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Does Job Search Assistance Reduce Unemployment? Experimental Evidence on Displacement Effects and Mechanisms

Maria Cheung, Johan Egebark, Anders Forslund, Lisa Laun, Magnus Rödin and Johan Vikström

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

Does job search assistance reduce unemployment? Experimental evidence on displacement effects and mechanisms*

Maria Cheung[a] Johan Egebark[b] Anders Forslund[c]
Lisa Laun[d] Magnus Rödin[a] Johan Vikström[e]

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Abstract

This paper uses a large-scale two-level randomized experiment to study direct and displacement effects of job search assistance. Our findings show that the assistance reduces unemployment among the treated, but also creates substantial displacement leading to higher unemployment for the non-treated. By using detailed information on caseworker and job seeker behavior we show that vacancy referrals passed on from caseworkers to job seekers is the driving mechanism behind the positive direct effect. We also examine explanations for the displacement effect and show that displacement is not due to constrained resources, but arises in the labor market. A comparison between different meeting formats suggests that face-to-face meetings and distance meetings are more effective than group meetings. Despite the existence of displacement effects, when we incorporate our results into an equilibrium search model we find that a complete roll-out of the program would lead to lower unemployment and reduced government spending.

Keywords: vacancy referrals, counseling, job search, randomized experiment.
JEL codes: J68, J64, C93

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[a]Swedish Public Employment Service.

[b]Swedish Public Employment Service and Research Institute of Industrial Economics (IFN), Stockholm.

[c]IFAU and UCLS, Uppsala University.

[d]IFAU.

[e]IFAU and UCLS, Uppsala University. Corresponding Author: IFAU, Box 513, S-751 20 Uppsala, Sweden. E-mail: johan.vikstrom@ifau.uu.se.

1 Introduction

Job search assistance (JSA), aimed at helping job seekers search for jobs more efficiently, constitutes an important component of active labor market programs (ALMPs) in many OECD countries. This widespread use of JSA as a policy instrument raises the important question to what extent such programs effectively reduce unemployment. Previous evidence suggests that job search assistance is one of the most powerful tools in the ALMP toolbox. As summarized in Card et al. (2010, 2017), both observational studies and randomized experiments from a wide range of countries document generally positive impacts of JSA.[1] Despite the numerous previous studies, there are still important questions where evidence is lacking. Few studies have been able to explain why job search assistance is comparably effective. In particular, little is known about how JSA affects caseworker and job seeker behavior and how this operates to the positive employment effects. This is unfortunate since a deeper understanding of the mechanisms may help to fine-tune programs and improve their efficiency. In addition, one concern is that the positive results for those who participate in JSA policies reflect negative impacts on non-treated job seekers (Crépon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). Since such displacement effects, which mainly represent a re-ordering of job queues, have important implications for the overall effectiveness of JSA programs, more evidence is needed on their size and origin.

This paper contributes to the literature on JSA in several ways. We present evidence from a large-scale two-level randomized experiment designed to detect both direct and displacement effects. We exploit rich information on caseworker actions and job seeker search behavior, which allow us to study mechanisms in a number of dimensions that, to the best of our knowledge, have not been studied before. We also analyze the

[1] The evidence from experiments includes for instance Gorter and Kalb (1996), Dolton and O'Neill (1996), Dolton and O'Neill (2002), van den Berg and van der Klauuw (2006), Hägglund (2011), Graversen and van Ours (2008a), Graversen and van Ours (2008b), Crépon et al. (2013), Arni (2015) and Maibom et al. (2017). Two recent US studies include McConnell et al. (2016) and Manoli et al. (2018).

origin of any displacement by contrasting displacement due to resource constraints and displacement of jobs. Comparing three different meeting formats further adds to the understanding of the mechanisms. Finally, we investigate the potential impact of a full-scale roll-out of the program, by developing and estimating an equilibrium search model using the results from the experiment.

The experiment, conducted in 2015, consisted of more frequent meetings with a caseworker during the first quarter of unemployment and included randomization of treatment both across and within local employment offices. It targeted newly unemployed job seekers at 72 PES offices in Sweden (one quarter of all offices), where each office typically served one entire local labor market. We randomly selected 36 of the 72 offices to provide the JSA program and within these offices job seekers were randomly assigned to the JSA program. The randomization at two levels allows us to credibly estimate the overall effect of JSA. Whereas the direct effect is captured by comparing the treated and the non-treated at the active offices, displacement is captured by comparing the non-treated at the active and the non-active offices.

In line with the previous literature, we find that JSA reduces unemployment among the treated. This raises the question what explains the effectiveness of job search assistance. To study the mechanisms in detail, we use administrative data to follow the actions caseworkers take during the program, and pair this with information on search behavior obtained from monthly activity reports submitted by the job seekers. By exploiting data on vacancy referrals for both caseworkers and job seekers, we show that the key mechanism behind the positive effects of the JSA program is an increased number of vacancy referrals passed on from the caseworkers to the job seekers and an accompanying increase in the number of referrals the job seekers apply to. Simply put, the caseworkers use their expertise to find and point job seekers to suitable job openings, and the job seekers take advantage of this information by applying to the jobs they are referred to. We also show that the increased number of vacancy referrals does not lead to broader search in terms of occupations or geographical area, but rather streamlines the search process by helping the treated workers apply to the most relevant jobs early in the unemployment spell.[2]

We have also examined competing explanations. First, using data on all registered violations of the job search rules along with information on job applications, we find no evidence that caseworkers increase the monitoring of the job seekers and no corresponding impact on job seekers' search effort. Second, using information from individual action plans and program participation data along with information on search activities, we do not find that the treated job seekers receive more job search training and support, and we see no impact on job seekers' search strategies.

Besides facilitating efficient policy, this new evidence on mechanisms fills an important gap in the literature. Previous evaluations of JSA polices have contributed to the understanding of mechanisms by providing results for different types of interventions and target populations. For instance, Meyer (1995) and Ashenfelter et al. (2005) compared different policies, and found that assistance combined with monitoring produced desired results, whereas monitoring alone did not. However, there is less evidence showing in which way a specific intervention alters caseworker and job seeker behavior, and how such changes translate into positive employment effects.[3]

In addition to the positive direct effects, we find that the JSA program creates substantial displacement leading to higher unemployment for the non-treated. While the exit rate from unemployment for treated job seekers increases by 4.6 percent it falls by 3.8 percent for the non-treated, implying a smaller, although still positive, overall employment effect. We see no impact on wages, but indications of an increase in the number of posted vacancies in the local labor market. All this suggests that

[2]This adds to other studies on the role of vacancy referrals in the job search process. Examples include Van den Berg et al. (2019), Fougere et al. (2009), Engström et al. (2012) and Bollens and Cockx (2017).

[3]A recent exception is Arni (2015), who combines a randomized experiment with survey data to study behavioral mechanisms of an intensive counseling program targeted at older job seekers at two PES agencies in north-western Switzerland.

JSA is associated with equilibrium effects, which imply that the effectiveness of JSA documented in many previous studies is exaggerated. These results are consistent with the results from a small but growing literature on displacement effects.[4] Most notably, Crépon et al. (2013) provide novel experimental evidence of JSA provided to young, long-term unemployed college graduates in France. They find that the positive effects for the treated were smaller than the negative displacement effects for the non-treated, suggesting that more jobs were lost than found. We find more positive results in our experiment that targeted a more general group of all newly unemployed job seekers and offered JSA earlier in the unemployment spell. This suggests that the setting is important for the overall assessment of JSA policies. Other recent studies finding evidence of displacement effects include Ferracci et al. (2014), who develop methods to study displacement effects with non-experimental data, and Gautier et al. (2018), who use an equilibrium search model to study JSA in Denmark.

We complete the analysis on displacement effects by presenting evidence that discriminates between displacement due to resource constraints and displacement in the labor market. Separating between these two sources is central since the policy implications are different. While displacement due to resource constraints can be avoided by carefully considering the funding arrangements, displacement in the labor market is more challenging to address with policy arrangements. Still, this division has not been analyzed before. In our experiment, the intention was to give more assistance to the treated with unchanged support to the non-treated. However, since it is difficult in practice to control every feature of a policy program we cannot automatically rule out displacement of resources. By using information on resource allocation at the local office level, we show empirically that there is no crowding out of resources. Instead, we document substantially larger displacement effects in weak labor markets compared

[4] Earlier evidence on displacement include Blundell et al. (2004), Pallais (2014), Lalive et al. (2015) as well as Dahlberg and Forslund (2005) and Albrecht et al. (2009) for Sweden. Previous studies with similar two-level randomization designs as we use include Miguel and Kremer (2004), Banerjee et al. (2010) and Crépon et al. (2013).

to labor markets with many job openings. Taken together this suggests that the sizeable displacement effects are due to displacement of jobs. It also adds to the existing evidence that displacement can be limited under good labor market conditions.

To further contribute to the understanding of the mechanisms, we analyze whether the impact of JSA depends on the meeting format. Here, we exploit that the 36 active offices were randomly assigned to provide face-to-face meetings with a caseworker, distance meetings using the internet or telephone, or group meetings. Ultimately, policy makers are looking to allocate resources to interventions with the largest potential. We find positive employment effects for face-to-face and distance meetings, but not for group meetings.[5] This finding further supports the vacancy referral mechanism, since we see an increase in referrals for face-to-face and distance meetings, but not for group meetings. Our interpretation is that, since group meetings involve support to several job seekers at the same time, caseworkers are unable to discuss vacancies with each participant.

The evidence on the displacement of jobs implies that JSA programs create search externalities. Since the size of the externalities depends on the share of program participants, the reduced form estimates alone are insufficient to study the implications of a full-scale roll-out of the program. Such an assessment can be done by incorporating the estimated responses into a structural model. To do this, we build upon the Diamond-Mortensen-Pissarides (DMP) model in Gautier et al. (2018), which was designed to study the equilibrium effects of a Danish JSA program.[6] A key feature of the model is the endogenous matching function, which specifies that the success of an application

[5]Maibom et al. (2017) also find that face-to-face meetings outperform group meetings. However, in their case each treatment was given in only one region, whereas, in this paper, we have a design that explicitly allows for inter-treatment comparisons. Another recent study is Crépon et al. (2015), which finds positive employment effects of frequent group meetings in the form of search clubs with meetings several times a week.

[6]Gautier et al. (2018) also study displacement using a randomized trial, but in this case there is no randomization over local offices, only over unemployed individuals in two non-random Danish regions. With their equilibrium search model, they conclude that increasing the share of treated will raise equilibrium unemployment and decrease welfare.

depends on the number of applications sent by other workers, hence creating search congestion. One contribution we make is that we adjust the Gautier et al. (2018) model to fit the JSA policy evaluated in this paper.

Simulations using the estimated model show that a larger share of participants leads to a lower unemployment rate. Increasing the treatment share from 0 to 100 percent lowers the unemployment rate by around 0.2 percentage points, suggesting that the net effect of a full-scale roll-out is positive—despite the substantial displacement of jobs. The program has a small negative effect on government spending, since the reduction in benefit payments due to the lower unemployment rate is only slightly larger than the program costs. Welfare is decreasing in the share of participants, because the program implies lost non-market time, and because vacancy costs go up as the vacancy rate increases. In sensitivity analyses, we also consider a model with a delayed vacancy response to explore the fact that it may take time for firms to observe and react to the new market conditions. This delayed vacancy model predicts larger reductions of unemployment and government expenditures, and reverses the welfare effects to positive numbers.

Section 2 details the experiment, and Section 3 presents the data sources and the empirical strategy. Section 4 gives the main results for the program and displacement effects. In Section 5, we shed light on the mechanisms behind the direct effect of the JSA program. Section 6 compares the three types of meetings, and Section 7 investigates the origin of the displacement effects. Section 8 presents our equilibrium search model and reports the simulation results. Section 9 concludes.

2 The experiment

2.1 Randomization

The experiment took place during six months in 2015 (March–May and August–November). It involved 72 Public Employment Service (PES) offices, which corresponded to roughly one quarter of all offices in Sweden. The offices were selected in order to be a representative sample with respect to geographical dispersion and size. The target population consisted of all newly unemployed job seekers at the 72 offices, only exempting job seekers who had been unemployed in the last three months and newly arrived immigrants. Most local labor markets have one employment office. Thus, randomization over offices implies randomization over local labor markets, facilitating estimation of displacement effects. In the metropolitan areas with more than one office we selected offices where job seekers were less likely to compete for the same jobs, e.g., one office in the northern part and one office in the southern part of the city.[7]

For the 72 local offices, we applied a two-level randomization strategy over both offices and job seekers. Randomization over job seekers within active offices identifies the direct effect of the JSA program by comparing treated and non-treated job seekers in the same labor market. Randomization over offices identifies the displacement effects by comparing non-treated job seekers at active and non-active offices. To achieve a balanced sample of active and non-active offices we used stratified randomization. Based on a model developed by the PES, we divided the offices into sixfolds with similar economic and demographic conditions. Within each strata, we then randomized each office into different categories: one office was assigned face-to-face meetings, one distance meetings, one group meetings, and the remaining three constituted non-active offices. In total, this gave 12 offices per meeting format and 36 non-active offices that continued with the baseline services offered to all job seekers.

