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Lower caseloads of placement officers and unemployment outcomes — New evidence from an Austrian field experiment*

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Abstract

We present results from a field experiment to test whether more intensive counseling service for the long-term unemployed improves their chances of finding employment. We find that more intensive counseling led to more counseling, more sanctions, and more assignment to active labor market programs. Matching the data of treated and control observations with administrative social security data, we find a significant increase of job starts, but also more withdrawals from the labor market. The experiment led to shorter unemployment durations and lower spending on unemployment benefits. A cost-benefit evaluation of the program suggests that more intensive counseling is cost-effective.

Keywords: job placement assistance; field experiment;
JEL classification: J64, J68

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1 Introduction

The caseload of employment officers in employment offices could influence the effectiveness of employment services. Hainmueller, Hofmann and Wolf (2016) show for Germany that a lower caseload led to shorter unemployment duration and more re-employment. Similar results were also found by Schiel, Schröder and Gilberg (2008). The caseload could limit the time and effort an unemployed person receives from the agency, and thus limit the effectiveness of the agency.

Although this is an important parameter for public policy, we have limited knowledge on how caseload influences the effectiveness of public employment services. Crépon, Duflo, Gurgand, Rathelot and Zamora (2013) find that unemployed youths who were randomly assigned to job placement assistance were significantly more likely to have found a stable job than those who were not.

We provide new evidence from a field experiment in an Austrian employment office. In 2015, the number of caseworkers was changed for randomly selected unemployed persons. For the unemployed who were born in January, February, and March, the number of caseworkers increased and this significantly reduced the caseload for the caseworkers. In consequence, there were more meetings between the unemployed and their caseworkers. The unemployed received more job offers from their caseworkers than the unemployed in a control group. They also received more offers for training and participated in more training programs than those in the control group.

We match the treated and control observations with their administrative social security records which provide detailed information on the persons' employment statuses. This allows us to analyze in detail how employment outcomes changed for the treated persons in the post-treatment period for a number of outcomes, such as re-employment wages or re-employment durations, which are typically not observed by unemployment agencies. The comparison with untreated unemployed job seekers from other unemployment offices allows the analyzes of potential externalities on other job seekers.

From a theoretical perspective, there are several reasons why more intensive counseling could improve the efficiency of employment agencies (Maibom, Rosholm and Svarer, 2017). For the majority of persons, unemployment is a rare event and counseling may help with search strategies and update information on the labor market. Counseling may also help the unemployed to focus on their job search through provision of information on support for e.g., child care or in case of financial difficulties. Caseworkers may help in identifying a lack of skills and target qualification programs better to the needs of the job seeker. Regular meetings may provide additional motivation and prevent withdrawal from the labor market (discouraged worker effect) (Maibom et al., 2017). Indeed, Card, Kluve and Weber (2010) suggest that job search assistance programmes are (weakly) more successful than other programme types, such as public sector jobs programmes.

Rosholm (2014) argues that intensive counseling is a relatively cheap tool to improve the reintegration of unemployment in the labor market, in particular, they are much cheaper than training programs. Our evaluation of the experiment also suggests that, from the agency's point of view, it has been cost-effective.

2 Background

The Austrian public employment service ("Arbeitsmarktservice", AMS) is a one-stop-shop for the unemployed. It administers unemployment benefits and unemployment assistance for those who are eligible, and provides also counseling and job offers for persons who are not eligible for benefits. The AMS organizes subsidized training programs for eligible persons. It is organized in 101 regional offices which are coordinated by nine provincial offices. A federal head office is responsible for management, controlling, evaluation, analysis, and strategic planning.

Unemployed workers are assigned to a regional office, typically the closest, by their residential zip code. Each of the 101 regional offices consists of three areas, an information zone, a service zone, and a counseling zone. (See Table 1 for an overview.) The information zone provides general labor market information to the public. It operates on self-service access and information can be obtained anonymously. The service zone is dedicated to newly registered unemployed whose claims for unemployment benefits are processed here. The unemployed receive counseling and job offers. They may be sanctioned by the case worker if they do not fulfill the requirements for obtaining benefits. The counseling zone supports the unemployed who have been unemployed for at least 6 months or who are hard to place, e.g., because of a criminal record. They receive more intensive guidance and assistance than in

Table 1: Structure of an Austrian regional unemployment office.

	Info Area	Service Area	Counseling Area
Target group	Public anonymous	New entrants "job-ready"	6 months + "hard-to-place"
(Main) Services	Information	Claims & Benefits	Guidance & Assistance
Mean caseload	Self-service	1:100	1:250

Note: Caseloads are averages for 2014.

the service zone. However, the caseload for a caseworker in the service zone is about 100 unemployed per month and it is about 1:250 in the counseling zone.

More intensive counseling could provide hard-to-place or long-term unemployed persons with better targeted support, which could result in better job matches. From a theoretical perspective, several channels exist which could improve the outcomes of unemployed persons (Hainmueller et al., 2016; Hofmann, Krug, Sowa, Theuer and Wolf, 2010, 2012; Hofmann, Kupka, Krug, Kruppe, Osiander, Stephan, Stops and Wolff, 2014; Rosholm, 2014). A lower caseload should result in more frequent meetings between a caseworker and her clients. Meetings serve both for the exchange of information about jobs offers and training programs, but also as a mechanism to motivate the unemployed. A lower caseload could provide casesworkers with more time to identify job offers or training programs which are suitable for their clients.

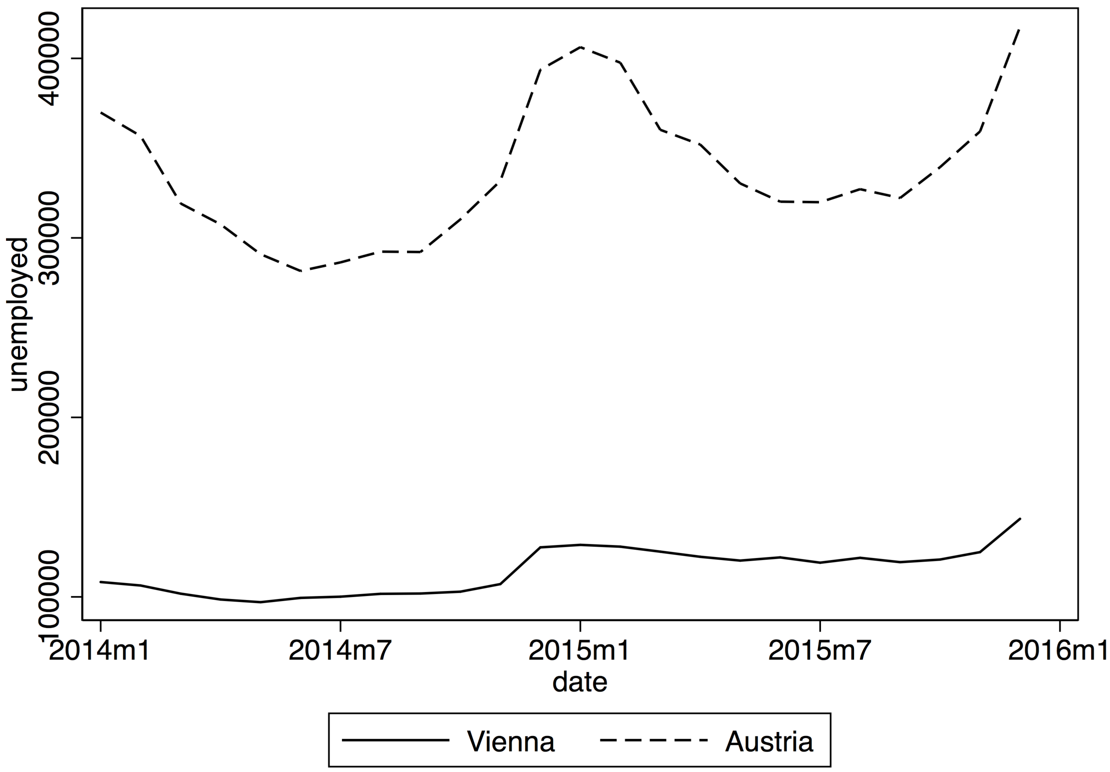
In addition, more time for individual meetings could allow the identification of problems to accepting suitable jobs (e.g., lack of child support), which could be solved by the caseworker. The information about available job offers could also help to form realistic expectations about the state of the labor market and the skills which are demanded by firms. Moreover, more frequent meetings between caseworkers and their clients could also lead to closer monitoring of the unemployed's search efforts and, if they are found to be lacking, could result in sanctions.

