Job search assistance and displacement effects: Evidence from a randomized experiment*

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Abstract

Three types of early interventions for unemployed Swedish job seekers are studied using a large-scale randomized social experiment. The two-level design with randomization over both local public employment offices and over job seekers enables us to estimate both treatment and displacement effects. Our main findings are that intensified meetings with caseworkers reduce unemployment rates for the participants, but almost all of the effects are due to displacement effects on the non-participants. Analysis of the mechanisms behind the observed effects shows that the positive effect of the intensified meetings are due to an increased number of job vacancy proposals and referrals from caseworkers to the treated unemployed workers. We also show that the observed displacement effects are due to displacement of jobs and not due to displacement of resources. Heterogenous effects reveal that weaker job seekers benefit more from the intensified meetings. The comparison of the three types of meetings shows that individual meetings with caseworkers and distance meetings using IT or telephone have favorable employment effects, but we find no effects of group meetings.

Keywords: intensified meetings, search intensity, search channels, vacancy referrals, displacement of jobs, displacement of resources, randomized controlled trial.
JEL codes: J68, J64, C93

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1 Introduction

Job search assistance (JSA) is an important part of active labor market policies (ALMPs) in many countries. Recent experimental evidence on intensified JSA suggests that more frequent meetings between caseworkers and job seekers have positive employment effects for the treated.[1] Meta studies by Card et al. (Card et al. (2010), Card et al. (2017)), which also include observational studies, point in the same direction. However, a concern is that the positive results for those who obtain the intensified support reflect negative impacts on the non-treated job seekers, especially for those applying to the same vacancies as the treated. Such displacement effects, which mainly represent a re-ordering of jobs and not any net employment effects, lead to a very different assessment of the policy.

Recently, displacement effects have also attracted more attention in the literature. Crépon et al. (2013) apply a two-level randomization of JSA in France, which involves randomization of treatment over both local employment offices and over individuals within the treated offices. This novel approach enables the authors to provide the first experimental evidence on displacement effects of ALMPs. Gautier et al. (2017) 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. Interestingly, both studies document substantial displacement effects. A number of studies have also estimated displacement effects with non-experimental designs.[2]

In this paper, we use a large-scale randomized social experiment and detailed data to contribute to this literature in several ways. The experiment set up in 2015 involves 72 local public employment offices in Sweden, where most of these offices serve one entire local labor market. By randomization, 36 of the 72 offices were selected to be treated offices, and within the treated offices a fraction of the job seekers were randomized to the treatment consisting of early and more frequent meetings with a caseworker at the local public employment office. This two-level randomization solves two identification problems. First, randomization over individuals ensures that characteristics are equally distributed among the treated and the non-treated within the treated offices. Second, the randomization over offices enables us to identify any displacement effects

[1]The experimental evidence include Gorter and Kalb (1996), Dolton and O'Neill (1996), Dolton and O'Neill (2002), van den Berg and van der Klauuw (2006), Hägglund (2011), Hägglund (2014), Graversen and van Ours (2008a), Graversen and van Ours (2008b), Crépon et al. (2013), Arni (2015) and Maibom et al. (2017). A recent US example is the 15-month impact evaluation of the WIA experiments with positive earnings effects of intensified services (McConnell et al. (2016)).

[2]Some examples are Blundell et al. (2004), Pallais (2014), Lalive et al. (2015) as well as Dahlberg and Forslund (2005) and Albrecht et al. (2009) for Sweden.

by comparing outcomes of non-treated individuals at treated and non-treated offices.

This experimental design follows the two-level design used by Crépon et al. (2013). However, our target population consists of *all* newly unemployed job seekers, while Crépon et al. (2013) study young, highly educated, long-term unemployed job seekers. One contribution of our paper, therefore, is that we can assess if the large displacement effects observed by Crépon et al. (2013) also apply to a more general population. This is also what we find: early frequent meetings reduce the time spent in unemployment for the treated, but also that large displacement effects imply a zero net effect on unemployment.

Another contribution is that we use various information to study the *mechanisms* behind the observed effects. We regard this as an important contribution as there is very little research about the mechanisms behind the effects of JSA.[3] Here, we use detailed information on the actions that take place during the meetings, as well as information on how these actions affected job search intensity, search channels, vacancy referrals and other job search related activities. For the latter we use information from monthly reports filed by all unemployed workers in Sweden on all job search activities during the last month.

From this we obtain several interesting results. The early frequent meetings do not affect the number of job applications (search intensity) and the mix of different job search channels. Instead, the effect of JSA seems to be driven by an increased number of job vacancy proposals and referrals from caseworkers to the treated unemployed workers. Simply, before the meetings caseworkers seem to scan the job market for suitable vacancies. These vacancies are then discussed during the meetings, which in the end induces the job seekers to apply to different and perhaps more suitable types of jobs, leading to positive employment effects.

We also study the channels for the displacement effect. In particular, we analyze whether the displacement is due to resource constraints on the non-treated imposed by the experimental design or whether displacement arises as a consequence of displacement of jobs. In both our experiment and in the experiment evaluated by Crépon et al. (2013), the idea was to give more assistance to the treated and unchanged support to the non-treated. However, since both studies find large displacement effects and

[3] Arni (2015) and Crépon et al. (2015) are two exceptions which tries to open the black box of JSA using randomized experiments. Arni (2015) study a coaching program for older workers and finds that the program did not increase search intensity, but instead improved the search efficiency of the treated. Crépon et al. (2015) study disadvantaged young workers and find substantial effects of JSA. The mechanism behind the results is that the treated substituted formal job search with informal search.

since displacement of resources and/or jobs lead to very different policy implications, examining displacement of resources is important. To this end, we use various data sources to study resource allocation. We find that the additional support to the treated did not crowd out support in terms of the number of meetings or the number of vacancy proposals or referrals obtained by the non-treated. We also construct detailed information on the caseworkers to exclude displacement of good caseworkers. Thus, we conclude that the sizable displacement effects in this paper and in Crépon et al. (2013) can arise solely due to displacement of jobs.

To further substantiate this conclusion, we also provide additional evidence of displacement of jobs. First, we analyze how displacement varies with the business cycle, and find substantially larger displacement effects in weak labor markets. This is in line with the theoretical predictions and the results in Crépon et al. (2013). A second contribution is that we analyze displacement among different groups in the labor market. Interestingly, we find that groups with high risks of long-term unemployment have positive net effects even when we take displacement into account. This improved employment outcome comes at the expense of unemployed workers with a stronger labor market attachment. Our interpretation is that caseworkers during the meetings provide the unemployed workers with information about suitable vacancies. This induces weaker job seekers to apply for different kind of jobs, leading to more employment, but this comes at the expense of stronger applicants who previously were first in the line for those jobs. Thus, even if we, due to displacement, on average see no effect on net employment, JSA may still be worthwhile if it is targeted to the right groups of unemployed workers.

