereby reduces 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  $(b - \gamma_0 a_0^{*2})$  and  $-\gamma_1 a_1^{*2}$  for nonparticipants and participants, respectively.<sup>15</sup> We define welfare as net (of all pecuniary and nonpecuniary costs) output per worker,

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

The welfare function does not include unemployment insurance benefits because those must be financed and are thus a matter of redistribution. After estimating the model parameters, we can investigate whether the experimental treatment intensity increases welfare, that is, whether  $W(0.3) > W(0)$ , and whether a large-scale rollout of the activation program increases welfare,  $W(1) > W(0)$ . The latter program effect is based on the policy-relevant treatment effect defined in equation (1). Furthermore, we can compute the welfare-maximizing value for  $\tau$ .

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 a total reduction in benefit payments. The total amount of benefit payment equals  $ub$ . This implies that the naive policy maker chooses  $\tau$  such that it minimizes the costs of the unemployment insurance program,

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

Finally, it is interesting to compare the results of these policy parameters to results from a typical microeconomic evaluation. As discussed in Section II.B, most microeconomic 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, Ham, and LaLonde 2002; van den Berg and van der Klaauw 2006),

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

<sup>15</sup> See Greenberg and Robins (2008), who show the importance of taking time costs into account when evaluating the Canadian self-sufficiency program.

where  $(1/m(a_1^*; \bar{a}, \theta)) - (1/(a_0^*; \bar{a}, \theta))$  is the difference in expected unemployment duration between unemployed workers participating and those not participating in the activation program. A positive value implies positive returns to the program. This evaluation ignores not only equilibrium effects but also, for example, foregone leisure of the participants; see van den Berg and van der Klaauw (2006).

## VI. 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 IV as our auxiliary model (see Gourieroux, Monfort, and Renault 1993 and Smith 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.

### A. 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 allows us to ignore measurement error 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 1 month. Next, we fix the treatment intensity of the activation program during the experiment to 0.3 (see the discussion in Sec. II.A). We denote the treatment intensity during the experiment by  $\tau^e$ . In Section VI.C, we investigate the sensitivity of the results by estimating the model for alternative levels of  $\tau^e$ . As mentioned above, 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 realize that the program will not continue forever, and therefore we take into account that workers expect that when they lose their job, they will 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, Seitz, and Smith (2004) and Fougère, Pradel, and



Roger (2009) and estimated by Frijters and van der Klaauw (2006). Productivity  $y$  is normalized to 1. The top panel of table 6 summarizes the values for the model parameters that we fix a priori.

After the discount rate, the treatment intensity, and productivity are fixed, there are eight 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 nine moment restrictions, which are presented in table 5. The model should capture (1, 2) the unemployment and vacancy rates in the experiment counties, (3, 4) the estimated program effect on the participants and the nonparticipants, (5) the estimated increase in vacancies due to the experiment, (6) the average matching rate in the experiment counties, (7, 8) the small (insignificant) wage increases for participants and nonparticipants, and (9) the fact that unemployment benefits are approximately 65% of the wage level. Define  $\xi = (\gamma_0, \gamma_1, \delta, \kappa, b, \beta, c, h)$  as the vector of parameters to be estimated. For given values of  $\xi$ , the model can be solved and the set of model predictions can be computed. To obtain estimates for  $\xi$ , we minimize the sum of squared differences between the data moments and the corresponding model predictions over  $\xi$ , where each squared difference is given an appropriate weight based on the variance of the (estimated) data moment (see the third column in table 5). 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 top panel of table 6 (col. 2, baseline model). Standard errors are computed with the delta method.<sup>16</sup> 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.<sup>17</sup> The job destruction rate is slightly over 1% per month. The parameter estimate  $\kappa$  indicates that the probability that an application results in a match is smaller for participants. Thus, program participants send applications at a lower cost, but the applications have an approximately 8.5% lower probability of being effective ( $\kappa = 0.915$ ). Unemployment benefits are 64% of productivity, and the bargaining power of workers is 0.70. The leisure costs of participating in the activation program are less than 4% of productivity.

<sup>16</sup> Standard errors are computed as follows. Define the vector of data moments (listed in table 5) as  $\theta_x$  with covariance matrix  $\hat{\Sigma}_\theta$ . The estimates of  $\xi$  are a function of the data moments:  $\hat{\xi} = f(\theta)$ . The covariance matrix of the estimated parameters is given by  $\hat{\Sigma}_\xi = (\partial f(\theta)/\partial \theta) \hat{\Sigma}_\theta (\partial f(\theta)/\partial \theta)'$ .

<sup>17</sup> Note that the relatively large standard errors for  $\gamma_0$  and  $\gamma_1$  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 fits of the model. The ratio of the two is very precisely identified, however.

**Table 5**  
**Moment Conditions**

	Data Moment	Inverse Weight <sup>a</sup>	Description	Corresponding Value Model
1. Program effect on unemployment exit rate, participants	.059	.007 <sup>2</sup>	Estimated effect (see table 2)	$[1 - (1 - (m_1   \tau = \tau^e))^3] - [1 - (1 - (m_0   \tau = 0))^3]$
2. Program effect on unemployment exit rate, nonparticipants	-.033	.014 <sup>2</sup>	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	.05 <sup>2</sup>	Estimated percentage effect on vacancies (see table 4)	$((v^*   \tau = \tau^e) - (v^*   \tau = 0)) / (v^*   \tau = 0)$
4. Outflow within 3 months	.51	.0007	Fraction of unemployed in Storstrøm and South Jutland that leaves unemployment within 3 months (see fig. 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	.01	.02 <sup>2</sup>	Estimated effect on wages (see table 3)	$((w_1   \tau = \tau^e) - (w_0   \tau = 0)) / (w_0   \tau = 0)$
6. Program effect on log wage, nonparticipants	.01	.02 <sup>2</sup>	Estimated effect on wages (see table 3)	$((w_0   \tau = \tau^e) - (w_0   \tau = 0)) / (w_0   \tau = 0)$
7. Unemployment rate	.05	.0001	Unemployment rate in Storstrøm and South Jutland during the experiment (see table 1)	$v^*   \tau = \tau^e$
8. Vacancy rate	.01	.001	Approximation of the number of vacancies as a percentage of the labor force in Storstrøm and South Jutland	$b[1 / (\bar{w}^*   \tau = 0)]$
9. Replacement rate	.65	.001	Unemployment benefits are 65% of the wage level	

<sup>a</sup> 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, however, and therefore it receives a high weight. The vacancy rate is much more noisy, and consequently it receives a lower weight. The replacement rate (moment 9) is an approximate value, and it also receives a low weight.