3 Experiment and Empirical Strategy

The randomized controlled trial was conducted in one of the twelve regional public employment offices in Vienna during 2015. The treatment changed the workload of caseworkers in the counseling zone by an administrative re-organization. The regional office has two teams in the counseling zone. Unemployed job seekers who were born between January and June are assigned to one team and those born between July and December are assigned to the other team. Before the trial, each team had about 22 full-time equivalents of caseworkers and, during September to November 2014, had an average caseload of about 1:250.

During 2015, each team obtained four additional caseworkers. For the trial, the caseload of the first team was reduced by limiting the counseled unemployed to those who were born during January, February or March. The other team had to counsel all other unemployed. The experimental design led to a reduction for the first team from 1:250 to about 1:100 in January 2015. The other department had an almost unchanged caseload of

Figure 1: Unemployment in Austria, 2014–2016.



Note: Registered persons, monthly numbers.

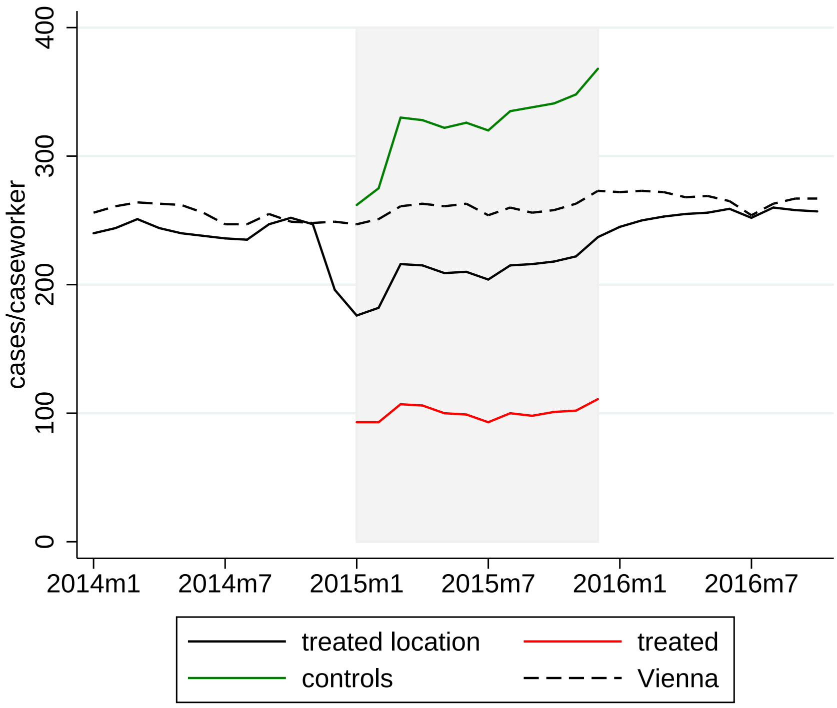
about 1:260 caseworkers per unemployed.

All other tasks of the caseworkers remained unchanged and the reduced caseload should not have resulted in more administrative tasks, for example, time in meetings or more intensive contacts with employers.[1]

The data for our evaluation is from an inflow sample of all unemployment episodes which started in the counseling zone during 2015. Unemployment

[1]Unlike Behncke, Frölich and Lechner (2010), we cannot match caseworkers to the individual unemployed to evaluate caseworker effort.

Figure 2: Unemployed per caseworker, 2014–16.



Note: The diagram shows the caseload per employment officer for the treatment and the control group over time. Treated observations are the unemployed in the treated unemployment office who were born in January, February or March; control observations are the unemployed who were born in April to December.

increased sharply towards the end of 2014. Figure 1 plots the evolution of the registered unemployed, 2014–16. The Figure shows a relatively strong seasonal pattern for the whole of Austria. In Vienna, however, this pattern is less pronounced. This is also evident in the development of the caseloads. The evolution of the caseloads is plotted in Figure 2. The Figure shows that the caseload for the treatment group was, relatively to the control group in the same regional office, lower over the complete duration of the trial. After March, however, due to an overall increase in unemployment, the case load deteriorated for the unemployed who entered the control groups.

There are 1,681 treated and 4,353 control observations. We match all 6,034 observations with their records from the social security administration. This combination of administrative records allows us to obtain labor market outcomes for at least 12 months after their first registration and all their social security history, including information on past wages and employment. For each unemployed person, we obtained detailed (anonymized) information from the Austrian public employment services. These data include individual characteristics, such gender, formal education, health restrictions or care responsibilities which may limit individual labor supply. In addition, we have information on the unemployment episodes such as benefit levels, duration of entitlement or participation in active training programs. The data also contain information on the caseworkers' interventions such as appointments, placement suggestions or assignment to training, but also sanctions for non-compliance with job search requirements.

Summary statistics by treatment status are tabulated in Table 2. The statistics show that the randomization of the unemployed was successful and the two groups differ only in few observable characteristics.[2] Statistically significant differences between the two groups are evident only for the receipt of unemployment assistance (UA), which unemployed may apply for when their eligibility for unemployment benefits is exhausted. On average, 17% of

[2]We provide further evidence on the successful randomization by estimating a linear probability model of being treated on a set of covariates. The results from this estimation are presented in Table 10 in the Appendix. Each coefficient is not statistically significant at conventional error error levels and an F-Test cannot reject that the coefficients are jointly insignificant (p-value: 0.55).

the unemployed in the control group received UA, while among the treated it was 13%.

Table 2: Summary statistics by treatment status.

	Controls	Treated	Difference	p-value
female	0.42	0.41	0.01	0.61
age	36.88	37.40	-0.52	0.15
Marital status				
single	0.45	0.45	0.00	0.84
divorce	0.17	0.16	0.01	0.39
married	0.37	0.37	0.00	0.81
widowed	0.01	0.01	0.00	0.20
Children[a]				
no child	0.88	0.88	0.00	0.95
one child	0.06	0.07	-0.01	0.31
two children	0.04	0.03	0.01	0.26
youngest between 0 and 2 years	0.02	0.02	0.00	0.53
Formal Education				
compulsory	0.40	0.43	-0.02	0.11
apprentice	0.19	0.18	0.01	0.62
secondary	0.04	0.04	0.00	0.46
college	0.19	0.18	0.01	0.40
university	0.18	0.18	0.00	0.75
Nationality				
Austrian	0.62	0.63	0.00	0.77
Nationalized	0.14	0.15	0.00	0.81
Job Market Indicators				
UB receipt	0.60	0.61	0.00	0.75
UA receipt	0.17	0.13	0.03	0.01
job promise	0.04	0.01	0.02	0.00
legal DI status	0.01	0.01	0.00	0.41
health problems	0.06	0.08	-0.01	0.10
Last job more than 1 year ago	0.18	0.17	0.00	0.79
last wage <=€1000	0.18	0.18	0.01	0.65
last wage >€2500	0.13	0.14	-0.01	0.29
days unemployed last two years	153.38	147.81	5.57	0.15
Last Sector				
building	0.07	0.08	-0.02	0.03
service	0.46	0.48	-0.03	0.07
public	0.11	0.10	0.01	0.31
N	3,737	1,363		

Note: [a] Children are only recorded for women (men have a value of zero). * p<0.1; ** p< 0.05, testing against the null of no statistical difference. All variables measured at the start of episode.