Finally, our randomization design also involves three different types of JSA. Specially, 12 out of the 36 treated offices were randomly assigned to provide individual meetings with caseworkers, 12 offices gave distance meeting using the internet or telephone and 12 offices implemented group meetings. The results show statistically significant employment effects for individual meetings and distance meetings, but not for group meetings. This evidence relates to recent evidence on group meetings. Maibom et al. (2017) analyze three types of treatments in an experimental setting, and also find effects for individual meetings but not for group meetings. However, in their case each treatment is 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 very frequent group meetings in the form of search clubs with meetings several times a week.[4]

[4]Gartell (2015) presents Swedish experimental evidence on the effect of group meetings for unem-

Section 2 details the experiment and Section 3 presents the data sources. Section 4 gives the results for the early frequent meetings and examines mechanisms. In Section 5, we present the displacement effects and contrast displacement of resources and displacement of jobs. Section 6 concludes.

2 The experiment

The experiment involved an increased frequency of meetings between the job seekers and caseworkers at the Public Employment Service (PES) during the first three months of an unemployment spell.[5] It was conducted in 2015 and involved newly unemployed workers in the periods March 9–May 31 and August 17–November 17. The meetings given within the experiment were of three kinds: individual meetings with caseworkers, distance meetings using the internet or telephone and group meetings.

We were allowed to set up the experiment at 72 local public employment offices (roughly one fourth of all offices in Sweden). An overall consideration was to sample offices that serve one entire local labor market to facilitate estimation of displacement effects.[6] This mainly includes offices in medium sized cities. However, to achieve geographical dispersion we also selected some offices in the three metropolitan areas, where there are several offices in the same local labor market. But, here unemployed job seekers which were less likely to compete for the same jobs, were chosen by, e.g., picking offices in southern and northern Stockholm or by picking different group- or industry-specific offices. This resulted in 18 metropolitan offices and 56 non-metropolitan offices.

To these offices we applied a *two-level randomization* strategy. We first randomized over the 72 offices in the following way: 12 offices offering individual meetings, 12 offices offering group meetings, 12 offices offering distance meetings and 36 non-treated offices offering the baseline service to all unemployed workers. To achieve a better balanced sample of treated and non-treated offices we used a stratified randomization design. To this end, we used a model developed by the PES, which divides all offices into clusters of offices facing similar economic and demographic conditions. Using this model all offices were stratified into sixfolds and the randomization was performed within each

ployed and finds no long-run effect but a small reduction in the number of days in unemployment over a limited follow-up horizon.

[5]The experiment is presented in detail in an implementation report prepared by the PES (PES (2017)).

[6]Local labor markets are constructed to minimize job commuting over labor market borders. We used the 2012 classification, which divides Sweden into 75 local labor markets. We also excluded very small local labor markets (monthly inflow less than 20 workers), because with a low inflow it is difficult to construct meaningful groups.

strata (1 individual, 1 group, 1 distance and 3 non-treated offices). Within the 36 treated offices we then randomized the intensified meetings (using date of birth). Here, the intensity of treatment was set to 50%.[7] The two-level randomization solves two important identification problems. First, randomization over individuals ensures that characteristics are equally distributed among the treated and the non-treated within the treated offices. Second, the randomization over offices enables us to identify any displacement effects by comparing outcomes of non-treated individuals at treated and non-treated offices.

All meetings were given on top of the baseline treatment, which normally means between two and three meetings within the first three months. This includes a registration meeting with a caseworker rather soon after registration and after this roughly one meeting every quarter. The *baseline* treatment, hence, does not entail very frequent meetings between job seekers and the PES. The *individual meetings* meant that the assigned unemployed job seekers should have three extra meetings with caseworkers within the first three months of unemployment. The caseworkers were instructed to design the meetings according to the needs of each job seeker. The meetings could be about matching, motivation and/or coaching, The caseworker could, for example, suggest job search activities or vacancies to apply for, participation in recruitment fairs or participation in training or education. The *distance meetings* could take place either using the internet or by telephone for job seekers without access to a computer. In all other respects, the distance meetings were supposed to be similar to the individual meetings, with three extra meetings during the first three months and similar content.[8]

The *group meetings* used a more specific protocol. The basic idea was that job seekers and caseworkers should meet frequently (five seminars) in an initial stage. The seminars covered topics such as methods to write job applications, to create professional networks and how to present one self for a prospective employer. The groups consisted of 10–15 participants. After two weeks of seminars, the participants were divided into smaller groups, which were supposed to meet their caseworker on a weekly basis during two months. All meetings were compulsory.[9] Unfortunately, the information given to the treated individuals differed by type of meeting. Participants in the group meetings were informed about all five meetings already before the first meeting, while participants

[7]During the spring wave, the treatment intensity was randomly set to either 50% or 80%.

[8]One dimension of this was that caseworkers should be able to make binding decisions. To do this in a legally secure way, the job seeker used an e-ID if using IT and answered control questions in the telephone meetings.

[9]The only exception is the distance meetings during the spring wave. During this wave, distance meetings were only possible for unemployed with access to a computer with internet connection and an e-ID. Hence, unemployed failing to meet these conditions were exempt from the distance meetings.

in the distance and individual meetings typically were summoned by caseworkers to one meeting at the time.

The *target group* for the treatments was the inflow of unemployed job seekers, excluding those who had been registered as job seekers in the last three months and excluding persons going into the introduction programme for newly arrived immigrants. Individuals who were about to enter a job within a month after the inflow were also excluded from the treatments.

3 Data

Our analysis benefits from access to very rich data, collected by the Swedish Public Employment Service. First, we have detailed information about individual characteristics as well as daily registrations of unemployment status for all individuals registered at the 72 offices in the experiment. This data also includes all participation in ALMPs. We define unemployment as being registered as openly unemployed or participating in active labor market programs. Our primary outcome variable is the number of days registered as unemployed at the PES during the first year since registration. We also analyze the registration status at specific points in time, by months since registration.

Second, we have rich information about all meetings between the caseworkers and the unemployed workers. This includes information on the extra meetings given within the experiment, but also all other contacts and meetings, for all unemployed individuals. The information about the experiment specific meetings includes information about the summons to meetings and participation in meetings specific to the program. This information is used to describe the quality and quantity of the experimental treatment. We also use this information to construct caseworker characteristics. To measure the work load we count the number of meetings per month and the number of unique clients that the caseworker meets each month. To measure tenure we count the number of days between the current meeting and the first registered meeting with a job seeker after 2010, both overall and within the PES office where the caseworker currently works.