[7]We excluded the smallest local labor markets (monthly inflow less than 20 job seekers), since it would have been difficult to assist job seekers using group meetings in these offices.

The active offices randomly assigned 50% of the target population to the treatment group.[8] Since the randomization within the active offices was based on the job seekers' date of birth, we know the treatment assignment according to the treatment protocol. We use this theoretical treatment assignment to perform balancing tests in Table 1.[9] Columns 1 and 2 present group averages for treated and non-treated job seekers at the active offices, and Column 3 gives the sample statistics for job seekers at the non-active offices. Columns 4 and 5 show p-values for comparisons across the groups. The results are reassuring: there are no significant differences between the treated and the non-treated job seekers at the active offices, or between job seekers at active and non-active offices.

2.2 Treatment

Perhaps the most straightforward way to intensify job search assistance is to increase the number of meetings between job seekers and professional caseworkers (see, e.g., Graversen and van Ours, 2008a,b). The program used in this study more than doubled the meeting frequency during the first quarter of the unemployment spell. All extra meetings were mandatory[10], although only job seekers with unemployment benefits could be subject to sanctions if they did not show up when summoned. The active offices were compensated by central project-specific funding, with the intention to fully finance the increased costs from the program.

To compare different types of JSA, the experiment included three meeting formats:

[8]During the spring wave, the treatment intensity was randomly set to either 50 or 80%. Since take-up varied across offices with different treatment intensities we had insufficient power to use this difference in the analysis.

[9]All the estimates presented in the paper are weighted by the intention to treat share, i.e., the observed share of job seekers at the local office who would be randomized to treatment based on the treatment protocol. This corrects for the different shares in the spring (50 and 80%) and for random differences between the offices (e.g., one office having 48% and another having 52% treated for a treatment share of 50%).

[10]The exception was the distance meetings during the spring wave when we could not use telephone meetings due to security issues. This meant that job seekers who did not have access to a computer with internet connection and an e-ID could not participate.

Table 1: Balancing test of differences between treated and non-treated at active offices and between active and non-active offices

Variables	Treated T (1)	Non-treated C (2)	Non-active offices NA (3)	p-val diff T–C (4)	p-val diff TC–NA (5)
Age	33.33	33.39	33.54	0.707	0.637
Male	0.542	0.539	0.555	0.571	0.186
Unemployment benefits	0.642	0.638	0.634	0.546	0.747
Health disability	0.052	0.052	0.054	0.931	0.602
Matchable	0.868	0.862	0.864	0.117	0.936
Education level					
Less than high school	0.224	0.223	0.225	0.873	0.878
High school	0.491	0.493	0.486	0.833	0.690
College	0.285	0.285	0.289	0.932	0.844
Place of birth					
Sweden	0.678	0.667	0.641	0.059	0.399
Nordic countries	0.013	0.015	0.015	0.197	0.547
West Europe	0.036	0.034	0.036	0.256	0.857
Outside west Europe	0.273	0.284	0.308	0.035	0.385
Unemployment days					
Year t–1	30.66	30.43	30.98	0.767	0.777
Year t–2	67.42	68.49	68.25	0.449	0.914
Year t–3	69.57	71.83	71.22	0.133	0.868
Year t–4	63.82	63.68	66.14	0.921	0.438
Unemployment spells					
Year t–1	0.431	0.440	0.442	0.427	0.793
Year t–2	0.789	0.800	0.793	0.518	0.972
Year t–3	0.806	0.815	0.819	0.630	0.840
Year t–4	0.706	0.721	0.716	0.397	0.960
No. programs, last 4 yrs					
Labor market education	0.024	0.020	0.022	0.172	0.965
Preparatory education	0.048	0.047	0.047	0.869	0.983
Labor market training	0.027	0.030	0.031	0.389	0.370
Subsidized employment	0.106	0.108	0.106	0.711	0.920
Observations	14,075	12,463	31,240	26,538	57,778

Notes: Summary statistics by treatment status, weighted by the observed intention to treat share. Standard errors in column 5 are clustered at the PES office level. *Unemployment benefits* is an indicator variable for collecting unemployment benefits, *Health disability* is an indicator variable for having a functional disability and *Matchable* is an indicator variable from an initial assessment about the job seeker's potential to take a job on short notice.

individual face-to-face meetings, individual distance meetings and group meetings. The face-to-face meetings and the distance meetings were similar in many respects. They added three extra meetings to the baseline services, which included a short meeting when the job seeker registered, followed by a longer planning meeting, i.e., about two meetings within the first quarter. The extra meetings focused on personalized job search assistance, but we did not provide any detailed instructions about the content of the meetings. Instead, it was up to the caseworkers to offer assistance according to the needs of each job seeker. The basic idea with offering distance meetings—online or via telephone—was that this could create greater flexibility, both for the caseworkers and for the job seekers.

In contrast, the group meetings used a more detailed protocol. Since the meetings gathered around 10–15 job seekers at once they were given in the form of seminars, where each occasion concerned a specific topic, such as CV writing, interview training, and advice for creating professional networks. The instruction was that job seekers and caseworkers should meet frequently in an initial stage, with five seminars over the first two weeks of unemployment. Thereafter, the participants were divided into smaller groups, which were supposed to meet on their own on a weekly basis during two months (not visible in our data).

Table 2 looks at the different treatments given within the experiment. It shows the intention-to-treat comparison of treated and non-treated job seekers (according to the treatment protocol) at the active offices, for the full sample and for each of the three meeting formats. We use information about all registered contacts between the job seeker and the caseworker in the PES administrative registers, which covers all job seekers. We distinguish between *physical meetings*, which include face-to-face meetings and group meetings, and *distance meetings*, which include contacts over telephone and online using an e-ID. *All meetings* are the sum of these two.

Panel A describes program participation. During the first registration meeting,

Table 2: Program participation and meetings in active offices

	All meeting types (1)	Face-to-face meetings (2)	Distance meetings (3)	Group meetings (4)
Panel A: Program participation				
Informed about the program	0.619***	0.601***	0.621***	0.643***
	(0.004)	(0.007)	(0.008)	(0.008)
At least one program meeting	0.230***	0.265***	0.247***	0.164***
	(0.004)	(0.006)	(0.007)	(0.006)
Panel B: Number of meetings				
All meetings quarter 1	0.500***	0.414***	0.418***	0.707***
	(0.026)	(0.039)	(0.046)	(0.052)
	[3.159]	[3.150]	[3.302]	[3.019]
All meetings quarter 2	−0.017	0.002	−0.034	−0.024
	(0.021)	(0.033)	(0.039)	(0.039)
	[1.176]	[1.152]	[1.247]	[1.131]
Panel C: Type of meeting				
Physical meetings quarter 1	0.318***	0.376***	-0.015	0.596***
	(0.021)	(0.032)	(0.035)	(0.043)
	[2.262]	[2.287]	[2.331]	[2.155]
Distance meetings quarter 1	0.182***	0.038*	0.433***	0.112***
	(0.015)	(0.022)	(0.030)	(0.027)
	[0.897]	[0.863]	[0.971]	[0.864]
Panel D: Time pattern of meetings				
All meetings month 1	0.193***	0.120***	0.155***	0.335***
	(0.014)	(0.021)	(0.024)	(0.030)
	[2.115]	[2.097]	[2.176]	[2.076]
All meetings month 2	0.204***	0.162***	0.144***	0.324***
	(0.013)	(0.019)	(0.021)	(0.026)
	[0.584]	[0.585]	[0.641]	[0.523]
All meetings month 3	0.104***	0.132***	0.119***	0.048***
	(0.011)	(0.017)	(0.020)	(0.019)
	[0.460]	[0.468]	[0.486]	[0.420]
Observations	26,538	10,567	8,259	7,712

Notes: The results are from a linear regression of each variable on a treatment indicator, weighted by the observed intention to treat share. The sample includes the active PES offices during the experiment period. Standard errors in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1. Face-to-face meetings include visits, Distance meetings include contacts over telephone or online using an e-ID, and All meetings are the sum of the two, all based on administrative records from the PES.

the caseworkers were instructed to use an experiment-specific tool that revealed the treatment status of the worker (based on date-of-birth), and then inform the treated job seekers about the program.[11] The first row of Panel A shows that this tool was used for 62% of the job seekers in the treatment group. The main explanations to the take-up rate was that individual caseworkers failed to use the experiment-specific tool and that some offices failed to correctly capture the target population. However, since we observe the entire target population and for whom the experiment-specific tool was used, we have full control over how the experiment was implemented. The second row of Panel A shows that 23% of the target population participated in at least one program specific meeting. Information about the JSA program specific meetings were collected from the written acts at the PES. The fraction who initiated treatment was highest for face-to-face meetings (27%), and lowest for group meetings (16%). Reasons for not initiating treatment was, for example, caseworkers failing to offer the extra meetings, job seekers finding a job before the first meeting, and job seekers simply not showing up for the extra meetings.

Panels B–D of Table 2 show that the impact of the program on the frequency, type and time pattern of meetings was consistent with the treatment protocol. Panel B shows that the treated job seekers received on average 0.5 more meetings, which, given the take-up rate of 23%, corresponds to about two more meetings for those who actually showed up (not adjusted for early exits).[12] We also see that the meeting frequency increased during the experiment period (quarter 1) but not after the experiment period (quarter 2), and that the increase was similar for face-to-face and distance meetings and slightly larger for group meetings. Panel C shows that job seekers assigned to face-to-face meetings and group meetings received significantly more physical meetings,

[11]Participants in the group meetings were informed about all five meetings immediately, while participants in the face-to-face and distance meetings typically were summoned by caseworkers to one meeting at the time.

[12]This figure is lower than the intended number of meetings, which was three for the face-to-face and the distance meetings and five for group meetings. However, given that a substantial fraction of the job seekers found a job within three months, this is consistent with the treatment protocol.

whereas job seekers assigned to distance meetings received more distance meetings.[13] Finally, Panel D shows that face-to-face and distance meetings were fairly evenly distributed over the first three months of the unemployment spell, while group meetings were more concentrated to the first two months.

3 Data and empirical strategy

3.1 Data

Our analysis benefits from access to rich data. Here, we provide an overview of the data records and then describe the data in more detail in the analyses. From the Swedish PES we have information on individual characteristics as well as daily records of unemployment status for all job seekers. The PES data also contains information on all meetings between the caseworkers and the job seekers. We have access to wage data on the full-time equivalent monthly wage rates from Statistics Sweden and detailed vacancy data from the PES on the number of posted vacancies per municipality and month.

Detailed information about caseworkers' actions and job seekers' search behavior allow us to study the mechanisms behind the effects of the JSA program. For caseworker actions, we use several administrative data sources collected by the PES. First, to study monitoring of job seekers, we use data on all registered violations of the job search rules. Second, to study the provision of support and training, we use information from individual action plans specifying all types of support given to each job seeker, along with daily records of participation in ALMPs. Third, to analyze how caseworkers communicate information about vacancies, we use data on vacancy referrals from the caseworkers to the job seekers. Finally, we use meeting records for each caseworker to

[13]All treatment assignments were associated with more distance meetings, but the association was by far the strongest for the distance meetings group. Among other things, distance meetings include all telephone contacts between job seekers and caseworkers.

study any reallocation of resources from treated to non-treated job seekers.

We are also among the first to use data on job seeker search behavior from monthly activity reports. Since September 2013, job seekers in Sweden are required to submit a monthly report on the number and type of all job search activities to the PES (e.g., job applications and search channels). Failure to provide a report, or submitting a report indicating too low level of search activity, may lead to a benefit sanction. Moreover, for each job application, the job seekers have to report the name and telephone number of the firm, and the occupation to which they apply, so that caseworkers can validate the information in the reports. Therefore, the activity report data should be a reliable data source for measuring job search behavior.

3.2 Empirical strategy

We first analyze the data the way it is usually done in evaluations of experiments: we utilize the randomization over job seekers within offices and compare the outcomes of treated and non-treated job seekers at the active offices. To do this, we assign treatment status according to the treatment protocol of the experiment, and estimate:

$$Y_i = \alpha_0 + \beta_0 1(\text{Assigned to program}_i) + \varepsilon_i, \tag{1}$$

where our main parameter of interest is the intention-to-treat (ITT) effect of the JSA program, β_0. We then estimate displacement effects by exploiting the randomization over offices. Here, the model for individual i in office j is:

$$Y_{ij} = \alpha_1 + \beta_1 1(\text{Assigned to program}_{ij}) + \beta_2 1(\text{In a program area}_j) + \varepsilon_{ij}, \tag{2}$$

where $1(\text{In a program area}_j)$ indicates an active office and $1(\text{Assigned to program}_{ij})$ indicates being in the treatment group. The displacement effect is given by β_2, which is identified through the comparison of non-treated job seekers at active and non-active offices. β_1 captures the direct comparison of the treated and the non-treated at the active offices, and the net effect for the treated job seekers, compared to non-treated job seekers at the non-active offices, is given by $\Delta \equiv \beta_1 + \beta_2$. The overall effect takes the share of treated into account and captures the average effect across both treated and non-treated job seekers at the active offices compared to job seekers at the non-active offices.