3.1 Outcome Indicators

Our main focus is on labor market outcomes. We compare transitions to various exit destinations between control and treated observations. Our main indicator for the effect of changed workload is "stable employment". Stable employment is defined as becoming employed within two weeks of leaving the register and remaining in employment, not necessarily with the same employer, for at least 63 days.[3] Self-employment is, for the purpose of this analysis, a separate destination state. We are also interested in differences in the start of subsidized employment and whether or not the unemployed withdrew from the labor market (OLF). Other outcome indicators are benefits received and sectoral or regional mobility. We also examine difference in durations and post-unemployment outcomes, such as wages in the next jobs.

We tabulate three average outcomes, without controlling for observable characteristics, in Table 3, again for the first three months upon entry. The figures indicate that 31% of treated unemployed left the register within the first three months. In comparison, about 27% of the control observations left the register within three months. On average, the unconditional means suggest that treated persons had greater transition rates to any employment than those in the control group, in particular, in the early months of 2015. During the later months, when unemployment increased overall, the differences were smaller. Based on these numbers, it does not appear that a lower caseload led to more stable employment.

[3] This is the definition used by the Austrian unemployment offices.

Table 3: Unconditional mean outcomes 3 months after entry.

Month	All exits			Any employment			Stable employment		
	C	T	D	C	T	D	C	T	D
1	0.28	0.34	0.06	0.15	0.17	0.01	0.00	0.01	0.00
2	0.30	0.35	0.05	0.18	0.20	0.02	0.00	0.00	-0.00
3	0.29	0.37	0.08	0.16	0.20	0.04	0.01	0.02	0.01
4	0.32	0.42	0.10	0.20	0.24	0.05	0.01	0.02	0.02
5	0.27	0.29	0.03	0.15	0.12	-0.03	0.00	0.02	0.02
6	0.28	0.29	0.01	0.16	0.14	-0.03	0.01	0.00	-0.01
7	0.26	0.28	0.02	0.15	0.15	0.00	0.01	0.01	0.00
8	0.23	0.27	0.05	0.14	0.16	0.02	0.00	0.01	0.01
9	0.22	0.29	0.07	0.12	0.17	0.05	0.01	0.00	-0.01
10	0.25	0.22	-0.03	0.15	0.12	-0.03	0.00	0.00	0.00
11	0.25	0.22	-0.03	0.11	0.12	0.01	0.02	0.00	-0.02
12	0.26	0.34	0.08	0.17	0.13	-0.04	0.00	0.00	0.00
All	0.27	0.31	0.04	0.16	0.16	0.01	0.01	0.01	0.00

Note: Sample consist of unemployed persons who entered the experiment in 2015. C indicates 3,737 observations from the control group, T indicates 1,363 observations from the treated unemployed. D indicates the difference in means. Different treatment ended by the end of 2015.

3.2 Empirical Strategy

In a first step, we estimate the treatment effect of a changed caseload where we compare the outcomes for the treated and untreated in the treated regional office. We exploit the randomization of the experiment to obtain estimates of the average treatment effect for unemployment spell i:

$$y_i = \alpha_0 + \alpha_1 \text{Treatment}_i + X_i'\beta + \delta_t + \epsilon_i, \tag{1}$$

where y_i is an outcome indicator, e.g., the unemployment duration after entering the counseling zone, for the unemployment episode i. The indicator *Treatment* indicates whether an unemployed person was treated or not. The vector X contains observable characteristics which were measured at the time of entry into the trial.

We use gender, age, age squared, indicators for marital status, number of children, age of the youngest child, whether the person was legally disabled, whether there were other health problems, indicators for the highest formal education, indicators for the unemployment's nationality as personal characteristics which are possibly correlated with the chances of finding a job. We also use an indicator for unemployment duration when entering the counseling zone and whether the unemployed had an employer's promise to be hired at a later date or not.[4]

Additional indicators describe the labor market situation. We use indicators for the receipt of unemployment benefits or unemployment assistance, whether the previous employment spell ended more than one year before entry into the experiment, wages in the last job, the number of days unemployed during the last two years, and indicators for the sector the person was working in the last job.[5] δ_t are monthly indicators which control for the entry month into the experiment.

However, the experiment might have influenced the behavior of the counselors of the untreated unemployed, for example, the counselors might have known of the intention to measure the effectiveness of counseling and adjusted their effort. If such spill-overs are present, the estimated effect obtained from estimating equation (1) might be biased. In order to obtain more robust es-

[4]This is typically relevant for persons in seasonal sectors who are often made temporarily redundant (Böheim, 2006).

[5]We also estimate treatment effects from specifications where we do not condition on X, but since the randomization was successful, the results hardly differ.

timates, we estimate a difference-in-differences (DD) specification:

$$\begin{aligned} y_{ilt} = \alpha_0 + \alpha_1 \text{Treatment}_i + \alpha_2 \text{Branch}_\ell \\ + \alpha_3(\text{Treatment}_i * \text{Branch}_\ell) + X_i'\beta + \delta_t + \epsilon_{i\ell t}, \end{aligned} \tag{2}$$

where we use observations from the eleven other Viennese branches of the public unemployment service. Again, *Treatment* indicates whether a person was born January–March or not, *Branch* is an indicator for the branch in which the experiment took place. The vector X contains personal characteristics and δ_t is a set of indicators for the start of the spell.[6]

4 Results

Figure 3 plots the estimated effects of the treatment on leaving for any destination. Results where we do not use covariates are presented in Figure 11. Each estimated effect is obtained from a separate estimation of equation (1) where the dependent variables are binary indicators which are set to one if the spell ended in a certain month and 0 otherwise.[7]

The Figure indicates that exit rates for the treated are significantly greater than for the controls over the whole period. Since randomization was successful, controlling for observable characteristics results only in minor differences between the raw and the estimated treatment effects. Overall, of all per-

[6]Because persons who became unemployed in 2014 and who had long unemployment durations "mature" into the experiment, we cannot estimate a difference-in-difference-in-differences specification.

[7]The estimates use different sample sizes as spells which ended earlier are not used for the estimation of later transition probabilities.

sons who were observed for 12 months after entry into the counseling zone, we estimate that treated persons were 2.46 (95% CI: 1.58; 3.33) percentage points more likely to exit from the registers within one month than control persons. The corresponding estimate from the approach where we do not use covariates is 2.15 (1.57; 2.74). The estimated coefficients are tabulated in Table 4.

Since it might be possible that both control and treated group were affected by the experiment, we estimate difference-in-difference specifications using observations from other regional offices in Vienna. This approach hinges on the argument that observations from the other regional offices provide a counterfactual outcome. In order to provide a first assessment of this approach, we plot the average exit probability within 3 months for four different groups over 2014 and 2015 in Figure 7. We consider four groups, those in the regional office where the experiment was conducted and those in the other offices. In each regional office we further distinguish between those born January, February, and March, and those born in other months. The Figure suggest that there were no difference in outcomes during 2014. In addition, during 2015, we see almost no difference between the untreated in the treated location and the unemployed in the untreated locations. This suggests that any changes in the exit probability that the experiment might have had on the control observations can be controlled for by resorting to these additional observations.

We provide formal evidence for the use of the observations from the untreated offices as counterfactual observations by estimating a difference-in-difference for a placebo treatment. The placebo treatment considers those born in January, February or March in untreated locations as treated and compares their outcomes to those born in the other month in the untreated locations. Since no actual experiment took place, we do not expect to estimate a significant treatment effect.

We interact the monthly indicators with the treatment indicator. If the parallel trend assumption is not violated, we expect to find no statistical difference in how outcomes changed in 2014. We also expect no significant treatment effects for 2015, since these persons were not subject to any invention. The estimated coefficients and their 95% CI are plotted in Figure 7 and we do not estimate any statistically treatment effect.

As a result of the greater probability of becoming employed, the duration in unemployment was greatly reduced. On average, treated unemployed left about 22 days earlier than control observations. This led to fewer financial transfers, on average, each treated person received about €566 less from unemployment insurance or assistance than control persons.

Although persons from the treatment group left unemployment faster than persons in the control group, after twelve months persons from the treatment group were significantly more likely to be unemployed, and less likely to be in OLF, than persons from the control group. This indicates that persons from the treated group are more likely than persons from the control group to rely on the services of the public employment agency rather than to withdraw from the labor market completely.