Third, we are among the first to use data from monthly activity reports from unemployed job seekers. Since September 2013, job seekers are required to submit a monthly report on their job search activities to the PES. Failure to provide a report or submitting a report indicating too low levels of search activity may lead to sanctions for those eligible for unemployment benefits. This data is used to characterize the effects of more frequent meetings on job search behavior and includes information about job applications, job interviews, unsolicited applications, vacancy proposals and vacancy referrals.

4 Treatment effects of intensified meetings

In this section, we analyze the data as is done in most experimental programme evaluations: we estimate treatment effects by comparing outcomes of targeted and non-targeted unemployed job seekers at the treated offices. We focus on the intention-to-treat (ITT) effect of early frequent meetings. To do this, we assign treatment status to all individuals in the target population according to the treatment protocol of the experiment.

4.1 Implementation

We first check if the treatment groups are balanced. Table 1 presents group averages for the treated (Column 1) and non-treated (Column 2), and p-values for tests of equality of the two averages (Column 3). We see no significant difference between the treated and non-treated, suggesting that randomization has worked. Table A-1 in the appendix shows that also the sub-samples by type of meeting are well balanced. We next examine the take-up, defined as being summoned to a meeting *or* participating in at least one program specific meeting. Panel A of Table 2 shows that on average 23% of the target population were treated and that the take-up was lowest for the group meetings (16%).[10,11]

Panel A of Table 2 also reports the number of meetings obtained by the treated and non-treated. A number of other features are worth mentioning. First, the treated received a significantly larger number of meetings during the experiment period (months 1–3), but not after the experiment period (months 4–6). This holds for all types of treatments aggregated as well as for all treatments individually. On average, the treated received 0.5 extra meetings, which, given the take-up rate of 23%, implies that those who actually participated in the experiment, received about two extra meetings. The intended number of meetings was three, so given that a substantial fraction of the

[10] The implementation report shows that the main explanation to the low take-up is that the local PES offices simply did not provide the treatment. Other explanations: some individuals found a job before the start of the treatments, two offices opted out of the experiment, problems with identifying the target population during the experiment and incorrect office definitions during the experiment. (PES (2017))

[11] In columns (4) and (5) we examine the characteristics of compliers and non-compliers. It suggests that the compliers were drawn from a more advantaged part of the distribution (higher education and more natives).

Table 1: Sample statistics for treated and non-treated in treated offices

Variables	Treated (1)	Non-treated (2)	p-val diff (3)	Compliers (4)	Non-compliers (5)
Age	33.333	33.392	0.705	35.389	32.696
Male	0.542	0.539	0.571	0.544	0.542
Unemployment benefits	0.642	0.638	0.546	0.761	0.605
Disabled	0.052	0.052	0.931	0.035	0.057
Matchable	0.868	0.862	0.117	0.920	0.852
Less than high school	0.224	0.223	0.873	0.174	0.239
High school	0.491	0.493	0.833	0.507	0.487
College	0.285	0.285	0.932	0.320	0.274
Born in Sweden	0.678	0.667	0.059	0.721	0.664
Born in the nordic countries	0.013	0.015	0.197	0.014	0.013
Born in west Europe	0.036	0.034	0.256	0.032	0.038
Born outside west Europe	0.273	0.284	0.035	0.232	0.285
Unemp. days year t-1	30.657	30.425	0.767	32.136	30.198
Unemp. days year t-2	67.422	68.493	0.449	70.903	66.343
Unemp. days year t-3	69.570	71.826	0.133	76.303	67.485
Unemp. days year t-4	63.822	63.678	0.921	73.240	60.905
Unemp. spells year t-1	0.431	0.440	0.427	0.405	0.439
Unemp. spells year t-2	0.789	0.800	0.518	0.783	0.791
Unemp. spells year t-3	0.806	0.815	0.630	0.838	0.796
Unemp. spells year t-4	0.706	0.721	0.397	0.797	0.678
No. spells, last 4 yrs					
Labor market educ.	0.024	0.020	0.161	0.029	0.022
Preparatory educ.	0.048	0.047	0.869	0.042	0.049
Labor market training	0.027	0.030	0.389	0.027	0.027
Subs. empl.	0.106	0.108	0.711	0.140	0.095
Observations	14,075	12,463	26,538	3,196	10,879

Notes: Summary statistics by treatment status, weighted by the intention to treat share, i.e., the observed share of individuals at the local PES office who would be randomized to treatment. This corrects for the different shares in the spring (50 and 80 percent) as well as for random differences between the offices (e.g. one office having 48 percent and another having 52 percent treated for a treatment share of 50 percent).

unemployed found a job within three months, what we see is consistent with what should be expected if the offices followed the treatment protocol.

Second, from Panel B, we see that individuals assigned to individual meetings and group meetings received significantly more physical meetings, while individuals assigned to distance meetings actually received more distance meetings.[12] Third, Panel C shows that the extra meetings are fairly evenly distributed over the first three months, the notable exception being that, as expected, there are more group meetings during the first two months, but not thereafter. Fourth, those assigned to the individual meetings are

[12]All treatment assignments are associated with more distance meetings, but the association is by far the strongest for distance meetings group. The association between being assigned to individual meetings and the number of distance meetings is weak and only borderline significant.

Table 2: Program participation and meetings at treated offices (ITT effects)

Variables	All (1)	Individual (2)	Distance (3)	Group (4)
Panel A: Overall				
Programme participation	0.229***	0.263***	0.250***	0.163***
	(0.004)	(0.006)	(0.007)	(0.006)
Control mean	0.00662	0.00394	0.0109	0.00568
Meetings month 1–3	0.503***	0.421***	0.413***	0.716***
	(0.026)	(0.039)	(0.046)	(0.051)
Control mean	3.159	3.150	3.301	3.019
Meetings month 4–6	-0.013	0.009	-0.039	-0.017
	(0.021)	(0.033)	(0.038)	(0.038)
Control mean	1.175	1.152	1.247	1.131
LMP participant month 1–3	0.004	0.011**	-0.007	0.007
	(0.003)	(0.005)	(0.005)	(0.005)
Control mean	0.0663	0.0674	0.0701	0.0609
Panel B: Type of meeting				
Physical meetings month 1–3	0.321***	0.381***	-0.021	0.605***
	(0.021)	(0.032)	(0.034)	(0.043)
Control mean	2.262	2.287	2.330	2.155
Distance meetings month 1–3	0.182***	0.041*	0.434***	0.111***
	(0.015)	(0.021)	(0.029)	(0.027)
Control mean	0.897	0.863	0.971	0.864
Panel C: Time pattern				
Meetings month 1	0.193***	0.123***	0.152***	0.339***
	(0.014)	(0.020)	(0.024)	(0.030)
Control mean	2.115	2.097	2.175	2.076
Meetings month 2	0.205***	0.165***	0.144***	0.326***
	(0.012)	(0.019)	(0.021)	(0.026)
Control mean	0.584	0.585	0.640	0.523
Meetings month 3	0.105***	0.133***	0.117***	0.052***
	(0.011)	(0.017)	(0.020)	(0.018)
Control mean	0.460	0.468	0.485	0.420
Observations	26,538	10,567	8,259	7,712

Notes: The results are from a linear regression of each meeting variable on a treatment indicator, weighted by the observed intention to treat share. The sample only includes the treatment PES offices. The control variables include the variables in Table 1.