The analysis focuses on the ITT effect of the JSA program, but we will also report instrumental variables (IV) estimates of the effect of program participation. One reason is that the structural model is set up for program participants. Since job seekers may react already to information about the program and such responses can be considered a program effect, we take a cautious approach and define program participants as those who were supposed to be informed about the program (first row of Table 2). However, since only a fraction of the program participants showed up for an extra meeting (second row of Table 2) our IV estimates do not reflect the effect of actual take-up of meetings. In that sense, they are conservative.

To obtain the direct program effect, we instrument program participation with the program assignment indicator, $1(\text{Assigned to program}_{ij})$, and estimate:

$$Y_{ij} = \alpha_2 + \beta_3 1(\text{Program participation}_{ij}) + \beta_4 1(\text{In a program area}_j) + \varepsilon_{ij}, \qquad (3)$$

where β_3 reflects the direct program effect at the active offices. The interpretation of this LATE effect depends on how representative program participants are compared to the target population as a whole. Table A-1 in the appendix shows that the program participants are similar to the target population in terms of observable characteristics, which suggests that the LATE may be similar to the average program effect. This is expected since the dropout mainly occurred at the caseworker and the office level, and was not due to individual job seekers selecting in or out of the program, as described in Section 2.2.[14] To estimate the net program effect for the participants, we follow the IV approach in Crépon et al. (2013).[15]

4 Effects of the job search assistance program

4.1 Unemployment

We begin with a visual inspection of the direct ITT effect of the program. As shown in Figure 1 there is a striking difference between treated (assigned to the JSA program) and non-treated (assigned to the control group) at the active offices: the unemployment rate is lower for the treated than for the non-treated. The effect of the program reaches its maximum of about three percentage points around the time when treatment stops, after 3–4 months.[16] The immediate effect of the program is expected since the intensive caseworker assistance aims at helping job seekers leave employment as fast as possible. Hence, in contrast to, for instance, training programs there are no lock-in effects. The figure also shows that even though the effect decreases after treatment ends, it is still significant until the tenth month since inflow to unemployment.

We next estimate treatment effects in a regression framework using three outcomes: (1) the probability of leaving unemployment during the first quarter of unemployment, and the number of days registered as unemployed during (2) the first quarter and (3)

[14]Not surprisingly, the picture is different for the job seekers who attended at least one extra meeting. Compared to the full target population, they receive unemployment benefits more often, are disabled to a lesser extent, are perceived as matchable to a higher degree, have a higher education level and are natives to a larger extent. On the other hand, they have more extensive unemployment history, with somewhat more days in unemployment over the last four years.

[15]See Crépon et al. (2013) for details. Here, the IV scaling takes into account that the compliers experience positive program effects, whereas the non-compliers experience negative displacement effects. The main identifying assumptions is that the displacement effects are uncorrelated with treatment status. In practice, this means that compliers and non-compliers on average should have similar potential outcomes under no treatment. In our case, the compliers and the non-compliers have similar observed characteristics, lending support to this assumption.

[16]Since the effect appears early in the unemployment spell, it may reflect pre-program responses. The median number of days until the first program meeting is 29 days and the direct effect during the first month is relatively small, so that pre-program responses are unlikely to be the main explanation to the observed pattern.

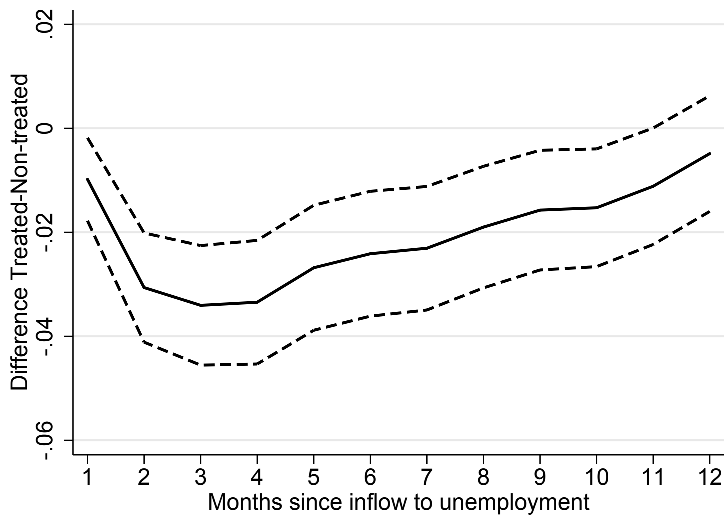


Figure 1: Difference in the share of unemployed between treated and non-treated job seekers at the active offices, by months since inflow to unemployment

the first year, where the last two measures include re-unemployment after a period of employment. Unemployment includes full-time unemployment and participation in an active labor market program. Panels A–C in Table 3 show the results for each of the three outcomes. The first two columns of the table, where we report the direct ITT effect, β_0, based on equation (1), confirm the graphical evidence from Figure 1. The JSA program increases the exit rate from unemployment by 3.5 percentage points, or about 10% (column 2 with individual control variables). We also see that the treated have 1.5 fewer days of unemployment during the first quarter (Panel B), and 5.9 fewer days of unemployment during the first year (Panel C).

Since other interventions for unemployed job seekers previously studied in the literature differ in many respects, such as content, intensity, target population, labor market situation and evaluation horizon, it is difficult to compare effect sizes across studies. We note that the direct effect in our study is somewhat smaller than effects from experiments evaluating JSA in the form of early meetings in Denmark (Graversen and van Ours, 2008b; Gautier et al., 2018). This is expected, however, since the Danish programs were more intensive.

The findings in columns 1 and 2 suggest that the JSA program successfully decreased time in unemployment. However, the overall effectiveness of the program ultimately depends on the degree of displacement. If the positive effect of job search assistance comes at the expense of non-treated job seekers there is reason to question the benefits of the policy. We therefore turn to models that estimate both direct and displacement effects, and report estimates from equation (2) using data from both active and non-active offices (Column 3 of Table 3). As expected, the direct effect is similar to the estimates above. Thus, estimating the direct effect using β_0 from equation (1) or β_1 from equation (2) lead to similar results.

Turning to the displacement effect in column 3, the estimate of being in a program area indicates sizeable displacement, but the precision is low. To increase precision, we therefore add data from time periods prior to the experiment, back until year 2012.[17] This strategy is illustrated in Figure 2. Each line in the figure compares the number of unemployment days during the first year between two groups of job seekers. The darker grey line compares the treated and the non-treated at the active offices, and the lighter grey line compares non-treated job seekers at active and non-active offices (by calendar month of inflow to unemployment). The dashed vertical lines, finally, indicate the two experiment periods. Before the experiment there is no systematic pattern in the data—the time series are noisy but stay around zero. In contrast, during the two experiment periods the series diverge. Unemployment decreases for the treated relative to the non-treated at the active offices, and, at the same time, unemployment increases for the non-treated at the active offices relative to the non-treated at the non-active offices. This pattern gives a clear indication that the JSA program caused displacement effects.

These displacement effect patterns are supported by the regression estimates in

[17] We include unemployed job seekers during the entire period 2012–2015 using the same sample restrictions for the target population as during the experiment in 2015. We use data beginning in 2012 since several reforms were introduced between 2011 and 2012, including public investments to increase the number of caseworkers, and a new mandate for the PES to provide services to immigrants.

Table 3: Direct and displacement effects of the JSA program on unemployment

	Experiment period			Pre-data, 2012–2015		
	ITT	ITT	ITT	ITT	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Exit unemp. 1st quarter						
Assigned to program	0.036***	0.035***	0.035***	0.035***	0.034***	
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	
Program participant						0.055***
						(0.010)
In a program area			−0.020	−0.016**	−0.015**	−0.015**
			(0.014)	(0.007)	(0.006)	(0.006)
Net effect treated[1]			0.014	0.019***	0.018***	0.039***
			(0.013)	(0.006)	(0.005)	(0.010)
Control mean	0.354	0.354	0.368	0.390	0.390	0.390
Panel B: Unemp. days 1st quarter						
Assigned to program	−1.574***	−1.502***	−1.512***	−1.505***	−1.456***	
	(0.318)	(0.310)	(0.328)	(0.399)	(0.353)	
Program participant						−2.352***
						(0.570)
In a program area			1.158	0.725**	0.695**	0.695**
			(0.731)	(0.344)	(0.318)	(0.318)
Net effect treated[1]			−0.355	−0.780**	−0.761**	−1.658***
			(0.755)	(0.334)	(0.326)	(0.552)
Control mean	74.37	74.37	73.51	73.78	73.78	73.78
Panel C: Unemp. days 1st year						
Assigned to program	−6.692***	−5.893***	−5.944***	−6.602***	−5.991***	
	(1.527)	(1.414)	(1.296)	(1.778)	(1.359)	
Program participant						−9.679***
						(2.195)
In a program area			7.627*	4.318**	4.160**	4.160**
			(4.504)	(1.912)	(1.727)	(1.727)
Net effect treated[1]			1.683	−2.284	−1.831	−5.519
			(4.478)	(1.612)	(1.484)	(2.936)
Control mean	196.1	196.1	190.7	187.0	187.0	187.0
Year dummies	No	No	No	Yes	Yes	Yes
Month dummies	No	No	No	Yes	Yes	Yes
PES office dummies	No	No	No	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Clusters	No	No	72	72	72	72
Observations	26,538	26,538	57,778	552,816	552,816	552,816

Notes: Regression of each outcome variable on an indicator for active PES office ("In a program area") and an indicator for active PES office × intention to treat status is treated ("Assigned to program"). The controls include the variables in Table 1. Standard errors in parentheses are clustered at the PES office level. *** p<0.01, ** p<0.05, * p<0.1.

[1] Net effect in column (6) is for program participants.

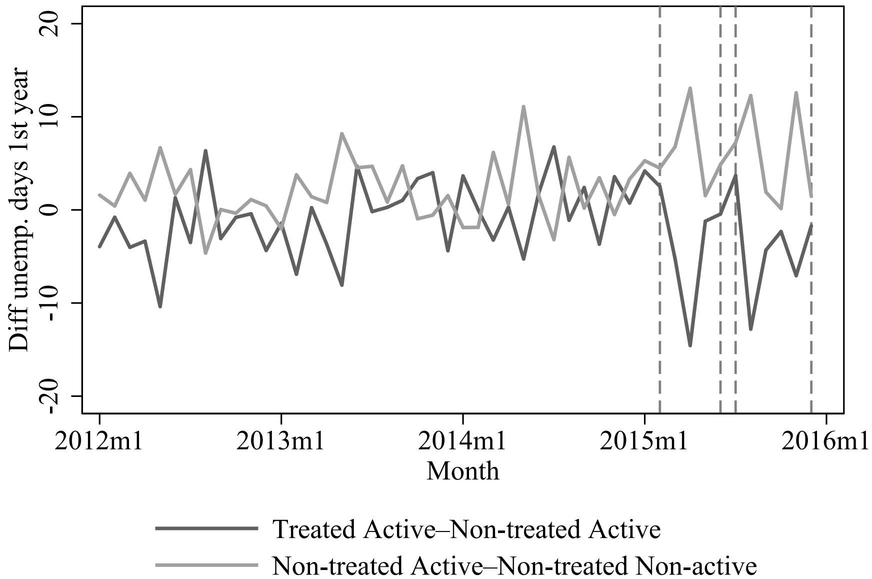


Figure 2: Differences in days in unemployment during the first year since registration between treated and non-treated job seekers in active offices, and between non-treated job seekers at active and non-active offices

columns 4–6 of Table 3. Here, we interact the variables *Assigned to program* and *In a program area* in equation (2) with an indicator for becoming unemployed during the *Experiment period*, and adjust for office fixed effects and calendar time (year and month dummies). This strategy of using pre-experiment data to capture office level heterogeneity gives a similar direct effect and a slightly lower displacement effect compared to using only data from the experiment period in column 3, but due to the increased precision the displacement effects are now significant.

Our preferred estimates in column 5 (with individual characteristics) suggest that the program reduces the exit rate for the non-treated job seekers at the active offices by 1.5 percentage points (3.8%) during the first quarter of unemployment, a finding that is consistent with substantial displacement effects of the JSA program. The net effect for treated job seekers, $\beta_1 + \beta_2$, is 1.8 percentage points, or 4.6%. The overall effect, taking both the impact on the treated and the non-treated job seekers at the active offices into account, depends on the share of treated job seekers which was roughly 50%. Thus, since the positive net effect for the treated is larger than the negative displacement effect for the non-treated, the overall effect of the program is positive. On average, across treated and non-treated at the active offices, the JSA program increases the job finding rate by 0.25 percentage points or 0.7%.[18]

Finally, by construction, the IV estimates in column 6 are larger than their ITT counterparts in column 5, with a net program effect corresponding to a 3.9 percentage point (10%) increase in the exit rate during the first quarter. The results for days in unemployment during the first quarter and the first year in Panels B and C of Table 3 display similar patterns.

Summing up, the JSA program reduces unemployment among the treated job seekers, but also leads to large displacement effects for the non-treated job seekers. This is similar to the results from the two-level randomized experiment reported in Crépon et al. (2013). In their case, the net effect for the treated is close to zero, insignificant, and smaller than the displacement effects for the non-treated, which implies that more jobs were lost than found. Hence, we find more positive overall effects of JSA. This may be due to the more general target population (all newly unemployed job seekers with no unemployment during the last 3 months) compared to Crépon et al. (2013) (young college graduates unemployed for at least six months), or that JSA was offered earlier during the unemployment spell. This strengthens the importance of studying displacement of JSA policies in different settings, to get a deeper understanding of when and why displacement arises.