**Table 6**  
**Structural Model Estimation and Simulation Results**

	Data Moment (1)	Baseline Model (2)	Cobb-Douglas (3)	Bertrand Wages (4)
Fixed parameters:				
Treatment share, experiment, $\tau^e$		.3	.3	.3
Per-period discount rate, $r$		.008	.008	.008
Productivity, $y$		1	1	1
Estimated parameters:				
Application cost, nonparticipants, $\gamma_0$		.504 (.142)	.907 (.663)	.033 (.005)
Application cost, participants, $\gamma_1$		.262 (.080)	.651 (.467)	.016 (.004)
Job destruction rate, $\delta$		.011 (.004)	.011 (.005)	.011 (.003)
Share of vacancies with positive surplus, $\kappa$		.915 (.040)		.895 (.044)
Unemployment insurance benefits, $b$		.638 (.007)	.650 (.205)	.527 (.043)
Bargaining power, $\beta$		.702 (.040)	.991 (.004)	
Vacancy costs, $c_v$		.981 (.119)	2.217 (1.342)	9.349 (.515)
Value nonmarket time, $b$		.036 (.038)	.023 (.155)	.109 (.021)
Search intensity elasticity, $\chi$			.000 (.000)	
Model Fit: Deviations from Data Moments				
Effect on job finding, nonparticipants	-.033	.000	.010	.008
Effect on job finding, participants	.059	.000	.001	.000
Effect on vacancies (%)	.047	-.004	-.025	-.004
Outflow within 3 months	.51	.000	.000	.000
Effect on wage, nontreated (%)	.01	-.011	-.010	-.028
Effect on wage, treated (%)	.01	-.009	-.010	-.004
Unemployment (for $\tau = .3$ )	.05	.000	.000	-.001
Vacancy rate	.01	.001	-.010	.002
Replacement rate	.65	.000	.000	.004
Key Model Predictions				
Policy-relevant treatment effect <sup>a</sup>		-.003 (.009)	.004 (.006)	.011 (.001)
Experimental (naive) treatment effect <sup>b</sup>		.050 (.004)	.045 (.334)	.046 (.003)
Change in unemployment <sup>c</sup>		.001 (.002)	-.001 (.014)	-.002 (.0003)
Change in welfare <sup>c,d</sup>		-.006 (.003)	-.002 (.002)	-.013 (.002)
Change in government expenditure <sup>c</sup>		.001 (.001)	.0003 (.010)	-.001 (.0001)

NOTE.—Standard errors are in parentheses.

<sup>a</sup> The policy-relevant treatment effect is defined as  $E[m(a_i^*; \bar{a}, \theta) | \tau = 1] - E[m(a_i^*; \bar{a}, \theta) | \tau = 0]$ .

<sup>b</sup> The experimental treatment effect estimate is defined as  $E[m(a_i^*; \bar{a}, \theta) | \tau = 0.3] - E[m(a_i^*; \bar{a}, \theta) | \tau = 0.3]$ .

<sup>c</sup> All changes are defined as  $(x | \tau = 1) - (x | \tau = 0)$ .

<sup>d</sup> Welfare is measured in terms of average monthly output per worker (which is normalized to 1) net of all costs. See eq. (12) for details.

The middle panel of table 6 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 effects of the program on both participants and nonparticipants are matched perfectly. Also the positive effect on vacancies and the outflow probability

within 3 months are matched perfectly. The wage effects are also close to the data, but since the empirical wage effects are 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.

### B. Increasing the Intensity of the Activation Program

Next, we use the model to predict how the program effects depend on the fraction  $\tau$  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  $\tau$  in the unemployed population. The results are shown in figure 6. The graph on the top left shows that increasing the treatment rate  $\tau$  monotonically increases the unemployment rate. The difference between no treatment and full treatment is approximately 0.06%. The increase in unemployment can be explained by the matching rates, which are presented in the 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  $\tau$  and shows that participants are about 5% more likely to find a job within a given month. The matching rates of both participants and nonparticipants decrease monotonically as  $\tau$  increases, and even the aggregate (average) matching rate decreases marginally. For a program intensity of 30%, participants have a 24% higher matching rate than nonparticipants.

The simulated matching rates can be related to the treatment effects presented in Section II.B. Results, including standard errors, can be found in the bottom panel of table 6. The evaluation of the randomized experiment estimates a treatment effect on the matching rates equal to  $0.243 - 0.193 = 0.050$ , which is  $E[m(a_1^*; \bar{a}, \theta) | \tau = 0.3] - E[m(a_0^*; \bar{a}, \theta) | \tau = 0.3]$ . However, the policy-relevant treatment effect is  $E[m(a_1^*; \bar{a}, \theta) | \tau = 1] - E[m(a_0^*; \bar{a}, \theta) | \tau = 0]$ , which is  $-0.003$  and not statistically different from 0. Even though the microeconomic evaluation suggests a positive effect on the matching rate, the policy-relevant treatment effect is close to 0.

The higher unemployment rate, combined with more applications per job seeker, leads to an increase in the number of vacancies. As can be seen in the middle-left graph in figure 6, market tightness increases in  $\tau$ , as vacancy supply increases more than unemployment. The wage negotiated by program participants is slightly higher than that of nonparticipants, because of their better outside option. The difference is small, however, and both levels are relatively constant as  $\tau$  increases. Furthermore, we find that the average num-