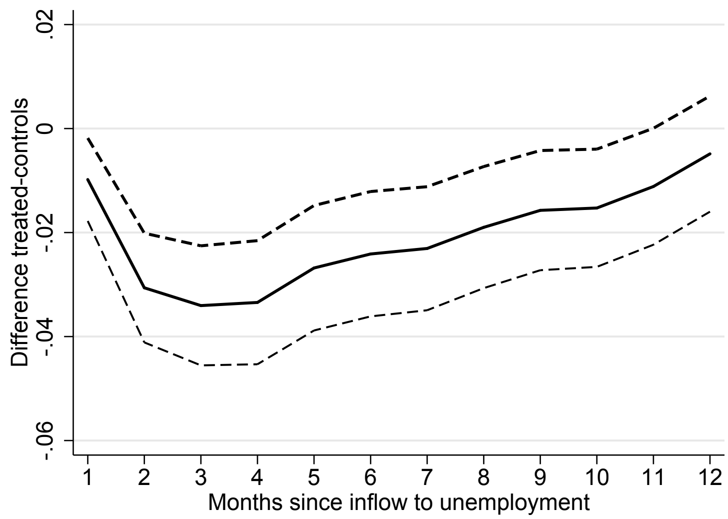

Figure 1: Difference between treated and non-treated in the share unemployed, by months since inflow to unemployment

significantly associated with increased participation in active labor market programs. By and large, hence, this evidence is consistent with what should be expected in terms of relationships between treatment assignment and treatment actually received.

4.2 The direct effects of meetings

Figure 1 reports the difference in the share of unemployed between treated and non-treated by months since inflow to unemployment. The figure shows that the share of unemployed is consistently lower (some two to almost four percentage points) among the treated, and that the difference has its maximum after 3–4 months, i.e., around the time when the treatment stops. The fact that the treatments lead to increased employment are confirmed by the estimates in Table 3. Here, we use our main outcome of interest: the number of days registered as unemployed at the PES during the first year in unemployment. Column 1 reports estimates without any covariates and Column 2 includes covariates, leading to very similar results. Looking at our preferred estimates in Column 2, we see that the treatments on average decrease the number of days in unemployment by 5.9 days during the first year, estimated with considerable precision. In Column 3, we present results from a placebo regression, where we pretend that the experiment took place in 2014 instead of 2015, but, as expected, the estimated placebo effect is insignificant and very close to zero.

Figure A-1 in the appendix, next, shows how the effects on the share of unemployed by type of meeting evolves over time. Here, we see a clear difference between on the

Table 3: Effects of intensified meetings on unemployment (ITT effects)

Variables	(1) No controls	(2) Controls	(3) Placebo
Assigned to program	-6.692***	-5.890***	-0.973
	(1.527)	(1.414)	(1.384)
Controls	No	Yes	Yes
Control mean	196.1	196.1	189.6
Observations	26,538	26,538	26,841

Notes: The results are from a linear regression of days registered as unemployed 1 year since registration at the PES on a treatment indicator, weighted by the observed intention to treat share. The sample only includes the treatment PES offices. The controls include the variables in Table 1.

one hand individual and distance meetings and on the other hand group meetings, where the former two are associated with significant employment effects, whereas we essentially se no significant effects for group meetings. Moreover, the aggregate pattern of the largest treatment effects early in the unemployment spell is clear also for each of the different treatments as long as we consider the treatments with significant effects.

The treatment effects appear so early that we suspected that they, at least partly, could reflect pre-program effects. Due to dynamic selection issues, such effects are hard to identify credibly. However, if we plot the meetings rate and the treatment effects, we see that the effect only appear after the first meeting, to that pre-program effects are unlikely to be a main explanation to the observed pattern.

4.3 Why do the meetings help the unemployed workers?

The analysis has shown that intensified meetings with a caseworker at the local employment office reduce the number of days in unemployment. This effect may arise through a number of possible mechanisms. The caseworker may provide monitoring of search behavior that affects search intensity, provide information about vacancies that affects the direction of search, and provide job search related support and education that affect search channels and enhances the job search technology.

To shed light on the mechanisms behind the effects of intensified meetings, we make use of the monthly activity reports that the job seekers submit to the PES. The reports include information on all job search related activities undertaken by the unemployed workers. We include reports during the first three months in unemployment, i.e., the period during which the experimental treatment took place.[13] Table 4 presents the

[13] We focus on the reports for individuals who were supposed to report their activity that month.

results. Panel A shows that the probability of reporting for the job seekers who were supposed to report is about 56 percent, but that there is no significant difference between the treatment and the control group. Hence, we have no reason to believe that there is systematic sample selection that could potentially bias the results. The remaining panels show the average number of activities per report.

Panel B in Table 4 shows no effects of intensified meetings on search intensity. There are no significant differences between the treated and the non-treated regarding the number of total activities reported or the number of total job applications. Panel C shows, however, that the direction of search has changed. Whereas there are no significant differences in the number of own-initiated job applications, program participants are significantly more likely to apply to vacancy proposals and vacancy referrals provided by the caseworker.[14] The difference is significant for individuals assigned to individual meetings and distance meetings, but not for individuals assigned to group meetings. This suggests that the mechanism behind the effect of intensified meetings is that the caseworker provides specific information about vacancies, and that group meetings do not work as well as individual and distance meetings since they do not involve specific information about vacancies.

We next examine search channels and contrast formal and informal job search. In our data we have information on informal job search in the form of unsolicited job applications made to firms with no open vacancy. This includes more formal applications as well as notices of interest made during more informal visits to a firm. Besides these unsolicited job applications we also study effects on other job-enhancing activities. Even though this does not capture all aspects of informal search activities, it will provide some insights on the effects on informal search. Interestingly, Panel D in Table 4 shows no evidence of changes in the number of unsolicited job applications and other job-enhancing activities, suggesting that there were no effects of the early meetings on search channels and job search technology.

This includes individuals who are still unemployed when the report is supposed to be submitted, and who are indicated in the PES registers to be supposed to report.

[14]The point estimate for applications to job vacancy referrals in fact implies a doubling of this activity.