Apart from the duration on the unemployment register, of concern to policy makers is also the quality of the post-unemployment job. One indicator of the quality of the job is the wage rate. Theoretically, it is also conceivable that the wages are higher for treated persons than for those in the control group. This difference could be caused by better matches facilitated through more effort on the caseworkers' side, however, it is also possible that the more intensively treated workers search more intensively than those in the control group. Ex ante it is not clear, whether these channels should result in on average higher or lower wages. It is entirely possible that treated individuals accept lower wages as a result of a more intensive job search that provides them with a more realistic view of the labor market. Alternatively, more dedicates job search might also result in more job offers with on average higher wages.

Our estimated wage difference between treated and control observations is, after controlling for observable characteristics, positive. This difference is, however, small and amounts only to about €9/month.

While the difference in wages is statistically significant, if small, we do not estimate any differences between treated and control persons when we consider regional or sectoral mobility. Among both groups, about 13% started a job in a different province and, after controlling for observable characteristics, we cannot detect evidence for differences between the groups. Similarly, we see that sectoral mobility, measured at the 2-digit NACE classification, is indistinguishable between the groups. In both groups, we find that about 33% find employment in an economic sector that differs from the one they worked in before their unemployment spell.

4.1 Channels

Table 6 indicates that already in the first month of being exposed to more intensive counselling, the share of unemployed who had a meeting with their caseworker was about 1.5 percentage points greater than that among the control observations. In the third month of exposure to the treatment, this difference was some 11 percentage points. We estimate, however, that in month 12 the difference was statistically different from zero in month 12 at conventional error levels. This probably due to the relatively few observations, there were about 86 treated and 320 control observations in month 12.

The more frequent meetings with their caseworkers led to significantly more job offers the unemployed received from their caseworkers. Table 7 tabulates the share of persons who received at least one job offer from their caseworker by whether or not they were in the treatment or control group. During the first month of exposure to the experiment, about 41.3% of the unemployed in the treatment group received at least one job offer. In comparison, the share of the unemployed who received at least one offer in the control group was 32.2%. We estimate similar differences for months 3, 5, and 9; in the 12th month after entry we do not find a statistical difference between treated and control observations.

Estimated differences tabulated in Table 8 indicate that the treated unemployed also received more training than persons in the control group. The differences, however, were at the beginning of their spells. During the first month, the share of the unemployed who received a training in the treatment group was almost 10 percentage points greater than in the treatment group. Overall, unemployed persons in the treatment group spent about 5 more days in training programs over the period of 12 months. It is remarkable that despite more training, which could result in a lock-in effect, the unemployed in the treatment group became employed quicker than the unemployed in the control group.

Caseworkers may impose sanctions for unemployed persons if they violate rules. Violations are missing appointments, refusal to apply for or to accept suitable jobs, and a general refusal to search for work. Sanctions result in the temporary or permanent stop of benefit payments. Table 9 indicates that treated unemployed were sanctioned more frequently than the unemployed in the control group. However, in both groups sanctions were relatively rare.

5 Financial aspects

Overall, these numbers suggest that more caseworkers indeed led to more intensive counseling of the unemployed. The intensive counseling of the unemployed, in turn, led to faster job starts. The experiment led to a more intensive counseling of 1,681 persons by four additional caseworkers. The costs for the additional caseworkers amounted to €306,560, including overheads or, alternatively, to about €183 per person. The more intensive counseling resulted in more training which cost on average €285 per person. In sum, the additional costs, ignoring any opportunity costs the unemployed might have due to more meetings or training courses, amounted to €468 per person. The gains from the experiment, from the unemployment agency's perspective, were fewer transfers in benefits. Due to the shorter unemployment duration, the agency paid on average €601 per person less in benefits.

We do not know if employers would have filled their vacancy as quickly without the more intensive counseling. If we assume that they would have searched longer to fill the vacancy, we might also consider social security contributions and taxes in this comparison. For example, since the treated unemployed started employment earlier than those in the control group, they paid on average €46 in labor taxes and €123 more in 2015 than those in the control group.[8]

This comparison of direct costs and benefits suggests that the experiment was not only successful in supporting long-term unemployment to find employment, but that it was also cost-effective. It has to be stressed that this is a short-term comparison as it is limited to 2015. Should the more intensive support also have long-term benefits, the net gains of the experiment could be greater. In addition, we cannot quantify any negative effects on the treated (opportunity costs of meetings, et cet.), which we believe to be moderate in comparison, nor any positive effects stemming from employment (e.g., purpose and satisfaction, social circumstances, potential health effects, et cet.).

[8] Earnings below €11,000 p.a. were exempt from labor taxes, however, social security contributions had to be paid for monthly earnings of more than €405.98.

6 Conclusion

The field experiment in an Austrian unemployment office which increased job counseling for randomly selected long-term unemployed suggests that more intensive counseling has positive effects. The provision of more caseworkers resulted in more meetings of the unemployed and their caseworkers, more offers of training programs and of job, and in moderately more sanctions for non-compliance with benefit rules. These channels led to significantly shorter unemployment durations and faster transitions to employment. The experiment suggests that more intensive counseling allows better targeted support of the unemployed and, perhaps, also more search effort.

We do not find any evidence for an impact on the post-unemployment job quality, such as starting wages or tenure. Similarly, we do not find evidence for changed regional or sectoral mobility.

The experiment was limited to registered unemployed. We cannot rule out that other job seekers, who did not register with the unemployment office, were negatively effected by the more intensive job counseling provided to the treated unemployed.

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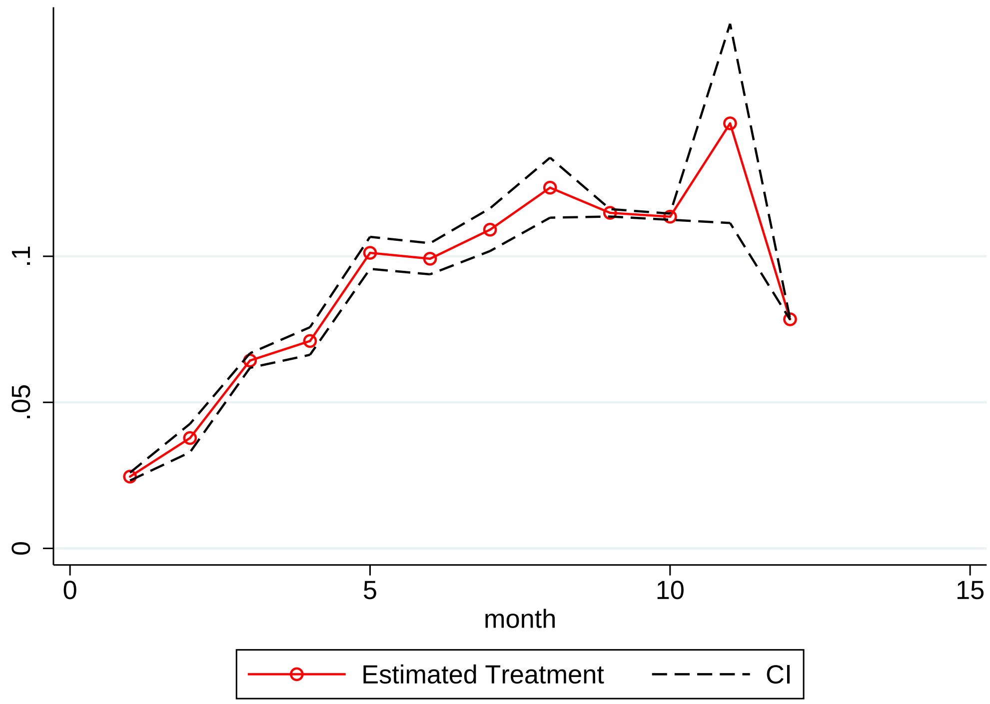
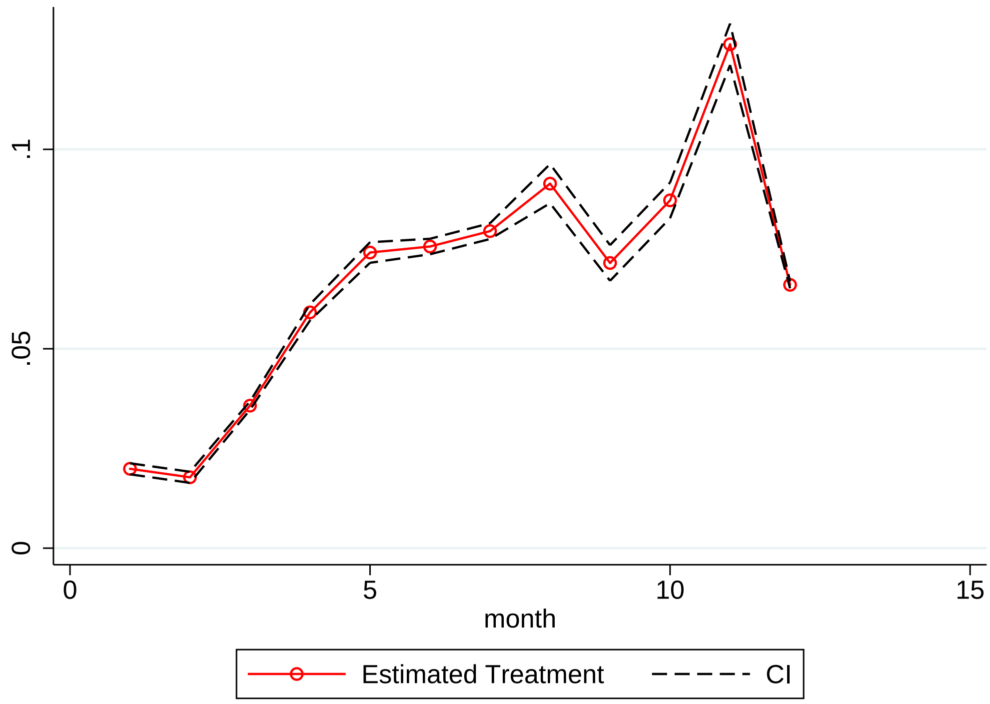
Figure 3: Estimated treatment effect for any exit (percentage points.