4.2 Direct and displacement effect heterogeneity

The two-step randomization setup allows us to credibly identify displacement effects and, hence, the overall effect of the program for the target population. However, this ignores displacement for non-treated job seekers *outside* the target population, such as

[18]The share of treated is 53%, so that the overall effect is 0.53*0.18 (effect for the treated)+0.47*-1.5 (effect for the non-treated)=0.25 percentage points.

the stock of job seekers that entered unemployment before the experiment, the inflow of unemployed during the summer months when no one was assigned to the program, and the inflow of unemployed during the experiment period that did not belong to the target population. It can be questioned whether these groups compete for the same jobs as the target population. Both the stock of unemployed at the beginning of the experiment period and the inflow of unemployed outside the target population during the experiment period (newly arrived immigrants and job seekers with a recent unemployment spell) are likely to be further from the labor market to begin with. The inflow during the summer months, on the other hand, mainly consists of short-term unemployed, such as students looking for a job during the summer break.

Table A-2 in the appendix, nevertheless, presents estimates of the displacement effects for these groups outside the target population. The first three columns study the impact on individuals in the target population who entered unemployment before the experiment, during the summer months, or after the experiment in 2015. The results indicate that these groups may be somewhat affected, but substantially less than the non-treated job seekers in the target population during the experiment. The last three columns study the impact on individuals who became unemployed during the experiment period but did not belong to the target population, who appear to be unaffected by the program. Overall, these findings speak against any large displacement effects of the program for groups outside the target population.

The existence of displacement effects in our experiment also raises the important question of *who* gains and *who* loses. From a welfare perspective we may accept displacement if job search assistance benefits job seekers who are less attached to the labor market at the expense of those who are closer to finding a job. Table A-3 in the appendix presents estimates by education, country of origin, unemployment history, and gender. Since the analysis reduces the sample sizes dramatically, most differences across sub-groups in the table are statistically insignificant. Still, overall, the results in columns 1–6 indicate that the least attached groups of job seekers benefit the most from the JSA program, in particular when considering the impact over the entire first year. Estimates of the direct effect are larger for low educated, foreign born and job seekers with long unemployment history (above median number of unemployment days in the last four years). The displacement effects, on the other hand, are more evenly distributed across groups. We finally note some interesting gender differences; men benefit more than women in terms of direct effects, but also suffer more from displacement (columns 7 and 8).

4.3 Wages and vacancies

The positive effect on job finding in our experiment could cause demand side responses with effects on wages and the number of vacancies created by firms in the market. Any such effects are important determinants of the market level unemployment rate in the new equilibrium. To find out how job search assistance affects wages we estimate the effect on the log wage rate for the first job after unemployment, using data on the full-time equivalent monthly wage rate from Statistics Sweden.[19] The results in Table 4 reveal no significant effects on wages neither for the treated nor for the non-treated at the active offices. These results are also informative for the impact of JSA on job quality. On the one hand, we may expect that professional support from caseworkers, with expert knowledge on the local labor market, would help job seekers to find higher-quality jobs or improve the matching between job seekers and employers. On the other hand, if job seekers feel pressured to exit unemployment early they may accept jobs of lower quality or worse matches with employers.[20] Notably, neither of these hypotheses are supported by our wage estimates.

To measure the vacancy effect, we exploit disaggregated data on all vacancies posted

[19]The data cover roughly 50% of all jobs, including the entire public sector, large private firms and a sub-sample of small private firms. There is no difference across treated and non-treated job seekers with respect to the sampling scheme (conditional on finding a job). Since treated job seekers find jobs

Table 4: Effects of the JSA program on log wages

	ITT (1)	IV (2)
Assigned to program	−0.002	−0.003
	(0.004)	(0.006)
In a program area	0.000	0.000
	(0.005)	(0.005)
Net effect treated	−0.001	−0.002
	(0.003)	(0.008)
Control mean	10.09	10.09
Observations	237,598	237,598

Notes: Outcome is the log monthly wage on an indicator for active PES office ("In a program area") and an indicator for active PES office × intention to treat status is treated ("Assigned to program"). The regressions include year dummies, month dummies, PES office dummies and the control variables in Table 1. The net effect in column 2 is for the program participants. Standard errors in parentheses are clustered at the PES office level. *** p<0.01, ** p<0.05, * p<0.1.

at the PES per municipality and month over the period 2012–2015.[21] For each local labor market (office) and month, we calculate the average number of vacancies facing the target population, based on their municipality of residence. We use the log of vacancies as outcome, and estimate a similar model as for the other outcomes above. The main difference is that we now have one outcome measure per office and month. This means that we focus on the program area dummy, which takes the value one for the active offices during the experiment period. Since the size of the local labor markets varies, we use both weighted (by size of target population in each office) and unweighted regressions.

The vacancy estimates in Table 5 show some interesting patterns. Column 1 indicates that the JSA program increased the number of posted vacancies by 3.3% during the experiment, but the effect is insignificant. Column 2 allows for different responses during the spring and the fall wave of the experiment. The rationale for separating between the immediate and somewhat longer run is that it may take time for firms to

faster we observe wages for a slightly larger share of treated compared to non-treated job seekers.

[20]Cottier et al. (2018) provide recent evidence from Switzerland suggesting that JSA may push job seekers into jobs of lower quality.

[21]Not all Swedish firms post vacancies at the PES. Firms hiring high-skilled workers, e.g., engineers, are less likely to use this channel. However, the vacancies posted at the PES should capture those that are relevant for our target population.

observe and react to the new market in the wake of the JSA program. This is supported by the data. Whereas vacancies are unaffected during the spring (the point estimate is virtually zero), we find a rather sizeable effect during the fall, which is significant at the ten percent level with the unweighted data (column 4). We return to this delayed vacancy pattern when we estimate the equilibrium search model in Section 8.

Table 5: Effects of the JSA program on vacancies

	Weighted		Unweighted	
	(1)	(2)	(3)	(4)
In a program area	0.033		0.048	
	(0.033)		(0.035)	
In a program area×Spring period		−0.002		0.018
		(0.033)		(0.043)
In a program area×Fall period		0.066		0.076*
		(0.041)		(0.039)
Control mean	5.896	5.896	5.896	5.896
Observations	16,269	16,269	16,269	16,269

Notes: Regression of the log number of vacancies facing the target population in each office and month on an indicator for active PES office ("In a program area"), year dummies, month dummies and PES office dummies. Weights are determined by the size of target population per office and month. Standard errors in parentheses are clustered at the PES office level. *** p<0.01, ** p<0.05, * p<0.1.

5 Why does the JSA help the unemployed?

We have shown that JSA, in the form of more meetings with a caseworker at the local employment office, has a positive direct effect on exit from unemployment. An important question is how the direct effect of the JSA program arises. While many previous studies have documented positive effects of JSA and monitoring policies (see, e.g., the overview by Card et al., 2017), the evidence on the driving mechanisms behind the effects is still scarce. In particular, we know little about how JSA alters caseworkers' and job seekers' behavior, and how this operates to lower unemployment. This is unfortunate since a better understanding of the underlying mechanisms facilitates efficient policy.

In this section, we analyze why the positive effect of the JSA program in our experiment arises. To give a complete picture of what drives the results we study both the actions taken by the caseworkers and the accompanying reactions by the job seekers. The richness and the quality of the data allow us to study mechanisms in detail. Information about caseworker actions is obtained from several different administrative records gathered by the PES. The information on search behavior comes from activity reports filed by the job seekers every month. The reports provide information about all search activities during the past month. As mentioned in Section 3.1, the job seekers risk benefit sanctions if they fail to provide a report and caseworkers can validate the job applications in the report.

The direct effect of job search assistance may arise through a number of possible channels. We broadly divide these mechanisms into three strains. First, caseworkers may use the more frequent meetings to increase the monitoring, so that they detect more violations of the rules that the job seekers should follow (Graversen and van Ours, 2008a,b). From a job seeker perspective, tighter monitoring should lead to increased job search effort (more job applications).[22] Second, the JSA program may give caseworkers more time to provide job search training and related support. Caseworkers may, for instance, use the meetings to help job seekers write better CVs and job applications, prepare them for interviews, or provide valuable information about job search strategies. Any intensified job search training may cause the job seekers to alter their search strategies, perhaps by promoting other channels besides formal applications, which in turn may lead to faster job finding.[23] Both monitoring and support have been in focus in some previous studies (Meyer, 1995; Ashenfelter et al., 2005; Van den Berg and Van der Klaauw, 2006). We contribute to this literature by presenting evidence that builds on a large-scale randomized trial combined with detailed register data on caseworkers' actions and job seeker search behaviour.

[22]Tighter monitoring may also affect reservation wages. However, the results in Section 4.3 reveal no wage effects for our experiment.

[23]Previous studies showing that labor market policies can affect search strategies include Van den Berg and Van der Klaauw (2006) and Bonoli et al. (2014).

A third possible channel that has been discussed to a lesser extent is that caseworkers can provide information about relevant vacancies. Vacancy referrals may lead to broader job search, both in terms of geography and in terms of occupation. They may also help the job seekers pinpoint the most relevant job openings, without changing how broad the search is. Both explanations suggest that vacancy referrals can speed up the exit from unemployment. To study this mechanism, we exploit data from the administrative registers on the number of vacancy referrals provided by the caseworkers. We pair this with data on job seekers' search behaviour to study to what extent the job seekers take advantage of the referrals.

Concerning data details, we focus on the first quarter of unemployment, which is the period during which the JSA program took place.[24] For each outcome, we estimate the model in equation (1), so that the reported coefficients represent the difference between the treated and the non-treated job seekers at the active offices during the experiment in 2015. One concern is that the extra meetings affect the likelihood to file an activity report, for instance, if the caseworkers push the treated job seekers to submit reports. However, as shown in Panel A of Table 6 there is no difference between the treated and the non-treated in the propensity to report.

Panel A of Table 6 shows differences between treated and non-treated job seekers with respect to monitoring and search effort. In Sweden, caseworkers are responsible for all monitoring of job search activities. If caseworkers observe a violation of the job search rules, they should notify the unemployment insurance funds, which then decide about benefit sanctions.[25] Here, we use data on these registered violations of

[24]We focus on activity reports of individuals who are still unemployed when the report is supposed to be submitted. We exclude job seekers who, according to the PES registers, are not supposed to report their activity. Data on caseworker actions include information for the entire target population.

[25]Benefit sanctions are monetary fines (suspension of unemployment insurance benefits). Refusal of job offers, insufficient job search, not showing up for meetings with PES, not applying for assigned jobs, and job quits may lead to a sanction. The size of the sanction depends on the type of violation, but the general rules are that the first violation leads to a warning; the second to fourth violations imply suspension for one day, five days, ten days, respectively; and the fifth violation leads to a permanent suspension.

the rules. Initially, column 1 shows that the total number of registered violations is higher for the treated job seekers. However, when we separate between different types of violations, it is only violations due to failure to show up for meetings that are affected (column 2). This difference between treated and non-treated job seekers is expected: since the intensified JSA implies more meetings, we expect a mechanical effect here. In contrast, violations regarding insufficient job search effort (column 3) are unaffected, suggesting that the caseworkers' monitoring of search effort did not change. Other types of sanctions are also unaffected (not reported).

The first three columns of Panel A focused on caseworkers' actions. Next, we turn to the corresponding changes to job seekers' search behavior. Since the treated job seekers do not face more intensive monitoring, we do not expect to see any difference in terms of job search effort. This is also supported by the data. There are no differences in terms of the total number of activities, or the total number of job applications (columns 6–7). We conclude that increased search effort—induced by a higher degree of monitoring—hardly explains the observed positive direct effect of the JSA program.

Next, Panel B looks at differences with respect to job search support and training. One of the caseworkers' initial tasks after an individual has registered at the PES is to set up an action plan. The plan constitutes a mutual agreement between the PES and the job seeker, and specifies both the type of support and training that the PES commits to offer, and what actions the job seeker should undertake to find a job. The action plan consists of up to eight different support categories and can be revised as the job search continues.[26] Columns 1–4 of Panel B show no difference between the treated and non-treated with respect to support and training. The probability of having an action plan is the same across groups. Both the total number of support categories in the action plan and the number of categories that specifically capture job

[26]The categories are: Search for jobs, Improve your search, Guidance to work, Education to work, Start new business, Clarify your qualifications for work, Adapt your work situation, and Work preparatory measures. We define Improve your search and Guidance to work to be categories that specifically capture job search training.