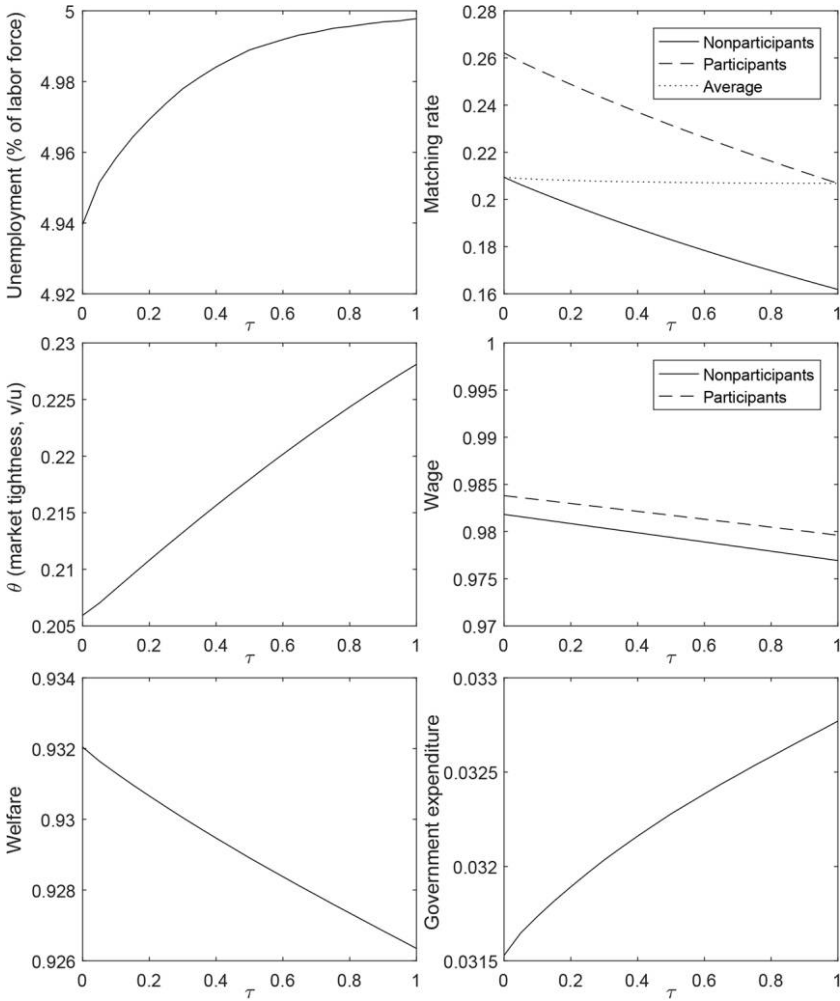


FIG. 6.—Simulation results of the baseline model.

ber of applications per month in the population rises from 0.65 to 0.95 as  $\tau$  increases from 0 to 1 (not shown in the figure).<sup>18</sup>

The estimated model allows for different types of cost-benefit analyses, as discussed in Section V.C. Equation (12) defines welfare as a function of  $\tau$ , which is plotted in the bottom-left graph of figure 6. In line with rising unemployment, vacancies (and thus vacancies' posting costs), and program

<sup>18</sup> The average number of applications per month is low and likely much lower than the actual number of vacancies to which unemployed workers apply. However, in our model  $a$  describes applications for which the unemployed worker is among the most suitable candidates and has a fair chance of receiving a job offer.

costs, welfare decreases with rising  $\tau$  and is maximized for  $\tau = 0$ . A large-scale rollout of the program reduces welfare by almost 0.006, which corresponds to 0.6% of workers' productivity. The reason for the decline in welfare is that the negative congestion effects of the increased search intensity dominate. We also consider total government expenditures on unemployment benefits and the costs of the activation program (see eq. [13]). We find that government expenditure increases in the share of the unemployed workers that participate in the activation program; see the bottom-right graph in figure 6. A large-scale rollout of the activation program increases total government expenditure on the unemployment benefits program by 4%.

Microeconomic evaluations often ignore equilibrium effects. Equation (14) shows the cost-benefit analysis that is usually applied in microeconomic evaluations. It simply compares the costs of the program with the difference in total benefits payments between participants and non-participants. The costs of the program ( $c_p$ ) are 2,122 DKK (about \$382), while the difference in average unemployment duration is 1.1 months. Average monthly benefit payments are 14,800 DKK. The gain for the government budget is, therefore, 14,158 DKK for each participant in the activation program. This microeconomic evaluation thus erroneously provides a positive assessment of the activation program.

Finally, we calculate how restrictive SUTVA would be in this application. We do this by evaluating the welfare function while ignoring all equilibrium effects. Specifically, we fix  $a_0^*$ ,  $a_1^*$ , and  $\theta$  at their values for  $\tau = 0$ . This implies that  $m_0(a_0^*; a_1^*, \theta)$  and  $m_1(a_1^*; a_0^*, \theta)$  also no longer vary with  $\tau$ . Next, we use equation (10) to compute the unemployment rate  $u$  for different values of  $\tau$ , which implicitly determines the vacancy rate  $v = \theta u$ . We can then substitute all variables in the welfare function, equation (12). Figure 7A compares predicted welfare when erroneously ignoring equilibrium effects with welfare that does take them into account. The results show that when equilibrium effects are ignored, welfare would increase in the treatment intensity  $\tau$ . The difference between the two welfare measures is shown in figure 7B, which can be interpreted as an error measure for the violation of the SUTVA assumption. For  $\tau = 1$ , the difference is 1.6% of output, and more importantly, under SUTVA the welfare effects appear to be positive instead of negative.

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. At 0% program participation, the unemployment rate is minimized and welfare is maximized. The results show the importance of taking equilibrium effects into account, and therefore the conclusions do not concur with the results from a standard microeconomic evaluation that typically ignores equilibrium effects.

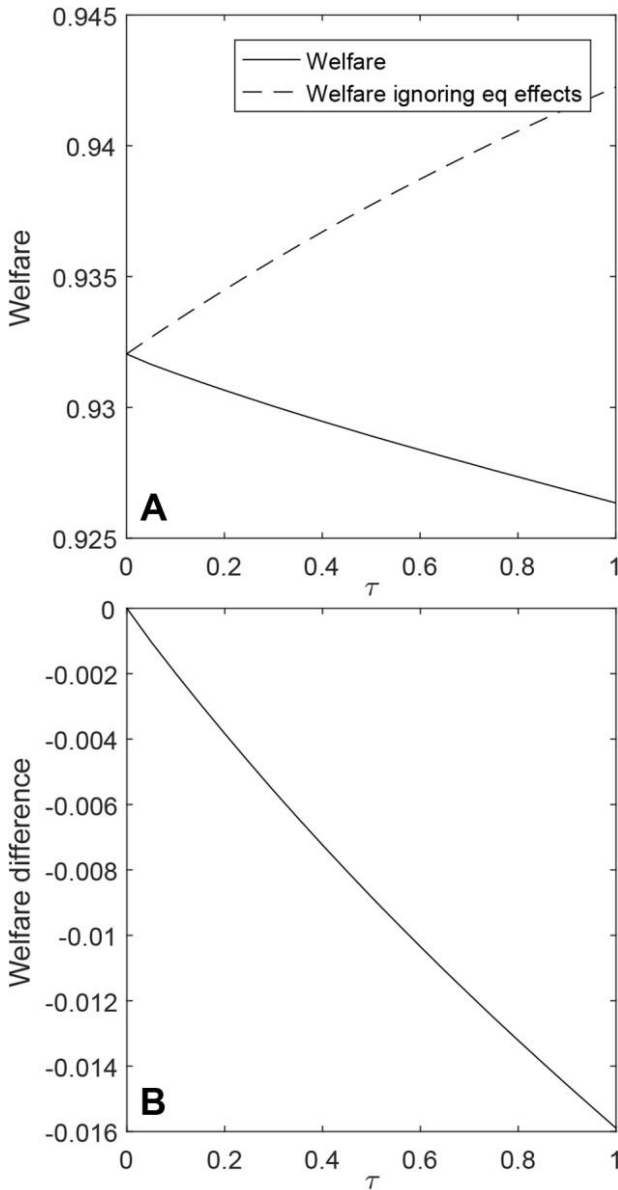


FIG. 7.—Welfare ignoring equilibrium (eq) effects.