Table 4: Effects of intensified meetings on search behavior and vacancy referrals

Variables	(1) All	(2) Individual	(3) Distance	(4) Group
Panel A: Activity reporting				
P(report)	-0.007	0.006	-0.016	-0.014
	(0.006)	(0.010)	(0.011)	(0.011)
Control mean	0.557	0.547	0.573	0.554
Observations	46,543	19,319	13,976	13,248
Panel B: Search intensity				
Total activities	0.121	0.158	-0.116	0.278
	(0.149)	(0.237)	(0.261)	(0.287)
Control mean	6.672	6.544	6.748	6.773
Total job applications	0.053	0.084	0.027	0.054
	(0.115)	(0.184)	(0.194)	(0.224)
Control mean	3.603	3.517	3.700	3.622
Panel C: Information about vacancies				
Own-initiated job applications	0.002	0.026	-0.051	0.039
	(0.113)	(0.182)	(0.190)	(0.221)
Control mean	3.391	3.324	3.430	3.445
Applications to job vacancy proposals	0.037***	0.041**	0.060**	0.008
	(0.012)	(0.019)	(0.024)	(0.017)
Control mean	0.197	0.176	0.251	0.168
Applications to job vacancy referrals	0.014***	0.016***	0.017***	0.007
	(0.004)	(0.006)	(0.006)	(0.006)
Control mean	0.015	0.016	0.019	0.009
Panel D: Search channels				
Unsolicited job applications	-0.038	-0.086	-0.002	-0.029
	(0.052)	(0.088)	(0.100)	(0.075)
Control mean	1.174	1.288	1.133	1.054
Other job-enhancing activities	0.108	0.185*	-0.111	0.199
	(0.066)	(0.103)	(0.111)	(0.130)
Control mean	1.576	1.434	1.609	1.746
Observations	25,773	10,653	7,828	7,292

Notes: The results are from a linear regression of each meeting variable on a treatment indicator, weighted by the observed intention to treat share. The sample only includes the treatment PES offices. The controls include the variables in Table 1.

5 Displacement effects

5.1 The displacement effects of meetings

We now turn to our empirical analysis of displacement effects of intensified meetings. The foundation of this analysis is our two-stage randomization over both individuals within treated offices and over treated and non-treated offices. The idea is straightforward. Because of randomization, we would expect that in the absence of treatment, the non-treated individuals in treated offices would have the same average outcomes as the non-treated in non-treated offices, so that any differences between those two groups capture the displacement effects. Specifically, for our main outcome variable days in unemployment during the first year since registration, our model for individual i in office f is:

$$Y_{if} = \alpha + \beta_1 1(\text{Assigned to program}_{if}) + \beta_2 1(\text{In a program area}_f) + \varepsilon_{if}, \qquad (1)$$

where $1(\text{In a program area}_f)$ indicates a treated office and $1(\text{Assigned to program}_{if})$ indicates treatment status for individuals in treated offices. That is, those who would have received the treatment if the take-up would have been complete. Thus, as in the previous analysis, we estimate ITT effects. The displacement effect is given by β_2, which is identified through the comparison of non-treated individuals at treated and non-treated offices. β_1 captures the direct comparison of treated and non-treated individuals in the treated offices, i.e. the same parameter as we studied in Section 4. The net effect for the treated is given by $\Delta \equiv \beta_1 + \beta_2$.

Before we present the results from this model, we check whether the randomization seems to have worked. To this end, Table 5 compares average worker characteristics for non-treated workers in treated offices (Column 1) and non-treated offices (Column 2). From the tests for a difference between these averages (p-values in Column 3), we see that all differences are insignificant. Hence, we have no indication that the randomization over offices did not work satisfactorily.

We now turn to the estimates of model (1), reported in Column 1 of Table 6. As expected is β_1 very similar to the corresponding estimate from Table 3. The displacement effect, β_2, is statistically insignificant, but it is also very imprecisely estimated and sizable in comparison with β_1, so that the estimates only indicate a very small net effect on employment (β_1+β_2). In Column 2, we estimate a placebo version of model (1), pretending that the experiment occurred in 2014 instead of 2015. These placebo results are reassuring, in that they do not suggest any significant displacement in the

Table 5: Sample statistics for non-treated individuals in treated and non-treated offices

Variables	(1) Treated offices	(2) Non-treated offices	(3) p-val	(4) Diff reform	(5) p-val
Age	33.362	33.539	0.637	-0.465	0.005
Male	0.540	0.555	0.186	-0.004	0.537
Unemployment benefits	0.640	0.634	0.748	-0.021	0.001
Disabled	0.052	0.054	0.603	-0.003	0.314
Matchable	0.865	0.864	0.936	-0.012	0.161
Less than high school	0.223	0.225	0.879	-0.002	0.725
High school	0.492	0.486	0.691	0.005	0.452
College	0.285	0.289	0.845	-0.003	0.548
Born in Sweden	0.672	0.641	0.400	0.001	0.882
Born in the nordic countries	0.014	0.015	0.547	-0.001	0.313
Born in west Europe	0.035	0.036	0.857	-0.001	0.480
Born outside west Europe	0.279	0.308	0.385	0.002	0.784
Unemp. days t-1	30.54	30.98	0.777	-0.886	0.141
Unemp. days t-2	67.96	68.25	0.915	0.106	0.941
Unemp. days t-3	70.70	71.22	0.868	1.251	0.441
Unemp. days t-4	63.75	66.13	0.439	-0.867	0.409
Unemp. spells t-1	0.436	0.442	0.793	-0.015	0.131
Unemp. spells t-2	0.794	0.793	0.972	-0.011	0.457
Unemp. spells t-3	0.811	0.819	0.841	-0.012	0.502
Unemp. spells t-4	0.714	0.716	0.960	0.007	0.638
No. spells, last 4 yrs					
Labor market educ.	0.022	0.022	0.976	-0.004	0.105
Preparatory educ.	0.047	0.047	0.988	-0.002	0.651
Labor market training	0.028	0.031	0.370	-0.001	0.821
Subs. empl.	0.107	0.106	0.920	0.005	0.374
Observations	26,538	31,241	57,779	552,818	552,818

Notes: Standard errors are clustered at the PES office level. Columns (1)–(2) show summary statistics for individuals at the treated and non-treated offices during the experiment in 2015, and Column (3) shows the associated p-values of the difference. Column (4) shows the β_2-coefficient from estimating equation (2), excluding the covariates in $\mathbf{X}$, on each covariate in the table, and Column (5) shows the associated p-value of the coeffient.

pre-treatment period. However, the standard errors are pretty large.

All this suggests that we have a problem with precision. To improve on the precision, we, therefore, use data from time periods before the experiment.[15] The idea is to use pre-experiment data to capture heterogeneity at the office level and use this to improve on the precision of our estimates for the experimental period. To this end, we sample newly unemployed workers in 2012–14 in exactly the same way as during the experiment in 2015. We also use data from non-experimental periods before, in between and after

[15] We use three years of pre-data, back until 2012. The reason for this is that several reforms were introduced between 2011 and 2012, including public investments in many more caseworkers and a new mandate for the PES to also provide services to newly arrived immigrants.