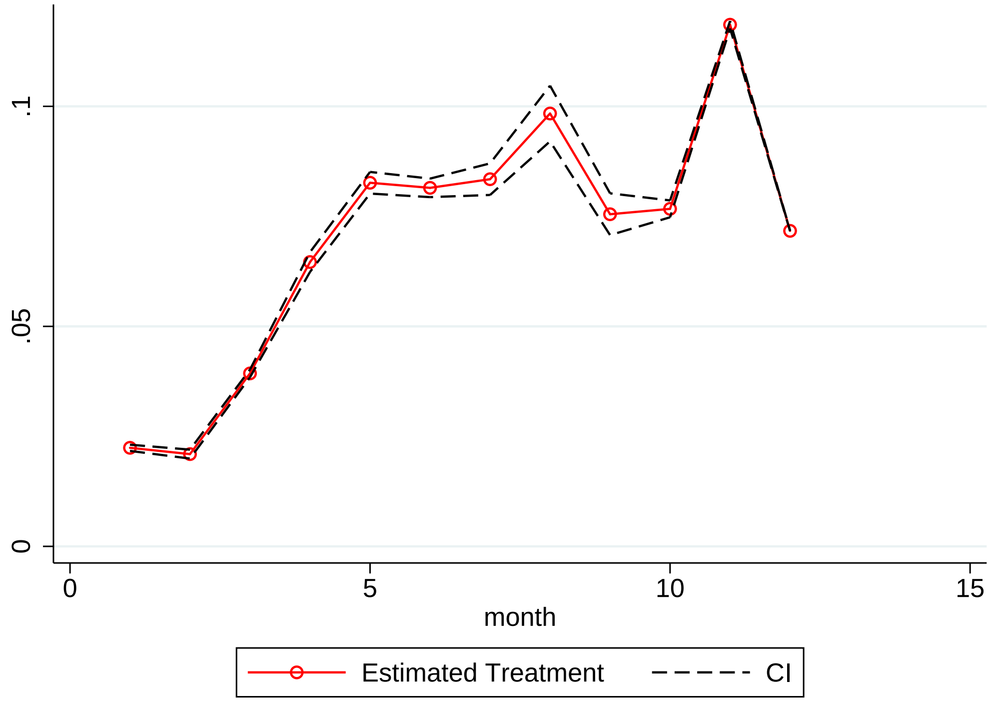
Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for any destination. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.

Figure 4: Estimated treatment effect on exiting for employment (percentage points.



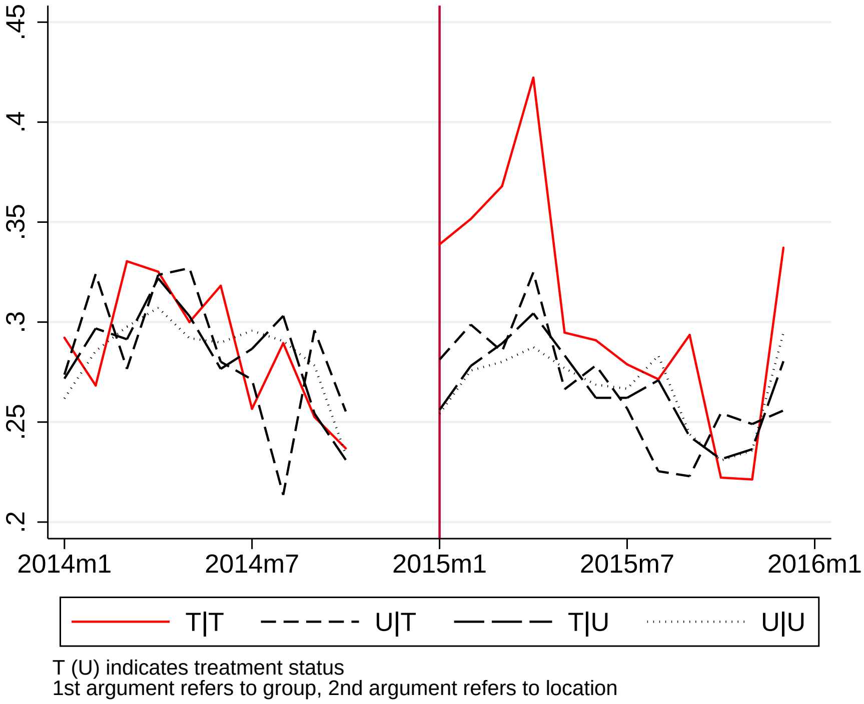
Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for starting unsubsidized employment. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.

Figure 5: Estimated treatment effect on exiting for stable employment (percentage points.



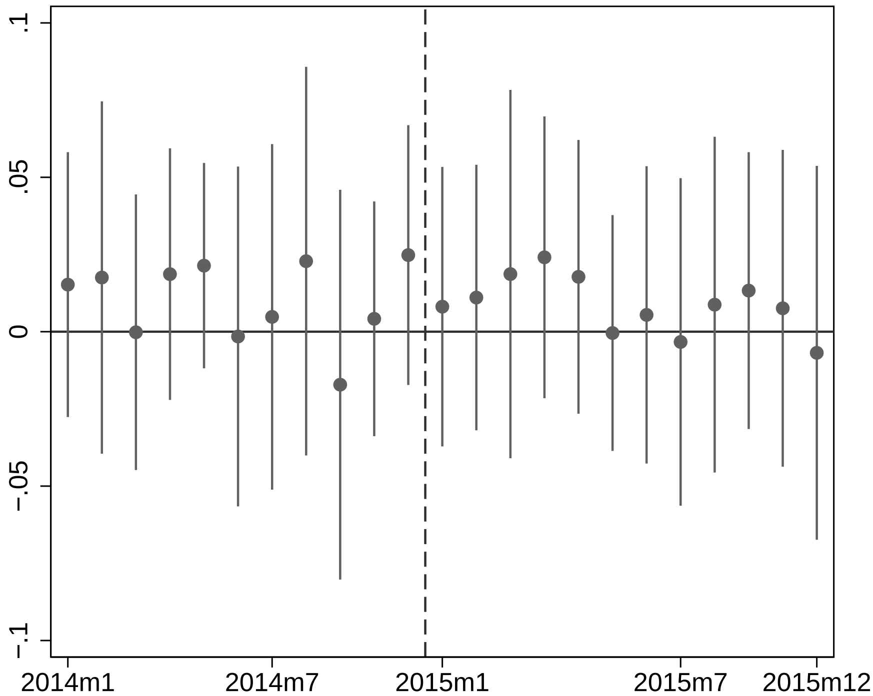
Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for starting stable employment. Stable employment is defined as being continuously employed for two months after exiting unemployment. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.