### C. Robustness Checks

In this subsection, we address the robustness of our empirical results. We focus on modeling choices in the equilibrium search model. We made three key assumptions. First, our matching function is of the urn-ball type rather



than the more commonly used Cobb-Douglas type. Second, wages are determined by Nash bargaining. Third, the treatment intensity during the experiment (30%) is based on a steady-state assumption. Below, we discuss alternatives to these assumptions.

### 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 matching function. The Cobb-Douglas function allows for different search intensities for treated and nontreated individuals, which are optimally chosen by the job seeker.

By allowing for a distinct elasticity ( $\chi$ ) 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

$$M(\bar{a}, u, v) = \bar{a}^\chi u^\alpha v^{1-\alpha}, \quad (15)$$

where  $\bar{a}$  is the average search intensity in the population. This specification is used because in a standard version ( $M(\bar{a}, u, v) = (\bar{a}u)^\alpha v^{1-\alpha}$ ), the estimate of  $\alpha$  converges to 0 (or to a negative value if no restriction is used in the estimation procedure). This is unrealistic and a clear signal that this specification is not flexible enough to reproduce the empirical findings. To keep the number of estimated parameters equal to the baseline model, we estimate  $\chi$  and set  $\alpha = 0.74$  (as was estimated for Denmark by Albaek and Hansen 1995). Results are very similar when  $\alpha = 0.5$ . Parameter estimates and the fit of the model can be found in column 3 of table 6. Note that the estimate for  $\chi$  equals 0, indicating that the spillover effect on nonparticipants can be matched only with a nonpositive relation between search intensity and aggregate matches. The fit of the model is not as good as that of the baseline model. The spillover effect on nonparticipants and the effect on vacancies are only partially reproduced. Also, the vacancy rate is much lower than the targeted level. The simulation results (bottom panel of table 6) show that the main predictions are quite similar to the baseline model's, although standard errors are larger. The policy-relevant treatment effect is close to 0, while the experimental (naive) estimate is positive. Unemployment decreases slightly, but the change in welfare is still negative, and government expenditure increases as  $\tau$  increases. The simulation results are summarized in figure C1. We conclude that the qualitative result for welfare is in line with our baseline model but that the fit is worse.

## 2. *The Wage Mechanism*

In the baseline model, wages are determined by Nash bargaining. In Section V.B, *ex post* Bertrand competition was mentioned as an alternative wage-setting mechanism. Below, we briefly discuss the results from Bertrand competition (see Albrecht, Gautier, and Vroman 2006 and app. C for more details).

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.

Parameter estimates, the fit of the model, and simulation predictions can be found in column 4 of table 6. The simulation results are presented in figure C2. We find that, except for a higher value of nonmarket time and very large vacancy costs, the parameter estimates are similar to the ones in the baseline model. Standard errors are smaller, however. The fit of the model is not as good as that of 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, again, much smaller than the experimental (naive) treatment effect. Unemployment decreases slightly, but welfare still decreases when  $\tau$  increases, as was the case in the baseline model. Government expenditures decrease slightly. The main conclusion, that because of spillovers the program leads to small but negative welfare effects, continues to hold under Bertrand wages.

## 3. *Treatment Intensity*

The model has been estimated under the assumption that about 30% of the unemployed workers participate in the activation program toward the end of the experiment period ( $\tau^c = 0.3$ ). 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 is reduced to about 26%. Furthermore, recall from Section III that the inflow into unemployment was higher in the preexperiment 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 lower values of  $\tau^c$ , 0.25 and 0.2. The intuition for how this changes the results is straightforward. A lower value of  $\tau^c$  implies that the observed negative program effects on the nonparticipants must be the result of a smaller group of treated individuals and thus that congestion effects must be even larger. Consequently, welfare decreases faster.

Heterogeneity in the pool of unemployed could also affect the value of  $\tau^e$  in a different way. One may argue that perhaps longer-term unemployed are unable to find a job anyway and therefore are not harmed by the higher job-finding rate of the program participants. If one takes this argument to the limit, the spillover effects are relevant only for the group of recently unemployed, which is the 50% of the newly unemployed who 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.<sup>19</sup> The resulting value of  $\tau^e$  would be 0.5. Even though this is an unlikely value, we present estimates based on  $\tau^e = 0.5$  as a lower bound on the spillover effects.<sup>20</sup>

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 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  $\tau^e$  are presented in figure 8, together with the simulation results from the baseline model ( $\tau^e = 0.3$ ). In the figure, we plot the unemployment rate and welfare. To make the different estimates comparable, we normalize welfare to 1 in the case where no unemployed worker enters the activation program. As expected, a lower value of  $\tau^e$  aggravates the negative effect of the activation program on the unemployment rate and welfare. For  $\tau^e = 0.2$ , the effects on unemployment and welfare are large, and full participation would increase unemployment by almost 1 percentage 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 on the unemployment rate. In this scenario, full participation reduces unemployment, but a slightly negative effect on welfare remains (relative to the case where no workers participate in the activation program).

#### 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.

<sup>19</sup> Another argument against this case is that almost everyone in our sample finds a job within 2 years, and therefore a scenario with a zero job-finding rate for long-term unemployed workers is unlikely.

<sup>20</sup> 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.

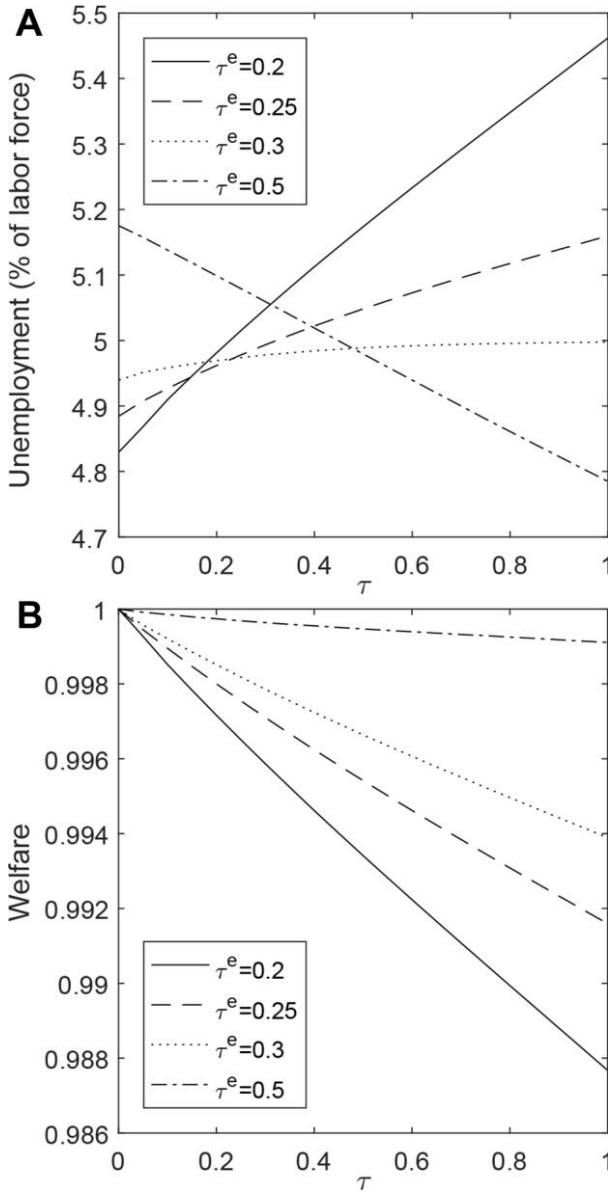


FIG. 8.—Simulation results from estimations using different values of  $\tau^e$ .