Table 6: Displacement and direct effects of intensified meetings

	(1)	(2)	(3)	(4)	(5)	(6)
	Experiment period		Event study approach, 2012–2015			
Variables	Progress period	Placebo period	No controls	Controls	Office dummies	Time trend
Assigned to program	-6.692***	-0.784	-7.036***	-6.373***	-5.990***	-5.990***
	(1.789)	(1.847)	(1.997)	(1.521)	(1.359)	(1.359)
In a program area	5.457	2.806	4.805**	4.682**	4.164**	3.182*
	(5.799)	(4.952)	(2.178)	(1.910)	(1.727)	(1.744)
Year dummies	No	No	Yes	Yes	Yes	Yes
Month dummies	No	No	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes	Yes
PES office dummies	No	No	No	No	Yes	Yes
Time trend	No	No	No	No	No	Yes
Mean of dep. var	190.7	186.8	187.0	187.0	187.0	187.0
Observations	57,779	60,014	552,818	552,818	552,818	552,818

Notes: Regression of our main outcome variable (time registered as unemployed during 1 year since registration at the PES) on an indicator for treatment PES office ("In a program area") and an indicator for treatment PES office$\times$ intention to treat status is treated ("Assigned to program"). The sample includes all offices: treatment offices and super control offices. So the excluded category is the super controls. The controls include the variables in Table 1. Standard errors are clustered at the PES office level.

the two experiment periods in 2015. Specifically, the pre-period information is exploited in a difference-in-differences (DiD) analysis. Our model for individual i at office f in period t is:

$$\begin{aligned} Y_{itf} &= \lambda_f + \lambda_t + \beta_1 1(\text{Assigned to program}_{itf}) \times 1(\text{Experiment period}_t) + \\ &\quad \beta_2 1(\text{In a program area}_{tf}) \times 1(\text{Experiment period}_t) + \mathbf{X}\gamma + \varepsilon, \end{aligned} \tag{2}$$

where, in addition to previously introduced notation, λ_f are office fixed effects and λ_t time fixed effects (year and month fixed effects), which captures office and time heterogeneity with the aim to improve on precision. $1(\text{Experiment period}_t)$ takes the value one in the experiment period. Thus, as before, the displacement effect is given by β_2 and the net effect for the treated is given by $\Delta \equiv \beta_1 + \beta_2$. As another way to improve precision we also include individual control variables. We also estimate models where we include treatment group specific time trends, i.e., we allow for separate trends for the treated and non-treated offices, leading to very similar results.

Before we present estimates from this model we analyze the patterns in the data graphically. Figure 2 shows trends in our outcome variable for the differences between treated and non-treated individuals in treated offices as well as between non-treated individuals at treated and non-treated offices. Before the treatment periods, there are no clear patterns in the differences, whereas the differences move systematically in opposite directions in the experiment periods. During the experiment treated individuals have a lower unemployment rate and non-treated in the same offices have a higher unemployment rate. This pattern is consistent with displacement effects – we only observe displacement effects for the non-treated when we see treatment effects for the treated.[16]

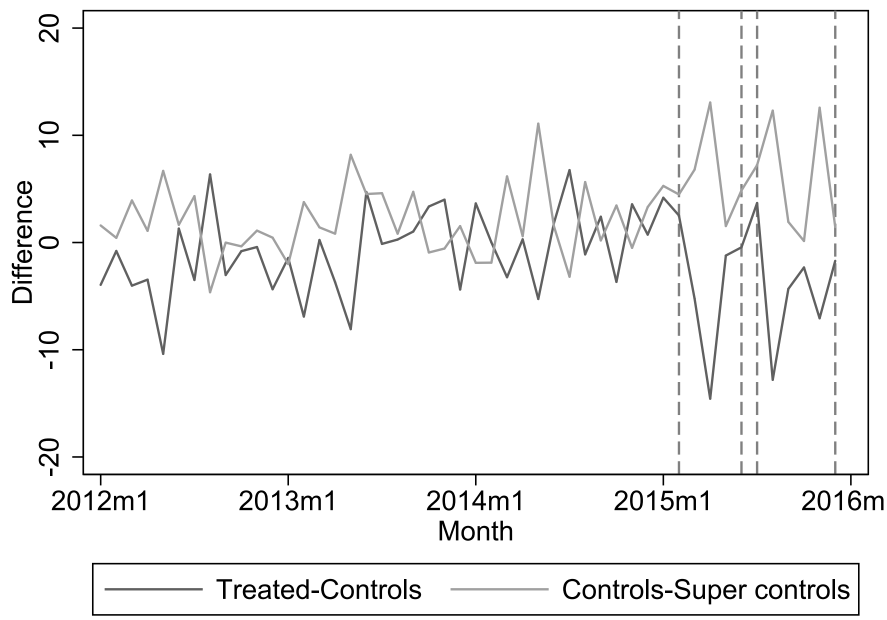

Figure 2: Differences in unemployment between treated and non-treated in active offices and non-treated at non-active offices

The displacement effects observed in Figure 2 are confirmed by the estimates of equation (2) presented in Columns (3)–(6) of Table 6. In these columns we stepwise expand our model: Column 3 includes time fixed effects, Column 4 adds individual characteristics, Column 5 also include office fixed effects, and in Column 6 we also include time trends. From this we see that the estimated effects are fairly unchanged across the specifications. In all cases, we find a sizable and significant displacement effect. Our preferred estimates in Column 6 indicate that unemployment among the non-treated individuals at treated offices increase with on average 3.2 days. This displacement effect can be compared to the net effect for the treated which is –2.8 days ($\Delta \equiv -(\beta_1 + \beta_2)$). The overall effect depends on the fraction of treated in the treated offices. In our sample, this fraction of treated is 53%, leading to a total effect very

[16]As an additional check, in Columns 4–5 of Table 5 we check whether there are significant changes in our covariates as a result of the reform. The main message is that this is not the case, even if there is a couple of statistically significant changes.

close to zero, and thus displacement effects very close to 100%.[17] These very extensive displacement effects are roughly in the same order of magnitude as the displacement effects found by Crépon et al. (2013).

5.2 Displacement through resource constraints or in the labor market?

Previous studies on displacement effects have focused on displacement at the labor market. If more frequent meetings with a caseworker makes the treated individuals more competitive and allows them to find jobs faster, this may happen at the expense of untreated individuals who were first in line to get those jobs. Displacement could also occur due to displacement of resources at the local employment office, if there are resource constraints due to funding limitations or delays in recruitment of new caseworkers. If the untreated individuals receive less than the baseline treatment before the experimental implementation, they may fair worse at the labor market because they are less well prepared than they would have been in the absence of the intervention. The policy implications of these two mechanisms for the displacement effects are quite different.