Table 6: Effects of the JSA program on caseworker actions and job seeker search behavior

Panel A:	**Caseworker action: Monitoring**				**Search behavior: Search effort**		
	Total violations	Violations absence meeting	Violations job search effort		Prob. reporting	Total activities	Total job applications
	(1)	(2)	(3)		(5)	(6)	(7)
Assigned to program	0.037***	0.035***	0.000		−0.003	0.152	0.032
	(0.011)	(0.005)	(0.001)		(0.006)	(0.337)	(0.249)
	[0.337]	[0.059]	[0.010]		[0.520]	[14.46]	[7.811]
Obs.	19,674	19,674	19,674		19,674	11,959	11,959
Panel B:	**Caseworker action: Job search support and training**				**Search behavior: Search channels**		
	Has action plan	Support cat. in plan	Job search supp. cat. in plan	ALMP participant	Unsolicited job applications	Other job-enhancing activities	
	(1)	(2)	(3)	(4)	(5)	(6)	
Assigned to program	−0.010	0.002	0.011	0.005	−0.140	0.266*	
	(0.006)	(0.015)	(0.009)	(0.004)	(0.111)	(0.140)	
	[0.752]	[1.520]	[0.285]	[0.082]	[2.546]	[3.417]	
Obs.	19,674	14,658	14,658	19,674	11,959	11,959	
Panel C:	**Caseworker action: Vacancy referrals**				**Search behavior: Applications to vacancy referrals**		
	Total	Suggested	Mandatory		Total	Suggested	Mandatory
	(1)	(2)	(3)		(5)	(6)	(7)
Assigned to program	0.112**	0.079*	0.034**		0.101***	0.071***	0.029***
	(0.048)	(0.044)	(0.016)		(0.026)	(0.024)	(0.007)
	[1.718]	[1.579]	[0.139]		[0.460]	[0.427]	[0.033]
Obs.	19,674	19,674	19,674		11,959	11,959	11,959

Notes: Results from a linear regression on a treatment indicator, weighted by the observed ITT-share. All outcomes measured during the 1st quarter of unemployment. Standard errors in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

search training are unaffected. Finally, we see no difference regarding the probability of participating in an active labor market program.

Turning to the job seekers' search behavior (columns 5 and 6) there is no trace of any impact on job seekers' search channels: column 5 shows no effect on the probability of filing unsolicited job applications, and column 6 shows only a small effect on the likelihood of reporting other job-enhancing activities. Summing up, the evidence in Panel B speaks against an increased amount of job search support and training as an important channel.

Panel C looks at whether the job seekers who participated in the JSA program became better informed in terms of which vacancies to apply to. We distinguish between two types of vacancy referrals, suggested job openings and mandatory referrals. The mandatory referrals are mainly used when caseworkers believe that there is a good match between the vacancy requirements and the job seeker's skills. They are only used for individuals collecting unemployment benefits, who risk benefit sanctions in case of insufficient job search. For suggested vacancies, the job seeker is free to choose whether to apply or not. They include both vacancies posted at the online PES job board and job openings communicated from employers to the PES without being posted as formal vacancies. Columns 1–3 show that the JSA program raised the total number of vacancy referrals passed on from caseworkers to job seekers by 0.11 or 6.5%. This is due to both more suggested job openings (up 5%) and more mandatory referrals (up 25%). Below we show that this increase is not due to displacement of resources with fewer referrals given to the non-treated.

We next study if job seekers reacted to the information provided by the caseworkers. Here, columns 5–7 show that the job seekers take full advantage of the vacancy referrals. The number of applications to referred vacancies increases by 0.10 during the first quarter, which is close to the increase in the number of referrals provided by the caseworkers. The fact that both sources of information—administrative records of caseworker actions and the monthly reports on job seekers' search behavior—paint the same picture gives strong support for the importance of the vacancy referral channel. We take this as evidence that caseworkers use their expertise to prepare suitable vacancies, point the job seekers to these vacancies during the extra meetings, and that the job seekers follow the advice and apply to the jobs.

The next question is why vacancy referrals speed up the exit from unemployment. One explanation is that vacancy referrals lead to broader job search, for instance in terms of occupation or geographical search area.[27] Job seekers who search broader already from the start of the unemployment spell may find a job faster. Another explanation is that the vacancy referrals help job seekers pinpoint the most relevant job openings in the market, without changing the targeted occupations and the search area. Put differently, the JSA program may generate more vacancy information from caseworkers, which helps the treated job seekers apply to the most relevant jobs earlier.

Table 7: Impact on different types of vacancy referrals

	2-digit occupation		County of residence	
	Within	Outside	Within	Outside
	(1)	(2)	(3)	(4)
Assigned to program	0.054*	0.050*	0.088**	0.015
	(0.030)	(0.027)	(0.037)	(0.017)
Control mean	1.106	0.932	1.670	0.368
Observations	26,538	26,538	26,538	26,538

Notes: The results are from a linear regression of each variable on a treatment indicator, weighted by the observed intention to treat share. The sample includes the active PES offices during the experiment period. The control variables include the variables in Table 1. Standard errors in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1. ITT are intention-to-treat-effects for individuals who were supposed to be randomized to treatment at the active offices. IV are instrumental-variables-estimates for those actually randomized to treatment at the active offices (given by the first line in Panel A in Table 2).

We explore these explanations by studying effects on different types of vacancy referrals. We exploit occupational and geographic information for each referral and distinguish between referrals within and outside the jobseeker's preferred occupation (2 digit level), and referrals within and outside the county of residence. The results

[27] Manning and Petrongolo (2017) show that the attractiveness of jobs decays with the distance to the job. To what extent referrals from caseworkers can push job seekers to apply to more distant jobs is an open question.

from this exercise in Table 7 show that the distribution of referrals is unaffected by the treatment: both referrals within and outside the occupation as well as referrals within and outside the region increase by around 5%. It means that the treated received more of the same, rather than new types of referrals. This supports the second explanation above, that vacancy information—while leaving the direction of search unaffected—streamlines the search process by pointing job seekers to the most relevant jobs already from the start of the unemployment spell.

6 Comparing different types of meetings

Our experiment uses three types of meetings between job seekers and caseworkers: face-to-face, distance and group meetings. In this section, we present additional evidence on the mechanisms behind the direct effect by comparing these three meeting formats. Since the type of meeting was randomized across offices, we can make a credible comparison of the formats. To support this, we have confirmed that we have balanced groups for each type of meeting (not reported).

The regression results in Table 8 (ITT effects) show that while face-to-face and distance meetings both significantly increase exits out of unemployment, group meetings appear to be less effective. Overall, group meetings display smaller point estimates, and for number of days in unemployment (1st quarter and 1st year) there is no significant impact. The smaller employment effects for group meetings are consistent with the results in previous studies (see, e.g., Maibom et al., 2017).

Above we presented evidence showing that vacancy referrals are key in explaining the direct effect of the job search assistance. One reason why group meetings perform worse could be that caseworkers who provide support to many job seekers at the same time do not have time to prepare and discuss suitable job openings with each job seeker. Table 9 provides support to this interpretation. It shows that group meetings is the

Table 8: Effects of the JSA program on unemployment, by type of meeting

	Face-to-face meetings ITT (1)	Distance meetings ITT (2)	Group meetings ITT (3)
Exit unemp. 1st quarter	0.043***	0.034***	0.024**
	(0.009)	(0.010)	(0.011)
	[0.329]	[0.367]	[0.375]
Unemp. days 1st quarter	−2.003***	−1.396**	−0.950
	(0.478)	(0.565)	(0.582)
	[75.92]	[73.40]	[73.30]
Unemp. days 1st year	−6.780***	−7.308***	−3.413
	(2.247)	(2.520)	(2.626)
	[204.4]	[191.9]	[189.3]
Observations	10,567	8,259	7,712

Notes: ITT estimates from a linear regression of each variable on a treatment indicator, weighted by the observed intention to treat share. The sample includes the active PES offices during the experiment period. The control variables include the variables in Table 1. Standard errors in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

only type of assistance that did not involve more vacancy referrals—there is no effect on caseworker actions nor on job seeker search behavior. In summary, we take this as additional evidence that information about vacancies is the main mechanism behind the effects of the JSA program.

It seems likely that distance meetings will become an increasingly important element of future JSA programs. Recent technological advancements make online communication a convenient complement to more traditional ways of providing assistance, especially for job seeker with long travel time to the local PES office. However, there is still a lack of evidence on the effectiveness of using new technologies when providing JSA and other forms of labor market policies. Here, we have shed some light on this question by showing that the direct effect of JSA is independent of whether the support from the caseworkers is given face-to-face or via distance meetings.

Table 9: Type of meeting and information about vacancies

	Caseworker action: Vacancy referrals			**Search behavior: Applications to vacancy referrals**		
	Total (1)	Suggested (2)	Mandatory (3)	Total (4)	Suggested (5)	Mandatory (6)
Face-to-face meetings	0.194**	0.144**	0.050	0.114***	0.077**	0.037***
	(0.079)	(0.070)	(0.031)	(0.039)	(0.036)	(0.012)
	[1.750]	[1.572]	[0.178]	[0.419]	[0.383]	[0.036]
Obs.	8,092	8,092	8,092	4,908	4,908	4,908
Distance meetings	0.195*	0.082	0.113***	0.151***	0.116**	0.035***
	(0.101)	(0.091)	(0.033)	(0.055)	(0.052)	(0.013)
	[2.193]	[2.042]	[0.151]	[0.584]	[0.543]	[0.042]
Obs.	5,982	5,982	5,982	3,653	3,653	3,653
Group meetings	−0.039	−0.019	−0.020	0.031	0.020	0.012
	(0.082)	(0.078)	(0.022)	(0.038)	(0.036)	(0.012)
	[1.752]	[1.652]	[0.100]	[0.382]	[0.364]	[0.0186]
Obs.	5,600	5,600	5,600	3,398	3,398	3,398

Notes: ITT estimates from a linear regression on a treatment indicator, weighted by the observed ITT-share. In columns 1–3, the outcome variables are for the 1st quarter after registration. Outcome variables in columns 4–6 are the sum over the monthly activity reports in the first quarter. Standard errors in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

7 How do displacement effects arise?

Our previous analyses showed that the JSA program gives rise to substantial displacement effects for the non-treated job seekers. This raises the question why displacement exists. One explanation is that it arises in the labor market: if there are more job seekers than vacant positions, targeted job search assistance can lead to a game of musical chairs where the non-treated job seekers end up last in line for the vacant jobs. A second explanation is that resources are allocated from the non-treated to the treated, so that the non-treated are offered less assistance than in the baseline. From a policy perspective, it is crucial to distinguish between these two sources of displacement. While expanding the JSA budget solves the problem with constrained PES resources, it is much harder to come up with policies addressing displacement in the labor market. This section adds to the literature by presenting evidence that discriminates between these two sources of displacement.[28] We first investigate displacement through resource constraints by exploiting the same detailed administrative data as above. Next, we look directly at displacement in the labor market by estimating displacement effects in tight and slack labor markets.

Table 10 presents displacement effect estimates, comparing the treated and the non-treated at the active offices with the non-treated at the non-active offices, for the same variables as in Section 5. For the non-treated, monitoring (Panel A), support and training (Panel B), and vacancy information (Panel C) is at the same level as if the experiment would not have taken place, all pointing in the direction of no displacement of resources. Using data covering the universe of contacts between job seekers and the PES, we also present evidence against displacement of meetings. Columns 1–3 of Table 11 show that the increase in the number of meetings for the treated is not due to less meetings for the non-treated at the active offices.

We next examine if non-treated job seekers were assigned to different types of caseworkers. To this end, we exploit information on caseload and tenure. For instance, caseworkers involved in the program may have handed over cases to personnel outside the program, in order to free up time for the extra meetings. If so, this would increase the workload for caseworkers working with the baseline assistance, which may affect the quality of the services provided to the non-treated. In addition, the local offices may have allocated their most tenured caseworkers to the extra meetings. Both these examples would imply a re-allocation of resources away from non-treated job seekers. To measure caseload, we count the number of meetings per month and the number of unique job seekers that the caseworker meets each month. Tenure is measured in days, and we study both overall tenure at the PES and tenure within the local office where the caseworker currently works.[29] The results reported in columns 4–7 of Table 11 con-

[28] Crépon et al. (2013) and Gautier et al. (2018) take displacement in the labor market as given without explicitly analyzing displacement of resources.

[29] Since our data on meetings start in 2010 we count tenure from this year.