Figure 6: Mean exit probability within 3 months, by treatment.



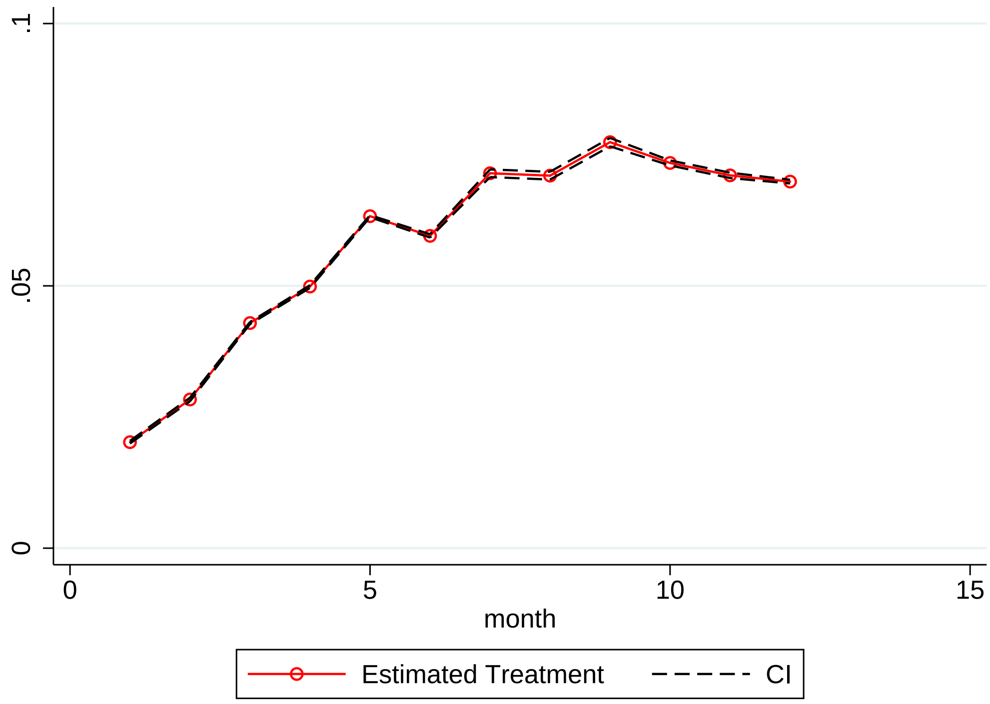
Note: The diagram presents the probability of leaving unemployment within 3 months for four groups. Treated are all persons who were born in Jan–March, persons born in the other months were not treated. T|T are the treated in the treated location, U|T are untreated in the treated location. T|U indicate the treated in the untreated location and U|U indicate the untreated in the untreated location. Note that no observations from November or December 2015 are used as these persons were subject to the experiment when their unemployment durations exceeded 3 months.

Figure 7: Placebo treatment in untreated locations.



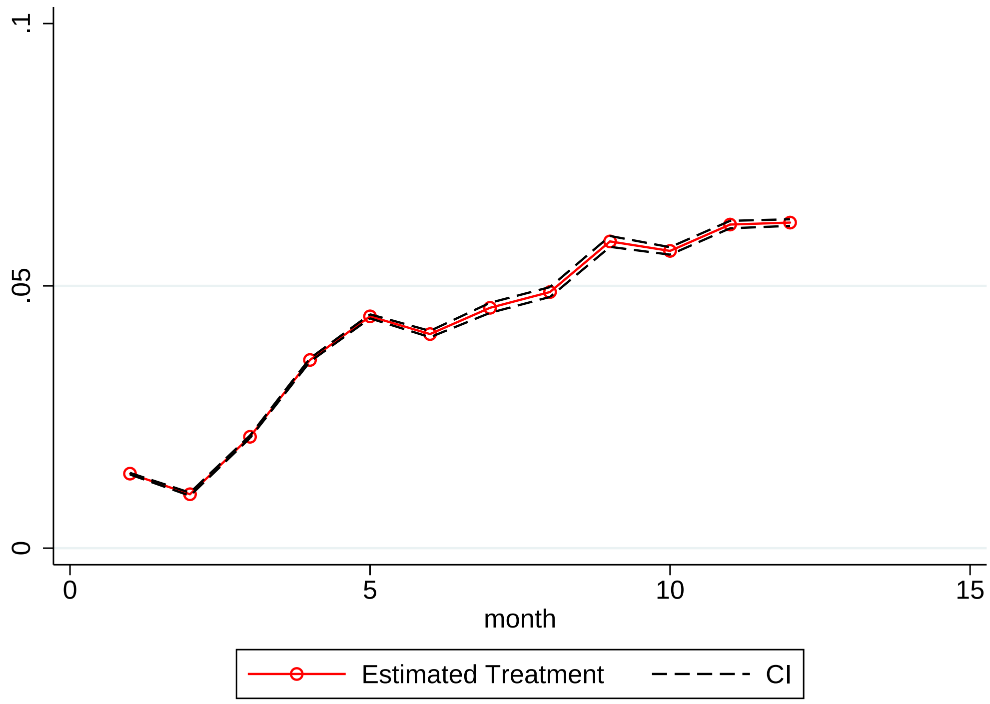
Note: The diagram presents the probability of leaving unemployment within 3 months for a placebo scenario. The scenario considers all persons who were born in Jan–March in untreated locations as treated and persons born in the other months as untreated.

Figure 8: DiD: estimated treatment effect on exiting unemployment (percentage points).



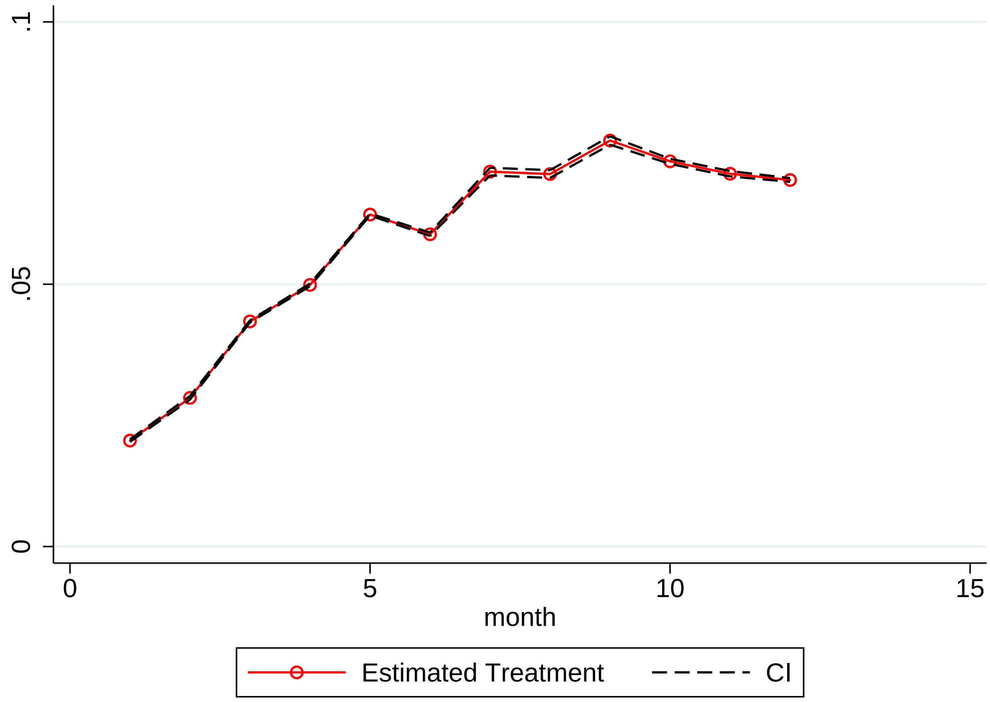
Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points). Each marker represents an estimate from a separate regression. Difference-in-difference regressions using observations from untreated locations in 2015 as counterfactual outcomes. Treated are all persons who were born in Jan–March, persons born in the other months were not treated. Standard errors are clustered on treatment status × location.