We begin by investigating displacement of resources in Table 7. Panel A shows that the significant increase in meetings of those assigned to treatment does not seem to have arisen at the expense of those not assigned to treatment in the treated offices. Panel B analyzes displacement of job vacancy proposals and referrals, since this was found to be an important mechanism for the direct effects of meetings in the previous analysis. The evidence suggests that the non-treated individuals did not receive fewer job vacancy proposals or referrals as a result of the experiment. Panel C investigates displacement of caseworker quality in two dimensions, using information about the caseworkers the individuals meet at the meetings that take place during the first three months of the unemployment spell. Columns 6 and 7 analyze differences in the work load of caseworkers, but find neither direct effects nor displacement effects in terms of the number of meetings per month or the number of unique clients per month. Columns 8 and 9 analyze differences in caseworker tenure at the PES and at the specific local employment office, but finds neither direct effects nor displacement effects regarding how experienced the caseworkers are. Overall, our results does not support that the estimated displacement is due to displacement of resources at the local employment

[17] Net effect for the treated equals -2.81 days. The effect for controls in treated offices equals 3.18 days. 53% of all individuals are treated. Hence, the total effect equals $0.53 \times -2.81 + 0.47 \times 3.18 = 0$ days. Hence, displacement is 100%.

Table 7: Displacement of meetings and services

Panel A: Displacement of meetings				
	(1) Meetings	(2) Physical meetings	(3) Distance meetings	
Assigned to program	0.498***	0.318***	0.180***	
	(0.052)	(0.056)	(0.047)	
In a program area	-0.019	-0.053	0.034	
	(0.049)	(0.038)	(0.023)	
Observations	552,816	552,816	552,816	
Panel B: Displacement of job vacancy proposals and referrals				
	(4) Place-ment proposal	(5) Job refer-ral		
Assigned to program	0.035***	0.013		
	(0.012)	(0.008)		
In a program area	-0.009	-0.002		
	(0.010)	(0.003)		
Observations	288,364	288,364		
Panel C: Displacement of good caseworkers				
	(6) No. meetings/ month	(7) No. clients/ month	(8) Tenure at PES in days	(9) Tenure at local office in days
Assigned to program	1.257	1.200	2.613	-3.737
	(1.254)	(1.055)	(8.514)	(11.474)
In a program area	0.555	1.716	-14.984	-7.435
	(5.660)	(3.606)	(18.996)	(25.325)
Observations	1,717,724	1,717,724	1,717,724	1,717,724

office.

We now turn to evidence of displacement taking place at the labor market. Crépon et al. (2013) set up a theoretical model that predicts that displacement is higher in weak labor markets and provide empirical support for this prediction. Table 8 reproduces this analysis in our setting. We estimate our main model with the local unemployment to population ratio as an additional regressor. In addition, we divide all offices according to whether the local unemployment rate is above or below median unemployment among the Swedish municipalities and interact this binary variable with treatment status.[18] The results are striking. The direct effect is similar in both types of labor markets, but the displacement effect is small and insignificant in low-unemployment municipalities,

[18]The local unemployment rate is the unemployment rate in the municipality that the local employment office belongs to. It is allowed to vary by month.

Table 8: Displacement effects of intensified meetings in strong and weak labor markets

	(1)
Assigned to program*Below median unemployment rate	-5.267***
	(1.941)
Assigned to program*Above median unemployment rate	-6.674***
	(1.947)
In a program area*Below median unemployment rate	0.918
	(2.326)
In a program area*Above median unemployment rate	7.280***
	(2.466)
Observations	552,818

Notes: Regression of our main outcome variable (time registered as unemployed during 1st year since registration at the PES) 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. The sample includes all offices: treated and non-treated offices. So the excluded category is the non-treated. The control variables include the variables in Table 1 as well as the monthly unemployment rate in the PES office municipality. Standard errors are clustered at the PES office level. Each column includes the treated offices assigned to the specific type of treatment (individual, distance or group) and all non-treated offices.

and large and significant in high-unemployment municipalities.

A possible interpretation of this pattern is that treatment is gainful for those who get it irrespective of the labor market conditions, e.g., by receiving information about vacancies that the job seeker would otherwise not have applied to. This is harmful for those who would otherwise be first in line for those jobs, but the harm is limited under good labor market conditions, when access to alternative job offers is good.

To shed more light on the mechanisms behind our estimated displacement effects, we also estimate separate treatment and displacement effects for a few groups of participants in Table 9.[19] This analysis is also informative in terms of potential targeting of intensified meetings to certain groups of the population. In the educational dimension, there are no clear differences in the effects comparing groups with and without college education. In terms of immigrant status, on the other hand, the treatment effect is considerably larger for non-western immigrants, whereas the displacement effect is evenly distributed across individuals with different birth countries. Finally, dividing the treated by predicted unemployment duration based on the individual characteristics in Table 1 again suggests that the direct effects are the largest for individuals furthest away from the labor market, whereas the displacement effects are evenly distributed among the three groups.

[19]The estimated models are specified as the model in column (4) in Table 6 for each group.

Table 9: Heterogeneous displacement effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Education level		Birth country		Predicted unemployment duration		
	Non-college	College	Sweden and west EU	Non west EU	P0–33	P33–67	P67–100
Assigned to program	-6.291***	-5.463**	-3.939**	-11.69***	-5.172**	-3.116	-9.549***
	(1.957)	(2.078)	(1.810)	(3.827)	(2.526)	(1.892)	(2.819)
In a program area	4.080*	3.843*	3.523*	4.744*	5.308**	2.204	4.153*
	(2.160)	(2.165)	(1.845)	(2.597)	(2.090)	(2.588)	(2.235)
Observations	402,836	149,982	413,278	139,540	182,429	182,430	187,959
Total effect	0.933	1.113	1.537	-0.976	2.697	0.642	-0.576
	(1.771)	(1.867)	(1.466)	(2.392)	(1.653)	(2.230)	(1.980)

Notes: Regression of our main outcome variable (time registered as unemployed during 1 year since registration at the PES) 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"). The sample includes all offices: treatment offices and super control offices. So the excluded category is the super controls. The control variables include the variables in Table 1. Standard errors are clustered at the PES office level. Separate regressions for different subsamples of unemployed.

Summing up, there is a tendency that groups with more unfavourable labor market prospects gain the most from treatment at the expense of job seekers from all groups. This supports the interpretation that increased information about vacancies broadens the search of the job seekers, in particular among those far from the labor market, and that this crowds out employment of individuals who were first in line for those jobs, typically among those close to the labor market. Although the estimated displacement effects are very large, such heterogenous effects could motivate targeted interventions towards job seekers with the most disadvantageous labor market prospects.