Table 10: Displacement effects of the JSA program on caseworker actions and job seeker search behavior

Panel A:	**Caseworker action: Monitoring**				**Search behavior: Search intensity**		
	Total violations (1)	Violations contact (2)	Violations job search (3)		Prob. reporting (5)	Total activities (6)	Total job applications (7)
Assigned to program	0.029***	0.031***	−0.000		−0.006	0.054	−0.001
	(0.010)	(0.006)	(0.001)		(0.006)	(0.270)	(0.194)
In a program area	0.009	0.003	−0.001		0.012*	−0.207	−0.272
	(0.010)	(0.006)	(0.002)		(0.006)	(0.336)	(0.197)
	[0.290]	[0.078]	[0.010]		[0.562]	[16.4]	[8.87]
Obs.	247,714	247,714	247,714		210,779	135,961	135,961

Panel B:	**Caseworker action: Support and training**				**Search behavior: Search channels**	
	Has action plan (1)	Support cat. in plan (2)	Job search supp. cat. in plan (3)	ALMP participant (4)	Unsolicited job appl. (5)	Other job-enhancing activities (6)
Assigned to program	−0.015**	−0.002	0.010	0.004	−0.136	0.208
	(0.006)	(0.016)	(0.009)	(0.003)	(0.107)	(0.155)
In a program area	0.004	0.017	0.022	0.005	−0.002	0.089
	(0.009)	(0.019)	(0.014)	(0.005)	(0.114)	(0.114)
	[0.760]	[1.62]	[0.360]	[0.051]	[2.85]	[3.58]
Observations	694,772	525,055	525,055	694,772	135,961	135,961

Panel C:	**Caseworker action: Vacancy referrals**				**Search behavior: Applications to vacancy referralls**		
	Total (1)	Suggested (2)	Mandatory (3)		Total (5)	Suggested (6)	Mandatory (7)
Assigned to program	0.102	0.062	0.040		0.097***	0.069**	0.028
	(0.066)	(0.049)	(0.036)		(0.035)	(0.027)	(0.018)
In a program area	−0.021	−0.022	0.001		−0.020	−0.016	−0.004
	(0.072)	(0.069)	(0.015)		(0.023)	(0.022)	(0.006)
	[1.13]	[0.979]	[0.152]		[0.397]	[0.351]	[0.045]
Obs.	694,772	694,772	694,772		135,961	135,961	135,961

Notes: Regression of each outcome variable on an indicator for active PES office ("In a program area"), an indicator for active PES office × intention to treat status is treated ("Assigned to program") and indicator variables for year, month and PES office. The sample includes all offices: active offices and non-active offices, so the excluded category is the non-active offices. Standard errors clustered at the PES office level in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

firm that the JSA program did not lead to any displacement of resources: irrespective of whether we study caseload (columns 4–5) or tenure (columns 6–7) it was not the case that the non-treated at the active offices received less resources.

Table 11: Displacement of meetings and services

	Displacement of meetings			**Displacement of caseworkers**			
	Meetings quarter 1 (1)	Physical meetings Q1 (2)	Distance meetings Q1 (3)	No. meetings/ month (4)	No. clients/ month (5)	Tenure at PES in days (6)	Tenure at local office in days (7)
Assigned to program	0.547***	0.360***	0.187***	2.69	1.81	4.59	0.004
	(0.058)	(0.057)	(0.053)	(2.15)	(1.62)	(9.54)	(13.7)
In a program area	−0.035	−0.061	0.026	−0.163	0.975	−20.5	−11.5
	(0.054)	(0.041)	(0.027)	(5.24)	(3.29)	(18.8)	(25.2)
	[3.472]	[2.617]	[0.854]	[152.5]	[103.2]	[1,185]	[943.9]
Obs.	451,472	451,472	451,472	451,406	451,406	451,406	451,406

Notes: Regression of each outcome variable on an indicator for active PES office ("In a program area"), an indicator for active PES office × intention to treat status is treated ("Assigned to program") and indicator variables for year, month and PES office. The sample includes all offices: active offices and non-active offices, so the excluded category is the non-active offices. Standard errors clustered at the PES office level in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

We now turn to evidence of displacement in the labor market. Crépon et al. (2013) set up a theoretical model predicting that displacement is higher in weak labor markets, and provide empirical support for this prediction. Table 12 reproduces this analysis in our setting. We estimate our main model from column 5 in Table 3, but classify all offices according to whether the monthly local unemployment rate is above or below median unemployment among the Swedish municipalities and interact this binary variable with treatment status.[30] The results are striking. The displacement effect is considerably larger in high-unemployment labor markets than in low-unemployment markets, but there is no significant differences for the direct effect.

One interpretation of these results is as follows. The direct effect arises since the treated job seekers gain from additional information about vacancies. As expected, the vacancy information channel works under any labor market conditions, tight or slack.

[30] We also include the monthly local unemployment to population ratio as an additional regressor. It is the average unemployment rate facing the job seekers in the target population of the experiment per office and month, based on their municipality of residence.

Table 12: Displacement effects of the JSA program in strong and weak labor markets

	Exit unemp. 1st quarter (1)	Unemp. days 1st quarter (2)	Unemp. days 1st year (3)
Assigned to program×Below median unemployment rate	0.027***	−1.416**	−5.372**
	(0.009)	(0.560)	(2.539)
Assigned to program×Above median unemployment rate	0.043***	−1.588***	−7.763***
	(0.009)	(0.592)	(2.559)
In a program area×Below median unemployment rate	−0.001	0.028	−0.828
	(0.008)	(0.468)	(2.506)
In a program area×Above median unemployment rate	−0.030**	1.413***	9.207***
	(0.011)	(0.521)	(2.672)
Observations	552,816	552,816	552,816

Notes: Regression of each outcome variable on an indicator for treatment PES office ("In a program area") and an indicator for treatment PES office × intention to treat status is treated ("Assigned to program"), both interacted with whether the unemployment level in the municipality of the local PES office was below ("Below median unemployment rate") or above ("Above median unemployment rate") the median municipality, as well as year dummies, month dummies, local PES dummies and the monthly unemployment rate in the PES office municipality. Standard errors clustered at the PES office level in parentheses, control means in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

But, the harm for the non-treated job seekers is limited under good labor market conditions, when access to alternative jobs is good. In contrast, when competition for jobs increases, the non-treated are hurt by the fact that the pool of candidates that firms chose from is larger.

8 Structural evaluation of the equilibrium effects

So far we have examined equilibrium effects of the experiment on exits from unemployment, wages, vacancies and job applications. We now study the implications of a full-scale roll-out of the program on labor market outcomes, government expenditures and welfare. To this end, we estimate a Diamond-Mortensen-Pissarides (DMP) model using the equilibrium effect estimates from the experiment. To this end, we build on the model by Gautier et al. (2018), but adjust it in accordance with the job search policy evaluated in this paper. In the model, the workers (job seekers) choose the number of job applications they file and firms post vacancies. A key feature of the model is the endogenous matching function in which the success of an application depends on the number of applications sent by other workers, which creates search congestion. To capture the empirical results that the program participants find jobs faster without increasing the total number of job applications, the matching function allows for differential success rates for participants and non-participants. The model is estimated by indirect inference using the results from the experiment presented in previous sections. Since we wish to capture the behavior of actual program participants, we use the IV estimates of the program effect for the participants. This is why the model refers to participants and non-participants.

8.1 The model

The model defines an equilibrium in which search intensity, wages, unemployment and labor market tightness are determined. It is a discrete-time model with ex ante identical and risk neutral workers with the same productivity, who only differ in whether or not they participate in the JSA program (indexed by 0 or 1). The unemployed receive benefits b and the value of non-market time is h. In each period, the worker decides the number of job applications to file, a, trading off job prospects and search costs. For convenience, we assume that the search costs are quadratic in the number of applications (γa^2). If an application is successful the worker becomes employed, otherwise (s)he has to apply again in the next period.

A key part of the model is the endogenous matching function that captures that the success of an application depends on the number of applications sent by other workers. Specifically, the matching function $m(a; \bar{a}, \theta)$ is increasing in the number of own job applications, decreasing in average search intensity of other workers, $\bar{a}$, and increasing in labor market tightness, $\theta \equiv v/u$, where u is the unemployment rate and v the vacancy rate. The matching function is derived below. Finally, let r be the discount rate, and $E(w)$ the value of being employed at the wage rate w. Then, the value of unemployment for a non-participant is[31]

$$rU_0 = \max_{a \geq 0} b + h - \gamma_0 a^2 + m_0(a; \bar{a}, \theta) \left[E(w_0) - U_0 \right]. \tag{4}$$

From the first order condition we have that the optimal number of applications is:

$$a_0^* = \frac{E(w_0) - U_0}{2\gamma_0} \frac{\partial m_0(a_0; \bar{a}, \theta)}{\partial a_0} \bigg|_{a_0 = a_0^*}. \tag{5}$$

The participants receive more meetings with caseworkers through the JSA program. We allow this to have three separate effects. First, obtaining job search assistance may reduce the participants' search costs, γ_1, in relation to the costs of non-participants, γ_0. Second, participating in the program costs non-market time. We normalize its value to zero for participants and let it be h for the non-participants. In the estimations, we do not restrict the sign of h, even though our prior is that the value of non-market time is lower for the participants. Third, we specify separate matching functions for participants and non-participants under the assumption that the treatment may affect job-search efficiency and the success rate per job-application (see below). Thus, the value of unemployment for a program participant is

$$rU_1 = \max_{a_1 \geq 0} b - \gamma_1 a_1^2 + m_1(a_1; \bar{a}, \theta) \left[E(w_1) - U_1 \right], \tag{6}$$

and the first order condition gives

$$a_1^* = \frac{E(w_1) - U_1}{2\gamma_1} \frac{\partial m_1(a_1; \bar{a}, \theta)}{\partial a_1} \bigg|_{a_1 = a_1^*}. \tag{7}$$

In equilibrium, the average number of applications in the market equals $\bar{a} = \tau a_1^* + (1 - \tau) a_0^*$, where τ is the share of program participants.

[31] Note that this expression implicitly means that benefits and search costs are realized at the end of the period. This simplifies the notation.

The value of employment depends on the wage, w, the exogenous job destruction rate, δ, and the difference between the flow values of employment and unemployment:

$$rE(w_i) = w_i - \delta[E(w_i) - \bar{U}] = w_i - \delta\left[E(w_i) - U_0\right], \tag{8}$$

where $\bar{U} = \tau U_1 + (1-\tau)U_0$ is the average utility.[32]

Firms are also assumed to be identical. The value of a vacancy, V, for a firm is determined by the vacancy cost, c_v, the probability of filling the vacancy, $\frac{m(a_1,a_0,\theta)}{\theta}$, and the value difference between a filled, $J(w)$, and an unfilled vacancy:

$$rV = -c_v + \frac{m(a_1,a_0,\theta)}{\theta}(J(w) - V). \tag{9}$$

The value of a filled vacancy is given by

$$rJ(w) = y - w - \delta(J(w) - V), \tag{10}$$

where y is the value of output in each period. With free entry, the value of a vacancy is zero in equilibrium. From (9), (10) and this zero-profit condition we have

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

which can be solved for the equilibrium value of tightness, θ^*, since the left-hand side is decreasing in θ and the right-hand side is increasing in θ, so that there is a unique θ^* that satisfies equation (11).

We now specify the matching functions for participants and non-participants, allowing for different search intensities and different success rates per application for the two groups. We use an urn-ball model, implying that a firm receiving many applications

[32]Workers presumably realized that the JSA program was temporary. We therefore replace $\bar{U}$ in (8) by U_0 when estimating the model, but use $\bar{U} = \tau U_1 + (1-\tau)U_0$ in the policy simulations, assuming that workers expect to be non-participants if they re-enter unemployment after losing their job.

randomly selects one and declines the others. This creates search congestion, since the probability that an application is accepted depends on the number of applications sent by other workers. Note that the empirical analyses in earlier sections show that the JSA program leads to more vacancy referrals and a higher job-finding rate, without changing the total number of job applications. This implies a higher probability that an application results in a job offer for the participants than for the non-participants. To incorporate this, we allow the job applications from the participants to have a higher probability of being drawn from the pool of candidates than the applications from non-participants.

Specifically, we introduce a parameter, ω, such that an application from a program participant results in a job offer with probability $\frac{\omega}{\omega+\omega j_1+j_0}$, if the number of participant competitors equals j_1 and the number of non-participant competitors for the job is j_0. For non-participants this probability is $\frac{1}{1+\omega j_1+j_0}$. By allowing both search costs and the success rate per application to differ, the model shows to what extent the higher job-finding rate for the program participants is due to changes in the search costs, or due to more suitable job applications (higher success rate per application).

If the number of workers and vacancies are sufficiently large, the number of applications from participants and non-participants are approximately Poisson distributed random variables with means $\tau a_1^*/\theta$ and $(1-\tau)a_0^*/\theta$, respectively. Thus, for the non-participants the probability that an application results in a job offer is:

$$\psi_0 = \sum_{j_1=0}^{\infty}\sum_{j_0=0}^{\infty} \frac{1}{1+\omega j_1+j_0} f_0(j_0) f_1(j_1), \tag{12}$$

where $f_0(j_0) = \frac{\exp(-[1-\tau]a_0^*/\theta)([1-\tau]a_0^*/\theta)^{j_0}}{j_0!}$ and $f_1(j_1) = \frac{\exp(-\tau a_1^*/\theta)(\tau a_1^*/\theta)^{j_1}}{j_1!}$ are the probability of j_1 applications from participants and j_0 applications from other non-participants.

For participants, we have

$$\psi_1 = \kappa \sum_{j_1=0}^{\infty} \sum_{j_0=0}^{\infty} \frac{\omega}{\omega + \omega j_1 + j_{00}} f_0(j_0) f_1(j_1). \tag{13}$$

Here, the parameter κ captures other effects of the JSA program on job search efficiency. For example, the program may affect the type of jobs the participants apply to, thereby affecting the share of vacancies for which a participant has a positive productivity. The idea is that the firm first selects a worker for the job and then learns whether the worker is productive or not.[33]

All this leads to the matching functions $m_0(a_0; a_1, \theta) = 1-(1-\psi_0)^{a_0}$ and $m_1(a_1; a_0, \theta)$ $= 1-(1-\psi_1)^{a_1}$, and the aggregate matching function $m(a_0, a_1, \tau, \theta) = \tau m_1(a_1, a_0, \theta) +$ $(1-\tau) m_0(a_0, a_1, \theta)$.