We assumed that workers and jobs are ex ante 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 IV.B, there is no evidence for an effect on wages. Therefore, it suffices for our application to restrict ourselves to ex ante homogeneous workers. Moreover, the estimate  $\kappa$  is less than 1, which suggests that the success rate of the extra applications made by the participants is relatively low. This goes against the idea that the activation program improves the quality of job applications or helps participants to select more suitable vacancies.

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 ( $\tau^e$ ) in the total pool of searchers. In Section VI.C.3, we replicated our analysis for different values of  $\tau^e$ , and we found that reducing  $\tau^e$  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 nonexperiment 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 afterward. The extent to which this discrepancy matters depends on how quickly the labor market moves toward a steady state. As stated in Section III, the median unemployment duration during the experimental period was about 15 weeks, which is much shorter than that in most other European countries and only a few weeks longer than that in the United States. This is because unemployment inflow and outflow rates are high and, in that case, the half-life of a deviation from steady-state unemployment is short, see Shimer (2012) and Elsby, Michaels, and Solon (2009). Therefore, the steady-state assumption not very restrictive for this application.

## VII. 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 estimate the program effect not only on the participants but also on the nonparticipating individuals. We consider various outcomes. In particular, we find that the activation program increases the job-finding rate of participants but has adverse effects on the job-finding rate of the nonparticipants. This implies that simply comparing unemploy-

ment durations of participants and nonparticipants overestimates the effects of the activation program. We found no impact of the program on post-unemployment wages of participants, nor do we find evidence of spillovers in terms of post-unemployment wages. Finally, we provide some 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 through indirect inference. The model allows for simulation of scenarios in which the number of participants in the activation program increases up to a large-scale rollout. 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 rollout 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 microeconomic evaluation and emphasize the importance of considering spillover effects when evaluating labor market programs.

Our setting is one in which intensive job search assistance is offered on top of basic assistance. Our results do not imply that all active labor market programs, which aim at increasing job search effort, are welfare reducing. In particular, when job search effort is low or when the program improves the match quality, the welfare effects can be positive.

## Appendix A

### Alternative Models for Unemployment Duration

In this appendix, we consider two alternative models to estimate the effects of the activation program on participants and nonparticipants.

#### A1. Log Duration Model

A disadvantage of the linear probability model is that it 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 a difference-in-differences specification similar to equation (2):

$$\log(T_i) = \alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i + \eta_{p_i} + U_i. \tag{A1}$$

A problem with this approach is that it cannot deal with censoring. However, because censoring occurs after 2 years, only 3% of the observations are censored. Estimation results are presented in column 1 of table A1 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 nonparticipants increases by approximately 7%. Both effects are significant at the 1% level.

**A2. Duration Model**

Previous studies used duration models to evaluate the experiment (Graversen and van Ours 2008; Rosholm 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:

$$\theta(t|p_i, r_i, x_i, d_i, c_i) = \lambda_{p_i}(t) \exp(\alpha_{r_i} + x_i\beta + \delta d_i + \gamma c_i), \tag{A2}$$

where  $\lambda_{p_i}(t)$  describes duration dependence, which is allowed to differ between individuals who enter unemployment in the experiment period and those who enter in the preexperiment 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 Tunali 1999).<sup>21</sup> The key advantage is that this does not 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  $i = 1, \dots, n$ , and the indicator variable  $e_i$  takes the value 1 if an exit from unemployment was observed and 0 if the observation has been censored. Stratified partial likelihood estimation optimizes the likelihood function

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

The set  $\mathcal{I}_p$  includes all individuals who entered unemployment in the same calendar time period (experiment or preexperiment) and therefore share the

<sup>21</sup> We tried estimating the model parameters by using Markov chain Monte Carlo methods allowing for unobserved 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 are the treatment effects on both the treated and the nontreated highly significant.

same duration dependence pattern. A Hausman test rejects that the pattern of duration dependence is the same in both time periods ( $p$ -value  $< .01$ ). 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 2 of table A1 shows the estimates using all information on unemployment durations in the data. Participating in the activation program increases the exit rate from unemployment by  $100\% \times (\exp(0.154) - 1) \approx 17\%$ , compared to not having any activation program. The effect is significant at the 1% level. The effect of the presence of the activation program on the exit rate of the nonparticipants in the program is negative but not significant.

The results in Section IV.A show that most effects of the activation program occur 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 2 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 1 year or 3 months. Results are shown in columns 3 and 4 of table A1. Censoring the data after 1 year has little effect on the results; the estimated effects are close to those in column 2. When we censor the spells after 3 months, the negative effect of the program on nonparticipants is much larger in magnitude and significant at the 1% level (see col. 4). The coefficient corresponds to an 11% decrease in the exit rate during the first 3 months of unemployment for the nonparticipants. The effect for the participants remains similar to that with 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, for example, by Graversen and van Ours (2008) and Rosholm (2008). The activation program is effective in stimulating participants to leave 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.<sup>22</sup>

<sup>22</sup> In theory, we can allow the treatment effects  $\delta$  and  $\gamma$  to depend on the treatment intensity  $\tau$ . 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.



In our specification we allow the duration dependence pattern to be different in the two calendar time periods, and we include fixed effects for all counties. Alternatively, we can include fixed effects for the calendar 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 close to the experiment counties or located as far away as possible or counties that 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 B.

**Table A1**  
**Estimated Effects of the Activation Program on Exit Rates of Participants and Nonparticipants**

	Regression on Log Unemployment (1)	Duration Model: Data Censored after		
		2 Years (2)	1 Year (3)	3 Months (4)
Participants	-.14*** (.02)	.154*** (.031)	.167*** (.032)	.151*** (.042)
Nonparticipants	.07*** (.02)	-.044 (.030)	-.031 (.031)	-.115*** (.044)
Individual characteristics	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Observations	77,057	77,057	77,057	77,057

NOTE.—Standard errors are in parentheses. Individual characteristics include gender, age dummies, education level, log previous earnings, immigrant status, labor market history, and quarter of entering unemployment. All estimates are coefficients.

\*\*\* Significant at the 1% level.