In the next step of the analysis, we will directly study whether the treated job seekers broaden their geographical or occupational area, by adding information on search area at the time of registration and combining it with the search area of the job vacancy proposals and referrals suggested by the caseworker. This will reveal if the caseworkers make the treated look for other jobs than they would otherwise have done.

6 Conclusions

This paper has evaluated a large-scale randomized experiment conducted in Sweden in 2015, specifically designed to capture displacement effects. The treated in the experiment received intensified meetings with caseworkers at the local public employment office. Our main findings are that the frequent meetings reduce the time spent in unemployment for the treated, but also that large displacement effects imply a zero net effect on unemployment. This is in line with previous evidence which have shown that more frequent meetings have positive effects for the treated, but also that the positive treatment effects to a large extent come at the expense of non-treated.

A novel feature of our analysis is that we are able to take a look into the treatment black box and discuss how the estimated impacts have arisen. Our main finding in this respect is that job vacancy proposals and referrals from caseworkers seem to be the main mechanism. Intensified early meetings give rise to more information about vacancies from caseworkers to treated job seekers, which increases employment for the treated at the expense of the non-treated. We also present evidence that suggests that this result is not an artifact of the experiment due to crowding out of resources from the controls, but instead seems to be driven by the competitive advantage at the labor market created by the increased numbers of job vacancy proposals and referrals to the treated.

We find that groups with more unfavorable labor market prospects gain the most from the intensified at the expense of job seekers from all groups. This is because the meetings involve information about vacancies which broadens the search of the weaker job seekers, and that this crowds out employment of individuals who were first in line for those jobs, typically among those with a stronger attachment to the labor market. We conclude that intensified meetings should be targeted at job seekers with the most disadvantageous labor market prospects.

We have also compared three types of meetings. Our results reveal significant employment effects for individual meetings and distance meetings, but not for group meetings. Our explanation to these results are that group meetings are ineffective because they do not involve vacancy proposals and referrals given to specific unemployed workers about specific vacancies.

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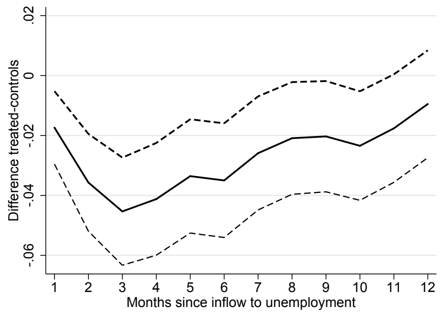
Appendix: Additional Tables and Figures

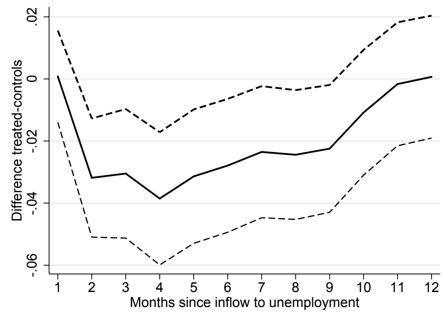
Table A-1: Sample statistics for treated and non-treated for each type of meeting

Variables	Individual meetings (I)			Distance meetings (D)			Group meetings (G)		
	T	C	p-val	T	C	p-val	T	C	p-val
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	33.008	33.298	0.231	33.080	33.034	0.869	32.916	32.979	0.624
Male	0.544	0.531	0.173	0.549	0.542	0.525	0.560	0.555	0.230
Unemployment benefits	0.629	0.624	0.569	0.625	0.624	0.986	0.634	0.635	0.660
Disabled	0.055	0.058	0.510	0.049	0.048	0.813	0.059	0.054	0.461
Matchable	0.875	0.860	0.026	0.867	0.866	0.867	0.904	0.913	0.885
Less than high school	0.220	0.222	0.828	0.235	0.229	0.519	0.219	0.213	0.899
High school	0.495	0.489	0.552	0.481	0.488	0.566	0.511	0.508	0.622
College	0.285	0.289	0.647	0.283	0.283	0.974	0.270	0.278	0.508
Born in Sweden	0.665	0.641	0.010	0.657	0.652	0.620	0.754	0.739	0.953
Born in the nordic countries	0.013	0.015	0.271	0.010	0.013	0.178	0.017	0.020	0.887
Born in west Europe	0.039	0.037	0.597	0.029	0.037	0.039	0.032	0.034	0.000
Born outside west Europe	0.283	0.306	0.009	0.304	0.298	0.551	0.197	0.207	0.129
Unemp. days t-1	30.611	30.050	0.652	30.637	30.963	0.815	30.212	29.449	0.793
Unemp. days t-2	67.944	66.897	0.641	68.590	73.385	0.062	61.025	61.808	0.995
Unemp. days t-3	69.891	71.757	0.432	70.720	75.001	0.116	58.292	59.272	0.821
Unemp. days t-4	64.441	62.911	0.508	64.912	66.547	0.536	55.866	56.258	0.956
Unemp. spells t-1	0.416	0.433	0.319	0.434	0.444	0.588	0.447	0.455	0.819
Unemp. spells t-2	0.785	0.779	0.835	0.819	0.841	0.482	0.747	0.752	0.478
Unemp. spells t-3	0.829	0.800	0.320	0.809	0.873	0.055	0.689	0.719	0.965
Unemp. spells t-4	0.704	0.700	0.889	0.718	0.759	0.182	0.679	0.692	0.743
No. spells, last 4 yrs									
Labor market educ.	0.022	0.021	0.888	0.026	0.019	0.100	0.027	0.029	0.457
Preparatory educ.	0.049	0.040	0.150	0.053	0.060	0.371	0.026	0.024	0.749
Labor market training	0.031	0.031	0.939	0.027	0.028	0.789	0.032	0.034	0.224
Subs. empl.	0.113	0.110	0.746	0.096	0.106	0.339	0.057	0.057	0.865
Observations	5,108	5,459	10,567	4,587	3,672	8,259	13,908	6,955	7,712

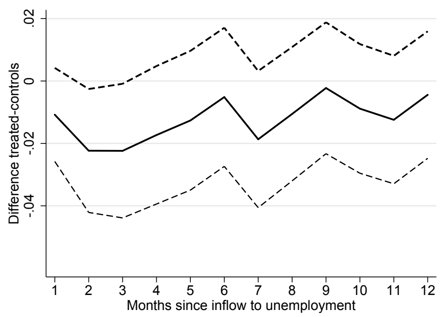
Note: Summary statistics by treatment status, weighted by the intention to treat share, i.e., the observed share of individuals at the local PES office who would be randomized to treatment. This corrects for the different shares in the spring (50 and 80 percent) as well as for random differences between the offices (e.g. one office having 48 percent and another having 52 percent treated for a treatment share of 50 percent).



(a) Individual meetings



(b) Distance meetings



(c) Group meetings

Figure A-1: Dierence between treated and non-treated in the share unemployed, by type of meeting