Wages are set in a Nash bargaining when workers and firms have met, with worker bargaining power equal to β. Since participants and non-participants have different outside options, U_1 and U_0, we allow their equilibrium wages to differ. The bargaining outcome is given by

$$w_i^* = \arg\max_{w_i} \left[E(w_i) - U_i\right]^{\beta} \left[J(w_i) - V\right]^{1-\beta}, \tag{14}$$

and the first order condition gives

$$(1-\beta)\left[w_i^* + \delta \bar{U} - (r+\delta) U_i\right] = \beta \left[y - w_i^*\right]. \tag{15}$$

In equilibrium, inflow into and outflow from unemployment are equal, and, hence, equilibrium unemployment is

$$u^* = \frac{\delta}{\delta + \tau m_1(a_1^*; a_0^*, \theta) + (1-\tau) m_0(a_0^*; a_1^*, \theta)}. \tag{16}$$

[33]The parameter κ is included in the model presented in Gautier et al. (2018). We incorporate it in our model for completeness.

The equilibrium can now be defined as $\{a_0^*, a_1^*, \theta^*, w_0^*, w_1^*, u^*\}$ satisfying equations (5), (7), (11), (15), and (16).

8.2 Estimation

We estimate the model using the actual share of program participants in the eligible population. From Section 2, we have that the program participants corresponds to 62% of those assigned to the program. With a fraction assigned to the program of about 0.5, the share of program participants during the experiment, τ^e, is 0.31. Note that this share is based on the participants and non-participants included in the experiment. The non-participants also include workers in the stock of unemployed before the experiment, and workers not in the target population (immigrants and workers with repeated spells of unemployment). These groups were not included in the experiment, and the analyses in Section 4.2 revealed no evidence of any displacement effects for these groups, indicating that the displacement mainly occurs within the target population. The absence of displacement outside the experiment explains why we use the share of participants in the target population. But, below we also report results from sensitivity analyses using lower treatment shares, implicitly allowing for displacement effects for the non-target population.

Estimation is based on indirect inference using the reduced form estimates from the experiment. The model estimates are then used to simulate a full-scale roll-out of the program. The data moments used in the estimations, adapted to the monthly intervals in the discrete-time model, are displayed in Table A-4 in the appendix. At the individual level, we use estimates for the exit rate from unemployment (Table 3) and wages in subsequent jobs (Table 4) for participants and non-participants. As already mentioned, IV estimates are used since they reflect the program effects for the participants. The data moments also include the average re-employment rate from the PES data, the average vacancy rate and unemployment rate facing the target population in each office based on their municipality of residence, and the average replacement rate (fraction of previous income replaced by the unemployment insurance) from calculations by the National Institute for Economic Research (2016). We also use information on the number of job applications filed by each worker, including both the average number of job applications and effects on the number of job applications for participants and non-participants (see Table A-4 for details). The latter helps to capture if the treatment is due to changed search costs or changed success rate per application. Finally, the interest rate is set to $r = 0.008$, which is the monthly interest rate implied by an annual rate of ten percent, and we normalize productivity to $y = 1$.

This gives nine unknown parameters to estimate in the model. Using indirect inference, we minimize the sum of the differences between the data moments and the corresponding model moments (see A-4 in the appendix for details). Each data moment is weighted by the inverse of its variance, so that more precisely estimated moments are given larger weight. Standard errors are computed using the delta method.

8.3 Government expenditure and welfare

Besides effects on unemployment, wages and labor market tightness, we study government expenditure and welfare. The expenditures include unemployment insurance benefits, b, and program costs, c_p, per worker. Then, government spending as a function of the share of participants is

$$C_B(\tau) = ub + \delta(1-u)\tau c_p, \tag{17}$$

where ub is the fraction of unemployed times the unemployment insurance benefits, and $\delta(1-u)\tau$ is the fraction of new entrants into unemployment (job destruction rate, δ, times the employment rate, $(1-u)$). To estimate the costs, we performed a detailed time-use survey, which was sent out to all caseworkers involved in the program. Caseworkers were asked to estimate the average time spent on a meeting, including time for preparation before the meeting, time spent on the actual meeting as well as time spent on documentation and other activities after the meeting. Taken together, caseworkers spent on average 75 minutes per meeting and 225 minutes in total for the three extra meetings in the program,[34] corresponding to 2.5% of the monthly working hours. Next, based on calculations made by the PES, we assume that the average caseworker wage equals the average wage obtained by the workers in our target population. With the monthly production, y, normalized to one, the average wage is 0.96. We also add overhead costs for premises, adminstration and managers, estimated to roughly 30%. In total, the estimated program cost is 0.026 of monthly production.

The welfare effects of the program summarize a number of distinct elements. First, we have the resources spent on the program, c_p, assumed to be funded by a non-distortionary tax to avoid complications introduced by tax incentives and their effects on job search behavior. Second, we have an effect on output, $(1-u)y$, of employment changes induced by the program. Third, we have vacancy costs, vc_v, which vary with the number of open vacancies, v, and the cost per vacancy, c_v. Fourth, both participants and non-participants experience search costs as a function of γ_1, γ_0 and the number of applications. Finally, program participation implies a loss of non-market time, h. In total, welfare, $W(\tau)$, is given by

$$W(\tau) = (1-u)y + u\left((1-\tau)\frac{h - \gamma_0 a_0^{*2}}{1+r} + \tau\frac{-\gamma_1 a_1^{*2}}{1+r}\right) - \delta(1-u)\tau c_p - vc_v. \tag{18}$$

8.4 Results

We now turn to the simulation results. Initially, Panel A of Table A-5 in the appendix assesses the model fit, displaying the difference between the moments implied by the

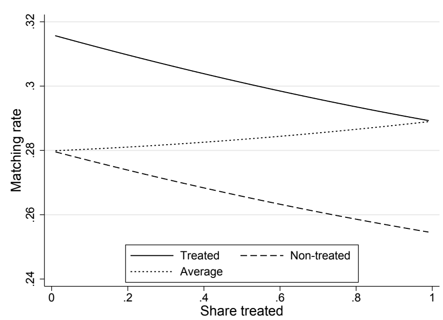
[34] This takes costs associated with cancelled meetings into account since the survey also asked how often meetings were cancelled and how much time that was lost due to cancelled meetings. These events account for 3 of the total 75 minutes per meeting.

model and the data moments. Generally, the fit is good: the program effects on the job-finding rate and the wage rate all match perfectly, as do the aggregate statistics. Panel B of Table A-5 in the appendix presents the model estimates. Let us comment on some of the estimates. The estimate of ω implies that the participants have a higher probability to be drawn from the pool of candidates, leading to a higher success rate per application. Participants also have higher search costs than non-participants ($\gamma_1 > \gamma_0$). The higher success rate per application combined with the increased search costs imply that participants apply to roughly the same number of jobs as non-participants. This is consistent with the analyses of the mechanisms, which suggest that program participants receive more vacancy referrals, leading to a higher job-finding rate even though they do not apply for more jobs.

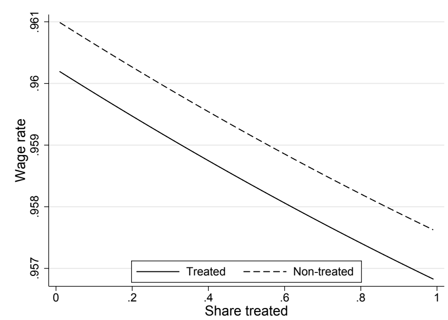
Figure 3 presents simulation results for different shares of participants, including a full-scale roll-out. Recall that the reduced form estimates show that the participants find jobs faster, and that the non-participants find jobs at a lower rate due to displacement of jobs. The simulation results are qualitatively similar, with a matching rate almost four percentage points, or around 15%, higher for participants compared to non-participants. We also see that the matching rate for both participants and non-participants are decreasing in the treatment share, since a larger share of participants creates more search congestion. Overall, however, the aggregate matching rate increases with the share of program participants. Figure 3 also shows that participants receive lower wages than non-participants, and that wages for both groups are decreasing in the treatment share. The latter is because more participants create more search congestion, which lowers the outside options of all workers. The higher matching rate and the lower wages induce firms to create more vacancies, so that market tightness is increasing in the share of participants.

Overall, the results in Figure 3 imply that unemployment decreases with the share of program participants. Increasing the share of participants from 0 to 100 percent lowers the unemployment rate by around 0.2 percentage points, implying that the net effect of the JSA program on unemployment is positive. Next, column 1 in Table 13 summarizes the impact on unemployment, and evaluates the impact on government expenditure and welfare. We see that that the JSA program reduces government spending, and that a full-scale roll-out of the program gives the lowest costs. Besides the case with monthly productivity normalized to one, the change in government expenditures is also calculated under the assumption that the monthly productivity is SEK 25,000 (average wage rate for the target population). These calculations show that a full-scale roll-out would decrease government expenditures by SEK 16 (≈ Euro 1.6) per worker, i.e. a small effect. This means that the decrease in benefit payments as a result of the lower unemployment rate is of roughly the same magnitude as the direct program costs.

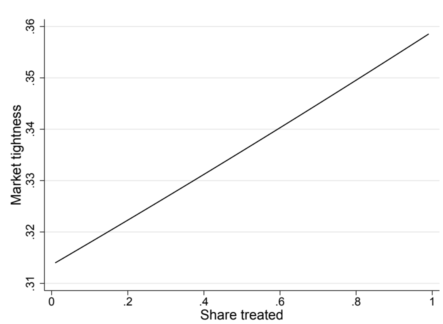
Figure 3: Simulation results for different shares of participants



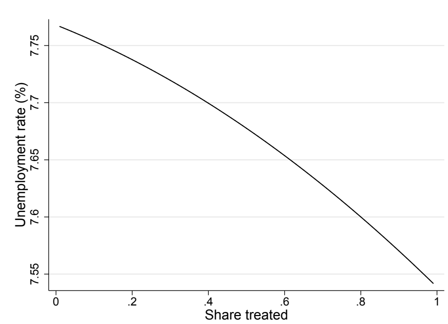
(a) Matching rate



(b) Wages



(c) Market tightness



(d) Unemployment rate

We next study the welfare implications, which also take changes to search costs, non-market time, and vacancy costs into account. The simulation results in column 1 in Table 13 reveal that welfare is decreasing in the share of participants. Thus, the positive effect on production from the decreased unemployment rate cannot compensate for the decreased value of non-market time (participants loose non-market time), the direct program costs, and the increased vacancy costs induced by the increased vacancy rate.

Table 13: Model predictions for the equilibrium search model

	Main model	Sensitivity analyses			
		Face-to-face and distance meetings	Delayed vacancy model	Treatment share 25%	Treatment share 20%
	(1)	(2)	(3)	(4)	(5)
Experiment ($\tau = 0.31$)					
Unemployment (measured in %)	-0.048	-0.163	-0.321	0.004	0.069
Government expenditure	-0.000	-0.001	-0.002	0.000	0.001
Government expenditure (SEK)	-1.9	-18.5	-42.0	4.8	14.2
Welfare	-0.002	-0.002	0.002	-0.003	-0.004
Full-scale roll-out					
Unemployment (measured in %)	-0.228	-0.599	-0.988	-0.083	0.094
Government expenditure	-0.001	-0.003	-0.005	0.000	0.001
Government expenditure (SEK)	-16.1	-69.4	-128.2	5.2	27.7
Welfare	-0.007	-0.004	0.006	-0.009	-0.012

Note: Outcomes are normalized with the monthly output per worker set to 1.

8.5 Sensitivity analyses

We now present results from several sensitivity analyses. One result from the reduced form analyses is that face-to-face and distance meetings increase the job-finding rate, whereas the group meetings do not. The first sensitivity analyses therefore excludes group meetings. To this end, the data moments are re-estimated using only the offices with face-to-face and distance meetings. We also re-calculate the program costs; face-to-face and distance meetings are more expensive than group meetings (0.031 of monthly production compared to 0.026 for all three meeting types). In all other respects the estimations are the same as before. The results from this exercise in column 2 of

Table 13 indicate a larger reduction of the unemployment rate, and a more substantial decrease of government spending than for the main analyses. However, welfare is still decreasing in the share of participants. One reason is the larger loss of non-market time in this model, i.e. the disutility from attending a face-to-face or a distance meeting appear to be higher than for a group meeting.

Next, the equilibrium conditions presume that the labor market is in the new steady state during the experiment, which includes the full demand side response with complete adjustment of vacancies. However, firms may not have time to observe and react to the new economic environment in the short run and. There was also some evidence of such a pattern in the data, with vacancies unaffected during the earlier parts and an increased vacancy rate towards the end of the experiment. One may therefore expect to see further adjustments of vacancies in the long run if the program was made permanent. If so, the main analyses underestimate the effect of the program. The reason is that in the short run, any increased job finding for some participants leads to search congestion with negative displacement effects for both the non-participants and other participants. In the long run, however, more job applications and lower wages induce firms to open more vacancies, which increases the job finding rate for both participants and non-participants, and this pushes down the unemployment rate. To explore the implications of this, we estimate an alternative version of our model under the assumption that job-search activity adjusts during the experiment, but that firms do not adjust vacancies during the experiment.[35]

[35]This delayed vacancy approach assumes that the participants react to the program, and that both participants and non-participants realize and respond to the fact that the return to an application is lower during the experiment. However, we assume that firms do not observe that the average wage and matching rates in the labor market have changed, and therefore post vacancies at the same rate as before the experiment. Specifically, we assume that the market tightness remains at the pre-experiment steady state level. We also assume that the wage bargaining is based on the outside options of the worker, leading to different wages for participants and non-participants. Formally, we estimate the model under the assumption that the labor market is in a steady-state equilibrium before the experiment. During the experiment, market tightness remains at the pre-experiment level. However, the number of applications and wages adjust during the experiment and are given by equations (5), (7) and (15). Under these assumptions, we re-estimate the model and simulate the new steady state, allowing for the full demand side response with a complete adjustment of vacancies.