## Appendix B

### Empirical Analyses with Restricted Comparison Counties

In Section IV, we presented our empirical results, which are based on comparing the experiment counties with all other Danish counties. Both the preexperiment 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 that 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 that are closest to the experiment counties. These counties might be most similar and experience a trend very close to that of the experiment counties. However, if there are spillovers between counties due to, for example, workers commut-

ing between counties, this most likely affects neighboring counties most. Therefore, as a second sensitivity analysis we consider the two counties that are farthest from the experiment counties as control counties. Finally, we consider as control counties five counties that are most similar in aggregate statistics to the experiment counties.

Table B1 shows, for the duration model for unemployment, the estimation results for the three sensitivity analyses. Comparing the parameter estimates across the different columns and with those presented in table A1 shows that the estimated effects are quite robust against the choice of the comparison counties.

In table B2, we repeat the sensitivity analyses but now for the difference-in-differences model for the stock of vacancies. Although the significance levels differ between the different choices of comparison counties, all results indicate substantial equilibrium effects quantitatively similar to those presented in table 4.

**Table B1**  
**Estimated Effects of the Activation Program on Outcomes of Participants and Nonparticipants with Restricted Comparison Groups**

	Three Closest Counties (1)	Two Farthest Counties (2)	Five Most Similar Counties (3)
Exit within 3 months:			
Participants	.072*** (.008)	.078*** (.010)	.073*** (.007)
Nonparticipants	-.021 (.014)	-.015 (.016)	-.029 (.014)
Log duration:			
Participants	-.198*** (.019)	-.155*** (.019)	-.156*** (.022)
Nonparticipants	.004 (.022)	.046* (.020)	.047* (.025)
PH censored at 3 months:			
Participants	.208*** (.046)	.205*** (.047)	.189*** (.043)
Nonparticipants	-.061 (.048)	-.066 (.049)	-.079* (.045)
Log earnings:			
Participants	.020 (.025)	-.005 (.024)	.009 (.023)
Nonparticipants	.020 (.025)	-.007 (.024)	.009 (.023)
Hours worked:			
Participants	1.002 (1.203)	.364 (1.286)	.531 (1.271)
Nonparticipants	.776 (1.191)	.216 (1.275)	.387 (1.258)
Individual characteristics	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Observations	28,394	25,530	53,682

NOTE.—Standard errors (clustered by county interacted with time period) are in parentheses. The closest counties are West Zealand, Ribe, and Funen; the farthest counties are Viborg and North Jutland; and the 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. PH = proportional hazard.

\* Significant at the 10% level.  
 \*\*\* Significant at the 1% level.

**Table B2**  
**Estimated Effects of the Experiment on the Logarithm of Vacancies with Restricted Comparison Groups**

	Three Closest Counties (1)	Two Farthest Counties (2)	Five Most Similar Counties (3)
Experiment Nov–Dec 2005	.092 (.094)	.039 (.168)	.039 (.098)
Experiment Jan–Feb 2006	.127*** (.023)	.025 (.144)	.089 (.060)
Experiment Mar–Apr 2006	.146** (.035)	.014 (.074)	.106* (.049)
Experiment May–Jun 2006	.158* (.068)	.088 (.053)	.120* (.049)
Experiment Jul–Aug 2006	.079 (.069)	.185** (.033)	.095* (.046)
Experiment Sep–Oct 2006	.009 (.108)	–.043 (.040)	–.066 (.038)
County fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Observation period	Jan 2004–Dec 2007	Jan 2004–Dec 2007	Jan 2004–Dec 2007

NOTE.—Standard errors (clustered by county) are in parentheses. The closest counties are West Zealand, Ribe, and Funen; the farthest counties are Viborg and North Jutland; and the most similar counties are Funen, West Zealand, North Jutland, Viborg, and Aarhus.

- \* Significant at the 10% level.
- \*\* Significant at the 5% level.
- \*\*\* Significant at the 1% level.

### Appendix C

#### Equilibrium Search Model with Bertrand Competition

In this appendix, we follow Albrecht, Gautier, and Vroman (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 ( $w = \gamma$ ). But if a worker receives only one offer, the firm receives the full surplus. In this latter case, the worker receives the reservation wage ( $w = w_1$ ). Therefore, the wage depends on the number of offers (denoted by  $j$ ), and the probability of receiving the low reservation wage, given a match, is

$$p_1(a) \equiv \Pr(j = 1 | j > 0) = \frac{\Pr(j = 1)}{\Pr(j > 0)}.$$

Recall, from Section V.B, that the probability that an application results in a job offer equals  $\psi = (\kappa\theta/\bar{a})(1 - \exp(-\bar{a}/\kappa\theta))$ , with  $\kappa = 1$  for the non-participants. In a large labor market, the number of job offers when making  $a$  applications follows a Poisson distribution with intensity  $\psi a$ . This implies that

$$p_1(a) = \frac{\psi a \exp(-\psi a)}{1 - \exp(-\psi a)} = 1 - p_h(a),$$

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 com-

petition, 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:

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

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.<sup>23</sup> For an unemployed worker who is in treatment state  $i$  and 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,

$$rU_0 = \max_{a \geq 0} b + b - \gamma_0 a^2 + m(a; \bar{a}, \theta)(E_0 - U_0) \tag{C1}$$

and

$$rU_1 = \max_{a \geq 0} b - b + \gamma_1 a^2 + m(a; \bar{a}, \theta)(E_1 - U_1). \tag{C2}$$

Since participants and nonparticipants in the activation program make different numbers of applications, respectively denoted  $a_1^*$  and  $a_0^*$ , they will also have different reservation wages.

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

$$\begin{aligned} E_{0,l} &= U_0, \\ E_{1,l} &= U_1, \end{aligned}$$

and therefore

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

The value functions for a filled job (eq. [8]) now become

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

<sup>23</sup> 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 is made in the baseline model.

This last equation gives the expected value of a filled vacancy, where  $\bar{p}_h$  describes the average probability in the population ( $\bar{p}_1 = (1 - \tau)p_{0,1}(a_0^*) + \tau p_{1,1}(a_1^*)$ ).