Figure 9: DiD: estimated treatment effect on exiting unemployment for employment (percentage points).



Note: The diagram shows the estimated treatment effect of leaving unemployment for any employment (percentage points). Each marker represents an estimate from a separate regression. Difference-in-difference regressions using observations from untreated locations in 2015 as counterfactual outcomes. Treated are all persons who were born in Jan–March, persons born in the other months were not treated. Standard errors are clustered on treatment status $\times$ location.

Figure 10: DiD: estimated treatment effect on exiting unemployment for stable employment (percentage points).



Note: The diagram shows the estimated treatment effect of leaving unemployment for stable employment (percentage points). Stable employment is defined as being continuously employed for two months after exiting unemployment. Each marker represents an estimate from a separate regression. Difference-in-difference regressions using observations from untreated locations in 2015 as counterfactual outcomes. Treated are all persons who were born in Jan–March, persons born in the other months were not treated. Standard errors are clustered on treatment status $\times$ location.

Table 4: Estimated treatment effects, by month after entry.

Month after entry	Treated	Control	Difference	(no controls)
All exits				
1	11.5	10.0	1.9	1.5
3	30.8	27.2	4.5	3.6
6	49.5	44.6	5.6	4.9
9	63.2	55.3	8.7	7.9
12	70.8	64.2	7.4	6.7
Stable Employment				
1	6.1	5.0	1.8	1.2
3	16.2	15.0	2.6	1.2
6	27.7	24.5	4.8	3.1
9	36.5	30.9	7.8	5.6
12	42.6	36.3	8.7	6.3
Unsubsidized stable employment				
1	5.8	4.7	1.7	1.2
3	14.6	13.7	2.1	0.8
6	24.5	21.6	4.5	2.9
9	31.7	26.9	7.1	4.8
12	36.4	30.8	8.1	5.6
OLF				
1	5.4	5.0	0.1	0.3
3	14.6	12.3	1.9	2.4
6	21.7	19.6	1.2	2.1
9	27.4	24.4	1.3	3.0
12	30.3	27.9	0.7	2.3

Note: Outcomes compared at the end of the month after entry to the experiment. Difference is estimated, controlling for observable characteristcs. "(no control)" indicates the differences in mean outcomes without controlling for observable characteristics.

Table 5: Employment status after t months of starting a job.

Months after job start	Treated	Control	Difference	(no controls)
All employment				
6	54.4	54.3	2.3	0.1
12	32.4	32.7	-1	-0.3
Unsubsidized employment				
6	48.6	48.1	3.3	0.5
12	30.1	30.6	-1.1	-0.5
Unemployment				
6	12.2	9.8	2.1	2.4
12	9.5	7.9	0.4	1.6
OLF				
6	33.4	35.9	-4.5	-2.5
12	58.1	59.4	0.5	-1.3

Note: Employment status 6 (12) months after the start of the first post-unemployment spell.

Table 6: Frequency of meetings with caseworkers, by treatment status.

Month	Treated	Control	Difference	(no controls)
1	96.3	95.1	1.5	1.2
3	59.3	48.4	11.4	10.9
6	52.8	40.8	13.4	12.0
9	51.2	39.4	11.6	11.8
12	37.9	41.2	-4.1	-3.3

Note: Share of persons with at least one meeting with their caseworker in the indicated month after entry. For each monthly value, we only consider unemployed who were unemployed during the month. "Difference" is the coefficient from a linear regression, "(no controls)" states the difference in means, without controlling for observable characteristics. Differences are statistically significant at the 10% level in month 1 and the 1% level in months 3, 6, and 9. The difference is not statistically different from zero in month 12 at conventional error levels.

Table 7: Job offers by caseworkers, by treatment status.

Month	Treated	Control	Difference	(no controls)
1	41.3	32.4	9.9	8.9
3	30.4	22.1	9.3	8.4
6	30.2	18.6	12.7	11.6
9	24.2	18.2	6.5	6.0
12	20.3	17.9	3.2	2.4

Note: Share of persons with at least one job offer from their caseworker in the indicated month after entry. For each monthly value, we only consider unemployed who were unemployed during the month. "Difference" is the coefficient from a linear regression, "(no controls)" states the difference in means, without controlling for observable characteristics. Differences are statistically significant at the 1% level in months 1, 3, 6, and 9. The difference is not statistically different from zero in month 12 at conventional error levels.

Table 8: Training programs offered, by treatment status.

Month	Treated	Control	Difference	(no controls)
1	37.3	27.4	9.4	9.9
3	18.4	16.1	1.7	2.4
6	19.6	15.7	2.9	3.9
9	16.7	13.9	2.0	2.9
12	16.7	16.5	-1.1	0.3

Note: Share of persons who received at least one training through their caseworker in the indicated month after entry. For each monthly value, we only consider unemployed who were unemployed during the month. "Difference" is the coefficient from a linear regression, "(no controls)" states the difference in means, without controlling for observable characteristics. The difference is statistically significant at the 1% level in month 1 and at the 5% level in month 6. The differences are not statistically different from zero in months 3, 9, and 12 at conventional error levels.

Table 9: Sanctions, by treatment status.

Month	Treated	Control	Difference	(no controls)
1	2.5	1.9	0.5	0.6
3	1.8	0.8	1.0	1.0
6	1.2	1.4	-0.2	-0.2
9	1.9	1.4	0.8	0.5
12	0	0.6	0.0	-0.6

Note: Share of persons with at least one sanction from their caseworker in the indicated month after entry. For each monthly value, we only consider unemployed who were unemployed during the month. "Difference" is the coefficient from a linear regression, "(no controls)" states the difference in means, without controlling for observable characteristics. Differences are statistically significant at the 1% level in months 1, 3, 6, and 9. The difference is not statistically different from zero in month 12 at conventional error levels.

A Background and Descriptives

Table 10: Balancing regression.

	coefficient	p-value
female	-0.001	[0.881]
age	-0.004	[0.516]
age2	0.000	[0.504]
Marital status		
single	0.023	[0.463]
married	0.014	[0.475]
Children[a]		
no child	0.016	[0.458]
one child	0.055	[0.465]
youngest between 0 and 2 years	-0.013	[0.319]
Formal Education		
apprentice	-0.021	[0.499]
secondary	-0.036	[0.510]
college	-0.030	[0.483]
university	-0.021	[0.488]
Nationality		
EU 15	0.006	[0.391]
EU 2004+	-0.011	[0.528]
former YU	-0.026	[0.416]
Turkey	-0.037	[0.467]
Other	-0.020	[0.523]
Unknown	-0.124	[0.516]
Nationalized	-0.002	[0.430]
Job Market Indicators		
UB receipt	-0.016	[0.494]
UA receipt	-0.062	[0.498]
job promise	-0.155	[0.556]
legal DI status	-0.056	[0.544]
health problems	0.046	[0.440]
Last job more than 1 year ago	-0.002	[0.213]
last wage <=€1000	-0.004	[0.647]
last wage >€2500	0.023	[0.486]
days unemployed last two years	-0.000	[0.188]
Last Sector		
building	0.082	[0.445]
service	0.034	[0.481]
transport	0.015	[0.364]
production	-0.017	[0.460]
tourism	0.028	[0.480]
Constant	0.318	[0.609]
Observations	5,100	