As expected, this delayed vacancy approach, predicts a larger reduction of unemployment and government expenditures (down by about SEK 85 per worker) than in the main analyses. Also, interestingly, the welfare effects are now reversed with positive welfare effects compared to the negative welfare effects found in the main analyses. In fact, the highest level of welfare is now obtained with a full-scale roll-out of the program, suggesting that the JSA program may also be welfare enhancing.

Finally, in the main analyses, the share of participants is set to the share in the target population. However, if non-participants outside the experiment, such as the stock of unemployed before the experiment and the inflow to the non-target population, also are subject to displacement effects, this implies a lower treatment share. We therefore explore setting the share of participants to 0.25 and 0.20. The displacement effects is set to the same level as for the non-participants in the experiment, which means that we extrapolate the displacement effects for the non-participants in the target population to the non-participants outside the target population. By construction, this leads to less positive effects. Table 13 shows that the share of participants in the main analyses (31%) implies a non-negligible reduction of the unemployment rate (0.2 percentage points) of a full-scale roll-out of the program. In contrast, a treatment share of 25% implies a smaller decrease of the unemployment rate (0.08 percentage points), and with 20% treated program participants, the unemployment rate increases with 0.09 percentage points).

9 Conclusions

This paper has evaluated direct and displacement effects of job search assistance, using a large-scale two-level randomized experiment. The JSA program more than doubled the frequency of meetings with caseworkers at the local public employment office during the first quarter of unemployment. In line with the previous literature on job search assistance, we find that treated job seekers exit unemployment faster than non-treated job seekers. By exploring detailed data on caseworkers' actions and job seekers' search behavior, we show that the driving mechanism behind the direct effect is an increased amount of vacancy referrals passed on from caseworkers to job seekers. This suggests that caseworkers play an important role in bringing job seekers back to work, but also that it is crucial *how* the extra assistance is designed. We also show that more information about vacancies does not lead to broader search, but rather streamlines the search process by pointing job seekers to the most relevant jobs early in the unemployment spell.

By comparing different meeting formats we find additional support for the importance of vacancy referrals as the driving mechanism. The two meeting types that involve more referrals—face-to-face meetings and distance meetings—are equally effective in bringing individuals back to work. In contrast, group meetings show no increase in referrals and are also less effective. A likely explanation is that it is difficult to prepare and discuss suitable vacancies during group meetings. Since technological advancements make distance communication a convenient complement to traditional face-to-face assistance, the fact that the distance meetings perform well is a highly policy-relevant finding.

The experiment was explicitly designed to detect displacement. In addition to the positive direct effects for the treated, we indeed find substantial displacement effects for the non-treated. We show that the displacement is not an artifact of the experiment due to crowding out of resources. Instead the displacement is due to displacement of jobs, as the competitive advantage for the treated due to the increased number of referrals have negative effects on the non-treated. This is supported by the fact that we see more displacement in tight than in slack labor markets. It implies that JSA is more efficient under favorable labor market conditions. The fact that the JSA program is associated with important equilibrium effects is consistent with recent findings in the literature

(Crépon et al., 2013; Gautier et al., 2018). Even if we find substantial displacement effects, our assessment of the benefits of the JSA programs is more positive than in these recent studies. Overall, the program reduces unemployment since the positive direct effect on the exit rate outweigh the negative displacement effect.

To trace out the equilibrium effects of a full-scale roll-out of the program, we develop and estimate an equilibrium search model. One result is that a full-scale implementation is associated with decreased unemployment, but because of program costs it has negligible effects on public spending. The impact on welfare is negative, however, due to the time costs of participants, direct program costs, and vacancy costs from an increased vacancy rate. We also show that the overall assessment of a full-scale roll-out hinges on how broadly the displacement effects hit and to what extent firms react to the new equilibrium by creating more vacancies. One example is that the welfare effect reverses to positive when we allow for a delayed vacancy response, taking into account that it may take time for firms to observe and react to the new market conditions created by the JSA program.

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Appendix A: Additional Tables and Figures

Table A-1: Sample statistics by treatment status

Variables	Assigned to program (1)	Program participant (2)	Attended at least one meeting (3)
Age	33.33	34.34	35.37
Male	0.542	0.538	0.544
Unemployment benefits	0.642	0.692	0.760
Disabled	0.052	0.048	0.035
Matchable	0.868	0.884	0.920
Education level			
Less than high school	0.224	0.206	0.172
High school	0.491	0.494	0.512
College	0.285	0.300	0.316
Place of birth			
Sweden	0.678	0.700	0.716
Nordic countries	0.013	0.013	0.015
West Europe	0.036	0.036	0.033
Outside west Europe	0.273	0.251	0.236
Unemployment days			
Year t–1	30.66	31.30	31.78
Year t–2	67.42	69.16	70.44
Year t–3	69.57	73.08	76.43
Year t–4	63.82	67.95	73.18
Unemployment spells			
Year t–1	0.431	0.419	0.400
Year t–2	0.789	0.787	0.779
Year t–3	0.806	0.830	0.833
Year t–4	0.706	0.752	0.793
No. spells, last 4 yrs			
Labor market education	0.024	0.025	0.029
Preparatory education	0.048	0.047	0.042
Labor market training	0.027	0.027	0.027
Subsidized employment	0.106	0.122	0.142
Observations	14,075	8,358	3,183

Notes: Summary statistics, weighted by the intention to treat share.

Table A-2: Impact outside target population

	Non-experiment periods 2015			Non-target population		
	Exit unemp. 1st quarter	Unemp. days 1st quarter	Unemp. days 1st year	Exit unemp. 1st quarter	Unemp. days 1st quarter	Unemp. days 1st year
	(1)	(2)	(3)	(4)	(5)	(6)
In a program area	−0.009	0.699*	1.585	0.011	−0.727	−3.049
	(0.008)	(0.419)	(2.188)	(0.007)	(0.581)	(1.973)
Control mean	0.390	73.78	187.0	0.535	52.34	162.7
Observations	552,816	552,816	552,816	367,778	367,778	367,778

Notes: Regression of each outcome variable on an indicator for active PES office ("In a program area") during 2015. The regressions include year dummies, month dummies, PES office dummies and the control variables in Table 1. Standard errors in parentheses are clustered at the PES office level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A-3: Heterogenous effects of the JSA program on unemployment

	No college	College	Born in west EU	Born outside west EU	Short unemp. history	Long unemp. history	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Exit unemp. 1st quarter								
Assigned to program	0.038***	0.024***	0.029***	0.049***	0.031***	0.037***	0.044***	0.022***
	(0.007)	(0.009)	(0.007)	(0.012)	(0.007)	(0.009)	(0.008)	(0.007)
In a program area	−0.014**	−0.016*	−0.013*	−0.018**	−0.016**	−0.014*	−0.025***	-0.005
	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
Control mean	0.389	0.391	0.427	0.286	0.384	0.395	0.383	0.398
Panel B: Unemp. days 1st year								
Assigned to program	−6.292***	−5.462**	−3.939**	−11.692***	−3.429**	−8.694***	−7.937***	−3.614**
	(1.956)	(2.079)	(1.809)	(3.827)	(1.536)	(1.858)	(1.972)	(1.659)
In a program area	4.075*	3.844*	3.518*	4.745*	4.035**	4.260*	6.431***	1.546
	(2.161)	(2.166)	(1.845)	(2.596)	(1.874)	(2.174)	(1.899)	(2.442)
Control mean	189.3	181.1	173.1	225.9	187.9	186.2	189.5	183.9
Observations	402,834	149,982	413,279	139,537	276,408	276,408	303,807	249,009

Notes: Regression of each outcome variable on an indicator for active PES office ("In a program area") and an indicator for active PES office × intention to treat status is treated ("Assigned to program"). The regressions include year dummies, month dummies, PES office dummies and the control variables in Table 1. Short/long unemployment history is defined as having below/above the median number of unemployment days during the last four years prior to registration. Standard errors in parentheses are clustered at the PES office level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A-4: Reduced form estimates used in the equilibrium search model (moment conditions)

	Data moment and inverse weight		Description	Value in the model
	Main model	Face-to-face and distance		
Panel A: Estimated program effects				
Exit rate, treated	0.039 (0.010^2)	0.065 (0.010^2)	Table 3	$\left[1-(1-(m_1\vert\tau=\tau^e))^3\right]-\left[1-(1-(m_0\vert\tau=0))^3\right]$
Exit rate, non-treated	-0.015 (0.006^2)	-0.017 (0.007^2)	Table 3	$\left[1-(1-(m_0\vert\tau=\tau^e))^3\right]-\left[1-(1-(m_0\vert\tau=0))^3\right]$
Log wage, treated	-0.002 (0.008^2)	-0.006 (0.007^2)	Table 4	$\frac{w_1\vert\tau=\tau^e-w_0\vert\tau=0}{w_0\vert\tau=0}$
Log wage, non-treated	0.0002 (0.005^2)	0.001 (0.005^2)	Table 4	$\frac{w_0\vert\tau=\tau^e-w_0\vert\tau=0}{w_0\vert\tau=0}$
Log vacancy rate	0.033 (0.033^2)	0.029 (0.036^2)	Table 5	$\frac{v^*\vert\tau=\tau^e-v^*\vert\tau=0}{v^*\vert\tau=0}$
Job applications, treated	-0.273 (0.35^2)	-0.308 (0.39^2)	Table 9	$a_1\vert\tau=\tau^e-a_0\vert\tau=0$
Job applications, non-treated	-0.272 (0.20^2)	-0.284 (0.22^2)	Table 9	$a_0\vert\tau=\tau^e-a_0\vert\tau=0$
Panel B: Aggregate statistics				
Exit rate (three months)	0.37 (0.003^2)	0.37 (0.003^2)	Fraction of job seekers in treated offices who exit for employment with three months.	$1-\tau(1-(m_1\vert\tau=\tau^e))^3-(1-\tau)(1-(m_0\vert\tau=\tau^e))^3$
Unemployment rate	0.077 (0.0001)	0.077 (0.0001)	Unemployment rate treated offices.	$u^*\vert\tau=\tau^e$
Vacancy rate	0.019 (0.001)	0.019 (0.001)	Vacancy rate treated offices. Calculated using information on vacancies at the municipal level.	$v^*\vert\tau=\tau^e$
Replacement rate	0.60 (0.001)	0.60 (0.001)	Average replacement rate in Sweden (ref).	$b\frac{1}{w^*\vert\tau=0}$
Job applications	3.45 (0.052^2)	3.45 (0.052^2)	Average number of job applications in treated offices.	$\bar{a}\vert\tau=\tau^e$

Note: Empirical moments used in the estimation of the equilibrium search model. Panel A reports moments from the estimated program effects with variances estimated in the empirical models. The effects on the number of job applications are re-scaled into IV estimate based on the estimates in Table 9. Moments for the face-to-face and distance meeting model are estimated in the same way as the main model. Panel B reports moments in the form of aggregate statistics for the treated areas. For exit rate and job applications variances across unit are used. Unemployment rate, vacancy rate and replacement rate do not have estimated variances. Here, the variances are set to capture that the unemployment rate is measured precisely, whereas the vacancy rate and replacement rate are less precisely estimated.

Table A-5: Model fit and model estimates for the equilibrium search models

Panel A: Model fit		
	Moment	Deviation from the moments
Estimated program effects		
Exit rate, treated	0.039	0.000
Exit rate, non-treated	-0.015	-0.000
Log wage, treated	-0.002	0.000
Log wage, non-treated	0.0002	-0.001
Log vacancy rate	0.033	0.018
Job applications, treated	-0.273	0.006
Job applications, non-treated	-0.272	0.080
Aggregate statistics		
Exit rate (three months)	0.37	-0.000
Unemployment rate	0.077	-0.000
Vacancy rate	0.019	0.006
Replacement rate	0.60	-0.001
Job applications	3.45	0.000

Panel B: Model estimates		
	Parameter	Estimates
Fixed parameters		
Treatment share	τ^e	0.31
Discount rate	r	0.008
Productivity	y	1
Estimated parameters		
Application cost, non-treated	γ_0	0.016 (0.008)
Application cost, treated	γ_1	0.022 (0.011)
Job destruction rate	δ	0.024 (0.003)
Share of vacancies w. pos. surplus	κ	0.903 (0.154)
UI benefits	b	0.576 (0.093)
Bargaining power	β	0.584 (0.196)
Vacancy costs	c_v	1.102 (1.171)
Value of non-market time	h	0.049 (0.047)
Return to application, treated	ω	1.426 (0.855)

Note: The table summarizes the model fit and present the model estimates.