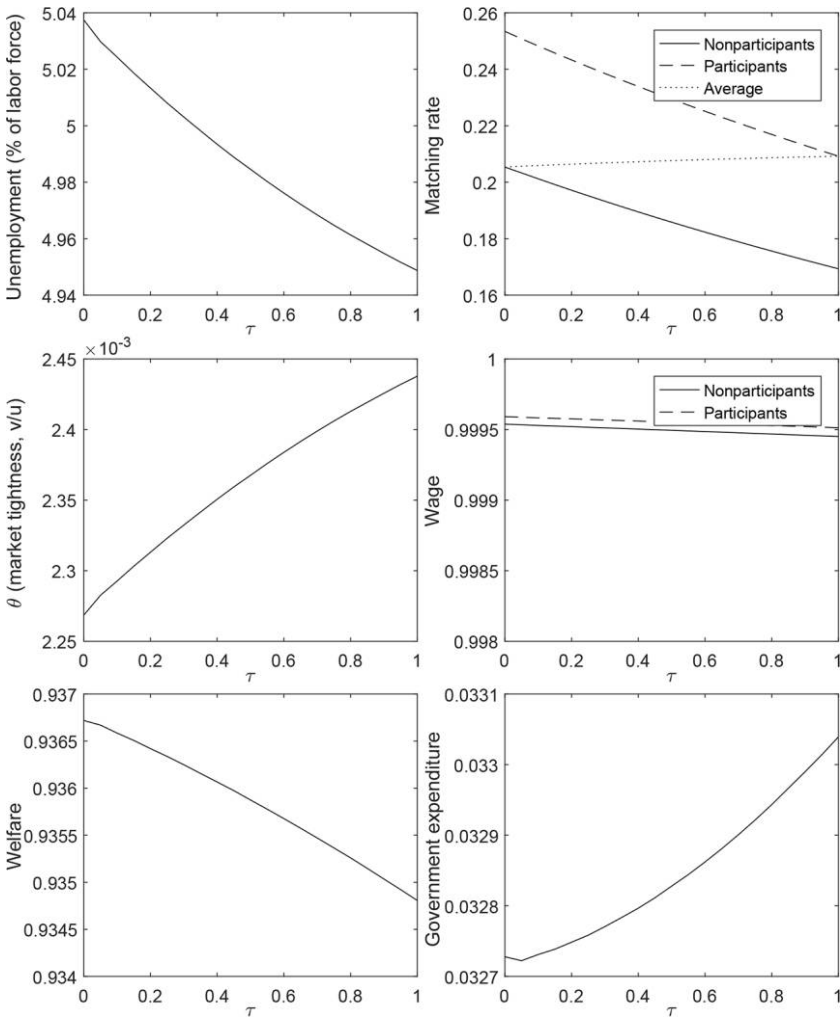


FIG. C1.—Simulation results: model with Cobb-Douglas matching function.

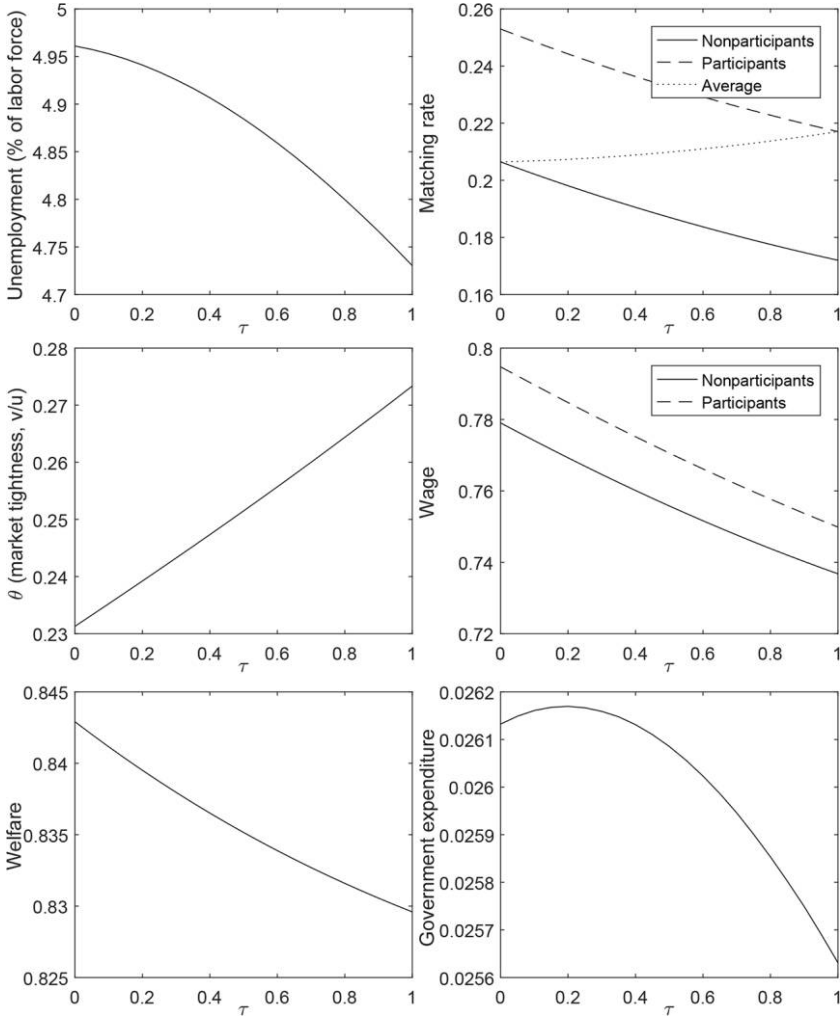


FIG. C2.—Simulation results with Bertrand wages.

**References**

Abbring, Jaap H., and James J. Heckman. 2007. Econometric evaluation of social programs, part III: Distributional treatment effects, dynamic treatment effects, dynamic discrete choice, and general equilibrium policy evaluation. In *Handbook of econometrics*, vol. 6B, ed. James J. Heckman and Edward Leamer, 5145–303. Amsterdam: North Holland.

- Albaek, Karsten, and Hendrik Hansen. 1995. Estimating aggregate labor market relations. Unpublished manuscript, University of Copenhagen.
- Albrecht, James, Pieter A. Gautier, and Susan Vroman. 2006. Equilibrium directed search with multiple applications. *Review of Economic Studies* 73, no. 4:869–91.
- Albrecht, James, Gerard J. van den Berg, and Susan Vroman. 2009. The aggregate labor market effects of the Swedish Knowledge Lift program. *Review of Economic Dynamics* 12, no. 1:129–46.
- Ashenfelter, Orley, David Ashmore, and Olivier Deschênes. 2005. Do unemployment insurance recipients actively seek work? Randomized trials in four U.S. states. *Journal of Econometrics* 125, no. 1–2:53–75.
- Attanasio, Orazio P., Costas Meghir, and Ana Santiago. 2012. Education choices in Mexico: Using a structural model and a randomized experiment to evaluate PROGRESA. *Review of Economic Studies* 79, no. 1:37–66.
- Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119, no. 1:249–75.
- Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel. 2003. Is the threat of reemployment services more effective than the services themselves? Evidence from random assignment in the UI system. *American Economic Review* 93, no. 4:1313–27.
- Blundell, Richard, Monica Costa Dias, and Costas Meghir. 2003. The impact of wage subsidies: A general equilibrium approach. Unpublished manuscript.
- Blundell, Richard, Monica Costa Dias, Costas Meghir, and John van Reenen. 2004. Evaluating the employment impact of a mandatory job search program. *Journal of the European Economic Association* 2, no. 4:569–606.
- Cahuc, Pierre, and Thomas Le Barbanchon. 2010. Labor market policy evaluation in equilibrium: Some lessons of the job search and matching model. *Labour Economics* 17, no. 1:196–205.
- Card, David, and Dean R. Hyslop. 2005. Estimating the effects of a time-limited earnings subsidy for welfare-leavers. *Econometrica* 73, no. 6:1723–70.
- Cox, David R. 1958. *Planning of experiments*. New York: Wiley.
- Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. Do labor market policies have displacement effects? Evidence from a clustered randomized experiment. *Quarterly Journal of Economics* 128:531–80.
- Deding, Mette, and Trine Filges. 2003. Is love all that matters? An empirical analysis of geographical mobility in Denmark. Unpublished manuscript.
- Diamond, Peter A. 1982. Aggregate demand management in search equilibrium. *Journal of Political Economy* 90, no. 5:881–94.