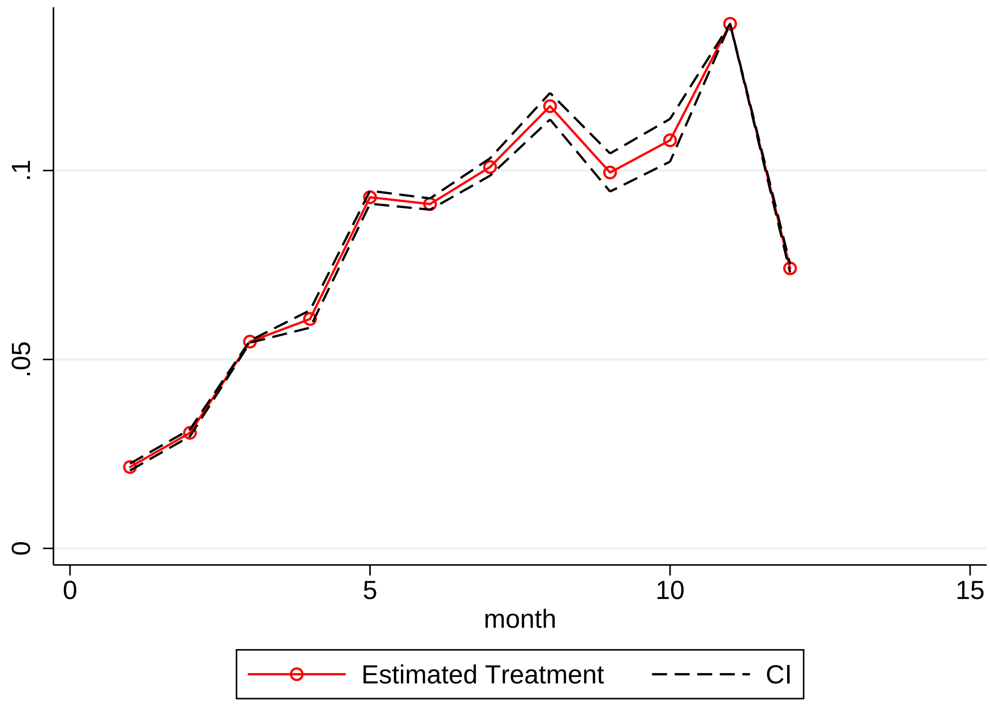
Note: p-values of testing against the null of no statistical significance. All variables measured at the start of episode.

B Outcomes

B.1 Estimation results without covariates

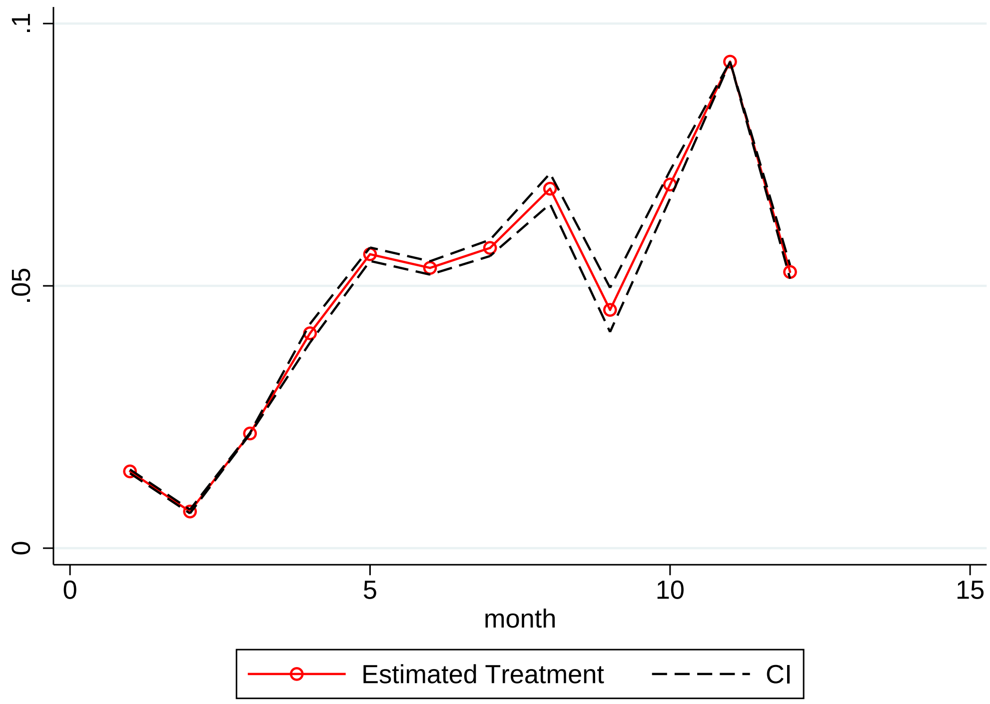
The following Figures replicate Figures 3–5 without using covariates other than the treatment indicators and indicators for the entry month into the experiment.

Figure 11: Estimated treatment effect for any exit (percentage points).



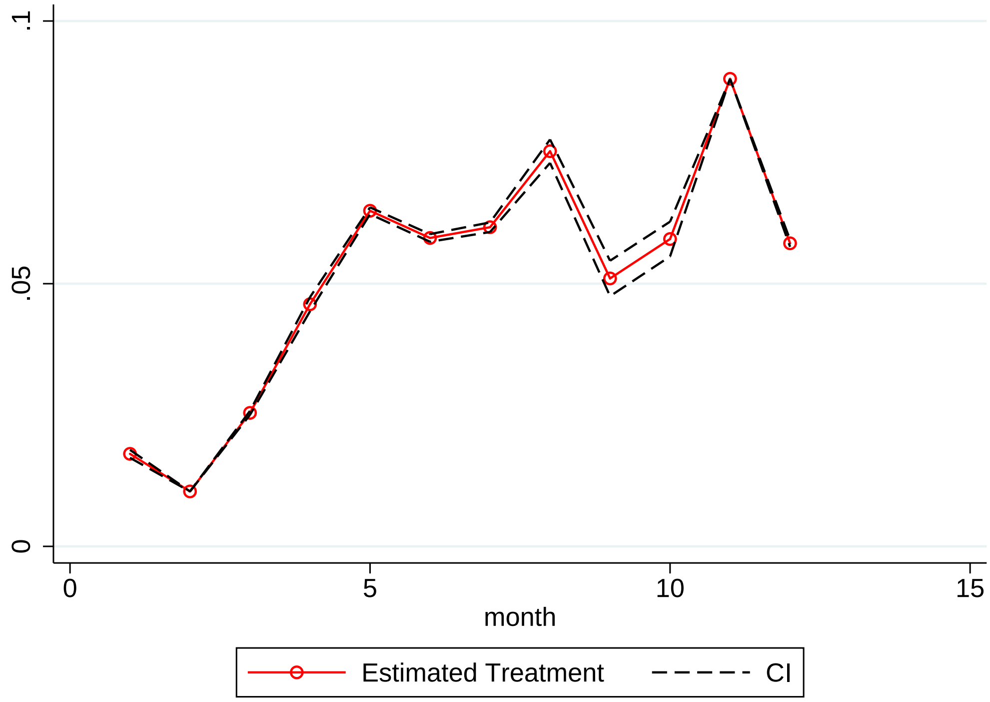
Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for any destination. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015 in the treated regional office. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.

Figure 12: Estimated treatment effect on exiting for employment (percentage points).



Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for starting unsubsidized employment. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015 in the treated regional office. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.

Figure 13: Estimated treatment effect on exiting for stable employment (percentage points.



Note: The diagram shows the estimated treatment effect of leaving unemployment (percentage points) for starting stable employment. Stable employment is defined as being continuously employed for two months after exiting unemployment. Each marker represents an estimate from a separate regression. OLS regressions using only observations who entered the counseling zone in 2015 in the treated regional office. Standard errors are clustered on treatment status. Treated are all persons who were born in Jan–March, persons born in the other months were not treated.