- DiNardo, John, and David S. Lee. 2011. Program evaluation and research designs. In *Handbook of labor economics*, vol. 4A, ed. Orly Ashenfelter and David Card, 463–536. Amsterdam: Elsevier.
- Dolton, Peter, and Donal O’Neill. 1996. Unemployment duration and the re-start effect: Some experimental evidence. *Economic Journal* 106, no. 435:387–400.
- Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2011. Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *American Economic Review* 101, no. 5:1739–74.
- Eberwein, Curtis, C. John Ham, and Robert J. LaLonde. 2002. Alternative methods of estimating program effects in event history models. *Labour Economics* 9, no. 2:249–78.
- Elsby, Michael W. L., Ryan Michaels, and Gary Solon. 2009. The ins and outs of cyclical unemployment. *American Economic Journal: Macroeconomics* 1, no. 1:84–110.
- Ferracci, Marc, Grégory Jolivet, and Gerard J. van den Berg. 2014. Evidence of treatment spillovers within markets. *Review of Economics and Statistics* 96, no. 5:812–23.
- Fougère, Denis, Jacqueline Pradel, and Muriel Roger. 2009. Does the public employment service affect search effort and outcomes? *European Economic Review* 53, no. 7:846–69.
- Frijters, Paul, and Bas van der Klaauw. 2006. Job search with nonparticipation. *Economic Journal* 116, no. 508:45–83.
- Gorter, Cees, and Guyonne R. J. Kalb. 1996. Estimating the effect of counseling and monitoring the unemployed using a job search model. *Journal of Human Resources* 31, no. 3:590–610.
- Gourieroux, Christian, Alain Monfort, and Eric Renault. 1993. Indirect inference. *Journal of Applied Econometrics* 8, no. S1:S85–S118.
- Graversen, Brian K., and Jan C. van Ours. 2008. How to help unemployed find jobs quickly: Experimental evidence from a mandatory activation program. *Journal of Public Economics* 92, no. 10–11:2020–35.
- . 2011. An activation program as a stick to job finding. *Labour* 25, no. 2:167–81.
- Greenberg, David H., and Philip K. Robins. 2008. Incorporating nonmarket time into benefit-cost analyses of social programs: An application to the self-sufficiency project. *Journal of Public Economics* 92, no. 3–4:766–94.
- Heckman, James J., Lance Lochner, and Christopher Taber. 1998. General-equilibrium treatment effects: A study of tuition policy. *American Economic Review* 88, no. 2:381–86.
- Heckman, James J., and Edward Vytlacil. 2005. Structural equations, treatment effects, and econometric policy evaluation. *Econometrica* 73, no. 3:669–738.



- Johnson, Terry R., and Daniel H. Klepinger. 1994. Experimental evidence on unemployment insurance work-search policies. *Journal of Human Resources* 29, no. 3:695–717.
- Lalive, Rafael, Camille Landais, and Josef Zweimüller. 2015. Market externalities of large unemployment insurance extension programs. *American Economic Review* 105, no. 12:3564–96.
- LaLonde, Robert J. 1986. Evaluating the econometric evaluations of training programs with experimental data. *American Economic Review* 76, no. 4:604–20.
- Lise, Jeremy, Shannon Seitz, and Jeffrey Smith. 2004. Equilibrium policy experiments and the evaluation of social programs. NBER Working Paper no. 10283 (February), National Bureau of Economic Research, Cambridge, MA.
- . 2015. Evaluating search and matching models using experimental data. *IZA Journal of Labor Economics* 4:16. doi:10.1186/s40172-015-0031-7.
- Meyer, Bruce D. 1995. Lessons from U.S. unemployment insurance experiments. *Journal of Economic Literature* 33, no. 1:91–131.
- Miguel, Edward, and Michael Kremer. 2004. Worms: Identifying impacts on education and health in the presence of treatment externalities. *Econometrica* 72, no. 1:159–217.
- Moretti, Enrico. 2004. Estimating the social return to higher education: Evidence from longitudinal and repeated cross-sectional data. *Journal of Econometrics* 121, no. 1–2:175–212.
- Mortensen, Dale T. 1982. The matching process as a noncooperative bargaining game. In *The economics of information and uncertainty*, ed. John J. McCall, 233–58. National Bureau of Economic Research Conference Report no. 32. Chicago: University of Chicago Press.
- Pissarides, Christopher A. 2000. *Equilibrium unemployment theory*. Cambridge, MA: MIT Press.
- Ridder, Geert, and Ínsan Tunali. 1999. Stratified partial likelihood estimation. *Journal of Econometrics* 92, no. 2:193–232.
- Rosholm, Michael. 2008. Experimental evidence on the nature of the Danish employment miracle. IZA Discussion Paper no. 3620, Institute of Labor Economics, Bonn.
- Rubin, Donald B. 1978. Bayesian inference for causal effects: The role of randomization. *Annals of Statistics* 6, no. 1:34–58.
- Shimer, Robert. 2012. Reassessing the ins and outs of unemployment. *Review of Economic Dynamics* 15, no. 2:127–48.
- Smith, Anthony A. 1993. Estimating nonlinear time-series models using simulated vector autoregressions. *Journal of Applied Econometrics* 8, no. 5:63–84.
- Todd, Petra E., and Kenneth I. Wolpin. 2006. Assessing the impact of a school subsidy program in Mexico: Using a social experiment to validate

- a dynamic behavioral model of child schooling and fertility. *American Economic Review* 96, no. 5:1384–417.
- van den Berg, Gerard J., and Bas van der Klaauw. 2006. Counseling and monitoring of unemployed workers: Theory and evidence from a controlled social experiment. *International Economic Review* 47, no. 3:895–936.
- van den Berg, Gerard J., and Aico van Vuuren. 2010. The effect of search frictions on wages. *Labour Economics* 17, no. 6:875–85.
- Vikström, Johan, Michael Rosholm, and Michael Svarer. 2013. The effectiveness of active labor market policies: Evidence from a social experiment using non-parametric bounds. *Labour Economics* 24:58–67.